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CONTENTS.

Page

Experiments and Observations on Phosphoric Acid; and on the salts denominated Phosphates. By Mr. John Dalton. 1

Experiments and Observations on the combinations of Carbonic Acid and Ammonia. By Mr. John Dalton. 18

Memoirs of the late Charles White, Esq. F. R. S. &c. with reference to his Professional Life and Writings. By Thomas Henry, *F. R. S.* &c. ... 33

Remarks tending to facilitate the Analysis of Spring and Mineral Waters. By Mr. John Dalton. 52

Account of the Floating Island in Derwent Lake, Keswick. By Mr. Jonathan Otley. 64

On the Refractive Powers of Muriatic Acid and Water, separate and in a state of mixture. By Mr. Henry Creighton. 70

An Essay on the origin of Alphabetical Characters. By the Rev. William Turner, *jun. A. M.* ... 77

Observations on the Rise and Progress of the Cotton Trade, in Great Britain, particularly in Lancashire and the adjoining counties. By John Kennedy, *Esq.* 115

Memoir on a new system of Cog or Toothed Wheels. By James White, *Engineer.* 138

Page

On the Flexibility of all Mineral Substances; and the cause of Creeps and Seats in old Coal Mines. By Mr. John B. Longmire. 161

Account of the Black-Lead Mine in Borrowdale, Cumberland. By Mr. Jonathan Otley. 168

Account of a White Solar Rainbow. By the Rev. R. Smethurst. 176

Remarks, chiefly Agricultural, made during a short excursion into Westmoreland and Cumberland, in August 1815. *By* John Moore, *jun. Esq.* 179

A Tribute to the Memory of the late President of the Literary and Philosophical Society of Manchester. By William Henry, *M. D. F. R. S.* &c. ... 204

An Essay on the Signs of Ideas; or the means of conveying to others the Knowledge of our Ideas. By Edward Carbutt, *M. D.* 241

Account of some Remarkable Facts observed in the deoxidation of Metals, particularly Silver and Copper. By Samuel Lucas, *Esq.* 271

Observations upon the Callous Tumour. By Mr. Kinder Wood. 275

On the Possibility of reconciling the Scriptural and Profane accounts of the Assyrian Monarchy. By the Rev. John Kenrick, *A. M.* 298

A descriptive Account of the several processes which are usually pursued in the Manufacture of the article known in Commerce by the name of Tinplate. By Samuel Parkes, *F. L. S.* &c. 347

Page

The Laws of Statical Equilibrium analytically investigated. By Mr. John Gough. 381

Experiments on the Gas from Coal, chiefly with a view to its practical application. By William Henry, *M. D. F. R. S.* &c. 391

An Inquiry into the Effects produced on Society by the Poor-Laws. By John Kennedy, *Esq.* 430

Memoir on Sulphuric Ether. By Mr. John Dalton. 446

Observations on the Barometer, Thermometer, and Rain, at Manchester, from 1794 *to* 1818 *inclusive. By Mr.* John Dalton. 483

ERRATA.

Page 383. line 23, for SC, read AC.
389. line 4, for "PI draw P", read "P draw PI".
—— line 6, for IK, read IR.

LIST OF BOOKS, &c.

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MEMOIRS

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OF

Manchester.

EXPERIMENTS AND OBSERVATIONS

ON

PHOSPHORIC ACID;

AND ON THE SALTS DENOMINATED PHOSPHATES.

BY JOHN DALTON.

(Read January 22, 1813.)

IN order to obtain pure phosphoric acid, it has generally been recommended to oxidize phosphorus by heated nitric acid. This process is both tedious and expensive; an ounce of phosphorus would require a pound of nitric acid, and the produce would be about 3 ounces of phosphoric acid, the elements of which would cost 7 or 8 shillings at least, and the labour attending it would perhaps cost as much more; so that the pure phosphoric acid could hardly be retailed under five shillings an ounce. This circumstance has hitherto in a great measure precluded the use of the acid in the arts, and has confined the operations of the practical chemist to a smaller scale than would otherwise have been the case.

Though phosphoric acid in a free state is not easily procured, nature furnishes us with a compound of that acid and lime in great plenty. The bones of animals, when calcined in a red heat for some time, consist principally of phosphate of lime. When these calcined bones are pulverized, and treated with sulphuric acid, the phosphoric acid is in part disengaged, and the sulphuric acid unites with the lime and forms sulphate of lime, which being very little soluble, is precipitated and leaves the phosphoric acid in the liquid, still united however with a portion of the lime, from which the sulphuric acid is not able entirely to separate it. The liquor, instead of being pure phosphoric acid in solution with water, is a *superphosphate of lime* in solution with water, or lime containing more phosphoric acid in union with it than is found in the earth of bones. This fact was ascertained in the last century; and the proportion of the elements in this new compound was investigated by Vauquelin.

With regard to the proportion of lime and phosphoric acid in phosphate of lime, authorities do vary a little; but the most modern nearly accord with each other.

	acid		base	
According to Vauquelin	41	+	59	=100 phosphate of lime
Klaproth	45	+	55	———
Davy	48	+	52	———
Berzelius	50.8	+	49.2	———
My experience	49	+	51	———

Hence we may conclude that phosphate of lime is constituted of nearly equal parts of acid and lime. The weights of the respective atoms are, of acid 23, of lime 24, as given at pages 415 and 508 of my chemistry, Vol. 1; and succeeding experience has furnished me with no cause for a variation of these numbers.

Any one may easily satisfy himself on this head; he has only to saturate by degrees a given quantity of lime water with phosphoric acid, then filter, and expose the precipitate to a red heat; when it will be found the phosphate weighs rather less than double that of the lime in solution. A very minute portion of phosphate of lime remains in solution; but it is too minute to deserve notice.

No author that I know of has published an analysis of superphosphate of lime, except

Vauquelin; according to him it consists of

54	acid
46	lime
100	

From this analysis I was led to suspect that superphosphate was in reality a biphosphate, or two atoms of acid to one of lime; this notion would require it to be

66	acid
34	lime
100	

but I soon found that the acid was in greater and the lime in less proportion than this, and that Vauquelin's results were extremely wide of the truth.

The first object that presented itself in this enquiry was to ascertain the component parts of *calcined bones.* Carbonic acid, lime and phosphate of lime I knew were found, but I did not know the exact proportions. By treating the calcined bones when pulverized with an excess of sulphuric acid, and weighing before and after the effervescence, I repeatedly found the loss of weight to be from 2 to 3 per cent., which may therefore be considered the proportion of carbonic acid. 100 grains were dissolved in a minimum of nitric acid, the phosphate of lime was precipitated by lime water, dried and heated to redness; it weighed 86 grains; the nitrate of lime liquor being tested by the hydrometer was found to contain 11 grains of lime more than the lime water added contained; so that it must have acquired that weight of lime from the bones. This conclusion was corroborated by the quantity of nitric acid in the liquor and the quantity of lime which it was calculated was necessary to saturate it. Hence then it appears that calcined bones may be deemed to consist of

3	carbonic acid
11	lime
86	phosphate of lime.
100	

It is remarkable that the carbonic acid is far inferior to the lime not directly engaged with the phosphoric acid, being 3 parts instead of 9. It is probable that part of the carbonic acid is driven off by calcination. It seems too that the lime is $\frac{1}{4}$ of that engaged with the phosphoric acid; so that we may conclude the constitution of bones, as far as regards the earthy parts, is probably 1 atom of carbonate of lime united to 4 atoms of phosphate of lime. The bones I used were those of graminivorous animals indiscriminately.

The nature of the earth of bones being thus known, it is pretty easy to see what must take place when sulphuric acid is added to them. The acid first expels the carbonic acid and seizes that part of the lime which may be considered as free. It then attacks the phosphate of lime, part of which it decomposes, takes the lime, and sets the phosphoric acid at liberty; but this last immediately joins the remaining phosphate of lime and constitutes a superphosphate; at length this superphosphate of lime gets so surrounded with phosphoric acid that the sulphuric is unable to make any further change or decomposition. Then, as the sulphate of lime is nearly insoluble and consequently precipitates, there remains in the liquid the soluble superphosphate of lime, sulphuric acid (if it have been used in excess), and a minute portion of sulphate of lime. This last may be got out by evaporating the liquor to dryness and then dissolving again in a minimum of water; the excess of sulphuric acid may be taken out by adding barytic water or carbonate of barytes as long as any precipitate appears; and then the superphosphate of lime remains alone in solution.

Experience soon taught me that there were various superphosphates of lime, or such that differed much from each other in the proportions of acid. This does not however appear very strikingly from any variation in the *quantities* of the products obtained by using more or less sulphuric acid. I expected that by using more sulphuric acid I should obtain a higher superphosphate; but I could not perceive much difference in the quantity of insoluble residue when much less sulphuric acid was used; only in this case a great part of the phosphate of lime remained undecomposed.

Though I have not yet perfectly satisfied myself as to the requisite proportions, I believe that 10 parts of calcined bones require $8\frac{1}{2}$ parts by weight of concentrated sulphuric acid, to produce the most complete and perfect decomposition.* The acid may be diluted with 6 times its weight of water; the bones should be finely pulverized, and the mixture

* It was not till the present year (1817), that I met with an essay of the ingenious Pelletier in the Journal de Physique for 1785 on the manufacture of Phosphorus, in which he prescribes as the result of experience, 15 lbs. sulphuric acid to 18 lbs. of calcined bones (that is, $8\frac{1}{3}$ to 10). See Vol. 7, page 28.

should digest for 3 or 4 days or more, according to the quantity of the materials, the fineness of the powder, &c. the liquor must then be strained through a linen cloth and treated as has been mentioned.

The superphosphate of lime thus obtained, when evaporated dry, assumes the appearance of butter; when rendered liquid by as little water as possible, it is of the sp. gravity 1.44 nearly; it is excessively acid to the taste, but not so corrosive as most other acids of equal concentration.

This acid salt is easily analysed. If we want to find the quantity of extra-acid, we have only to saturate a given portion of it, by lime water, marking at the same time the quantity of lime water, and previously knowing its saturating power. If we want to find the lime the superphosphate contains, we may saturate it with caustic soda, when phosphate of lime will fall down, and one half of its weight must be taken for the lime.

According to these methods I have not found the superphosphate of lime to contain more than 11 or 12 per cent. of lime nor less than 8 per cent., instead of 46, as given by Vauquelin. In fact the salt instead of being a *biphosphate*, is an *octophosphate* or a *dodeca-*

phosphate. That is, it consists of 8 atoms of acid and 1 of lime, or of 12 atoms of acid and 1 of lime.*

The octophosphate would be constituted of

acid	$88\frac{1}{2}$
lime	$11\frac{1}{2}$
	100

The dodecaphosphate would be constituted of

acid	92
lime	8
	100

Hence then it appears that this superphosphate of lime is almost entirely phosphoric acid; and as it seems that one pound of it in the strongest liquid form may be obtained from $2\frac{1}{2}$ lb. sulphuric acid and 3 lbs. calcined bones, it is obvious that the labour and ex-

* I am not quite satisfied whether the ultimate effect of sulphuric acid is to produce an octophosphate or dodecaphosphate of lime, but incline to the former. I have adopted two modes of investigation; namely, to precipitate the remaining lime by oxalic acid; and to saturate the superphosphate by lime water; the former of these involves the question whether oxalic acid, even in excess, will precipitate the whole of the lime; the latter, which is perhaps preferable, requires accurate measures of *quantity* in the elements of phosphate of lime.

pence in procuring this would not be more than $\frac{1}{10}$ or $\frac{1}{20}$ of what the pure phosphoric acid would cost.

When lime water is gradually added to superphosphate of lime no precipitate ensues till the salt is reduced below a triphosphate. So that a superphosphate consisting of three atoms of acid and one of lime is still a soluble salt. But what is very remarkable I have not been able to procure a biphosphate of lime either in the solid or liquid form; for, when lime water is added to a triphosphate in such proportion as to reduce it to a biphosphate, simple phosphate is precipitated and triphosphate remains in solution.

If a triphosphate solution be evaporated to dryness in a moderate heat and then dissolved in water again, simple phosphate of lime precipitates, and there remains a quadriphosphate in solution.

If instead of lime water we keep adding gradually, recently precipitated and moist phosphate of lime to superphosphate, it is instantly dissolved till at length the solution is reduced to triphosphate.

If a drop of sulphuric acid be added to a triphosphate or quadriphosphate of lime, the liquor becomes turbid and deposits sulphate of lime. I have not pursued the distinctions of the various superphosphates of lime much farther.

I procured a test superphosphate of lime, or one of such strength that 100 measures of it would neutralize as much lime water as 100 measures of my test sulphuric and other acids. Its specific gravity was, 1,115; of course it must contain $11\frac{1}{2}$ per cent. phosphoric acid, capable of being united to any base, and 2 or 3 phosphate of lime.

There is a salt now considerably used both in medicine and chemistry, called *phosphate of soda,* which is prepared from superphosphate of lime and soda. I was desirous to ascertain the proportions of the elements of this salt in the first instance, as it was likely to be an useful agent in the formation of other salts. Into 200 measures of $\frac{1}{2}$ test carb. soda of 1.11 sp. gr. I dropped by degrees 200 measures of the test superphosphate of lime, a brisk effervescence ensued after each portion and due agitation, which effervescence did not terminate till the whole 200 measures had been added. About 7 grains phosphate of lime were thrown down and the liquid possessed all the characters of a solution of phosphate of soda. It contained 23 phosphoric acid and 14 soda; consequently the salt was in reality a *biphosphate of soda,* or was constituted of 2 atoms of acid and 1 of soda.

A saturated solution of the biphosphate in water is of the sp. gr. 1.032. Though constituted of 2 atoms of acid to 1 of base, it is remarkably alkaline by the colour test, and in fact requires the acid to be doubled before it is neutral. Or it may be neutralized by adding any other acid sufficient to engage one half of the soda. Hence the neutral phosphate is a *quadriphosphate* of soda.

If to a solution of biphosphate of soda we add as much more caustic soda as is in the solution, we reduce the salt to a *simple phosphate,* or one atom to one; and this is the salt that should be used in most cases as a chemical agent in the analysis of metallic and other salts. It is more soluble than biphosphate; it crystallizes in very fine needle shaped crystals in a liquid of 1.086 sp. gr.; whereas those of the biphosphate are rhomboidal and the liquid is 1.032 sp. gr.

It appears then that there are at least 3 distinct combinations of phosphoric acid and soda; the phosphate just mentioned; the common salt or biphosphate, and the quadriphosphate or neutral salt.

Phosphate of barytes.

This salt may be formed by mixing muriate of barytes with simple phosphate of soda; the phosphate of barytes falls down and muriate of soda remains in solution in the liquid. The phosphate is constituted of 23 parts acid and 68 barytes.

It consists of	25.2 acid	or	100 acid
	74.8 bar.		297 bar.
	100		

Phosphate of strontites.

This salt may be formed in like manner as the above.

It consists of	33.3 acid	or	100 acid
	66.7 stron.		200 stron.
	100		

Phosphate of magnesia.

This salt may be formed by mixing sulphate of magnesia with phosphate of soda; when duly proportioned the phosphate of magnesia precipitates and neutral sulphate of soda remains in solutiou in the liquid.

The phosphate of magnesia consists of

57.5 acid	or	100 acid
42.5 mag.		70 mag.
100		

Phosphate of alumine.

This salt may be formed by mixing a solution of common alum with phosphate of soda; phosphate of alumine falls down and sulphates of soda and potash remain in solution in the liquor. It consists of

60.5 acid	or	100 acid
39.5 alumine		64 alumine
100		

All these earthy phosphates resemble that of lime, and are soluble in an excess of acid, but whether in the same proportions as lime I have not yet ascertained.

Phosphate of potash.

I have not had time to examine the various compounds of phosphoric acid and potash; but from some trials I have reason to believe they will be found very nearly to agree in character with the phosphates of soda already described.

Phosphate of ammonia.

The phosphoric acid combines with ammonia in circumstances nearly similar to those of soda; but I have not ascertained the precise distinctions of the compounds. If ammonia, or carbonate of ammonia be added to superphosphate of lime by degrees till the liquid indicates neutrality by the colour test, it will be found when 46 acid by weight are combined with 6 of ammonia, or two atoms of acid to one of ammonia, which may be called a *biphosphate* (or if we state the atom of ammonia at 12 it will be a *quadriphosphate*); no doubt a simple phosphate, and perhaps a subphosphate, may exist.

I have not yet examined the triple salts, said to be formed by some of the alkalies and earths with phosphate of ammonia.

General remarks on the phosphates.

It is well known that nitric and muriatic acid dissolve the phosphates, and that they are again precipitated unaltered by lime water and ammonia. I imagined this to be a proper example of solution and not of decomposition; and this notion appears to me to be generally entertained. According to this idea sulphuric acid *decomposes* the *phosphates*, but nitric and other acids only *dissolve* them. I find however this notion not correct. Nitric and muriatic acid *decompose* the phosphates in the same manner as sulphuric acid. Only it happens in their case that the new compound (for instance, nitrate or muriate of lime) is soluble; whereas, sulphate of lime is not soluble, and hence a visible effect of decomposition is exhibited. But there is sufficient proof to be found that when nitric acid acts on phosphate of lime, it decomposes the phosphate, unites with the lime and turns afloat a part of the phosphoric acid which unites to another part of the phosphate of lime and forms with it a superphosphate, soluble in the liquor. So that when phosphate of lime is dissolved in a minimum of nitric acid, the liquor contains nitrate of lime and triphosphate of lime in solution. If phosphate of *magnesia*, instead of lime, is the subject, then even *sulphuric* acid *dissolves* it (in the same way as nitric), and lime water and ammonia precipitate the phosphate of magnesia unchanged. The reason of this apparent difference is, that *sulphate of magnesia*, the new compound, is a soluble salt, and hence no *visible* decomposition is effected.*

October, 1817.

*IT is now nearly 5 years since the above essay was read, and since I have made any train of experiments with a view to investigate the phosphates. In this period other chemists have published results of their labour on this subject, some of which materially differ from the above. In the 85 Vol. of the An. de Chimie, page 328, Thenard determines phosphorous acid (obtained by slow combustion) to be 100 phosphorous and 110 oxygen, and is led to infer phosphoric acid to be 100 phosphorus and 165 oxygen. Berzelius' account of phosphoric acid and phosphate of lime, published in 1811, I have noticed above; but he has since published another (An. de Chimie, 2—1816) in which he assigns 100 phosphorus and 128 oxygen for phosphoric acid and 100 + 77 for phosphorous acid. Dr. Thomson in his Annals of Philos. for April 1816, has given an account of his own labours on this subject; he finds phosphoric acid 100 phosphorus + 123 oxygen, and the phosphates of lime to be a most intricate class of salts, namely, 5 atoms of acid united with 1, 2, 3, 4, 5 and 6 atoms of lime severally; the common earth of bones is supposed to be 5 atoms acid + 4 lime; and the pentaphosphate (5 + 1), is the superphosphate from bones by sulphuric acid. In an article on phosphuretted hydrogen in August of the same year Dr. Thomson finds phosphoric acid to be 100 phosphorus and 133 oxygen. M. Dulong in the Memoires d'Arcueil, Tom. 3, finds 4 acids, namely;

Hypophosphorous	100	Ph.	+ 37.5 Oxy.
Phosphorous	——		+ 75 —
Phosphatic	——		+112.5
Phosphoric	——		+125

After duly considering all that is advanced by these writers, I am still of opinion that phosphoric acid is constituted nearly of 100 phosphorous and 150 oxygen (the atom weighing 23); and for this constitution I am supported by the authorities of Lavoisier and of Davy. In regard to the phosphates too, I have seen no sufficient reason for any alteration.

EXPERIMENTS AND OBSERVATIONS ON THE COMBINATIONS OF CARBONIC ACID AND AMMONIA.

BY JOHN DALTON.

(Read March 19, 1813.)

THE compounds of carbonic acid and ammonia in various shapes have been known to Chemists and Physicians for more than a century; but the nature and constitution of these compounds were comparatively little known till Dr. Black discovered some of the characteristic properties of carbonic acid; and soon after Dr. Priestley discovered several of those of ammonia. These two elements being never exhibited alone, except in the state of elastic fluids, it is not to be expected that any precise information can be obtained respecting them till the æra of pneumatic chemistry, which may be said in some sense to have commenced with the two distinguished chemists above mentioned.

In the progress of science, Lavoisier more particularly pointed out the nature of carbonic acid, shewing it to be a compound of the two elements *carbone* or *charcoal* and *oxygen*, and ascertaining the proportion of these two elements which combine to produce the acid; soon after, Berthollet discovered that ammonia is a compound of the two elements or principles denominated azote and hydrogen, and succeeded in determining the proportion of these elements which combine to form ammonia.

Since these discoveries various authors have attempted to determine the proportions of the elements of the salt formed by the combination of carbonic acid and ammonia, but not with equal success. It is agreed that the salt contains the two elements just mentioned and water; but the precise quantities of each have not been ascertained, as the analyses differ materially one from another. One principal reason for these differences, it will appear from what follows, is, that the salt itself is subject to great variations in its constitution, which the authors have not been sufficiently aware of. It has lately fallen in my way to use this salt in chemical investigations, and I found it expedient to understand the proportions of its elements more accurately, and therefore instituted a course of experiments with this express object. The following are some of the results of my enquiries, stated with as much order as the short time allowed me will permit.

Carbonate of ammonia is manufactured in the large way only by a very few people in the kingdom. The process which all chemical authors recommend is to take pulverized sal ammoniac (muriate of ammonia) and pulverized chalk or carbonate of lime; to mix these intimately and to apply heat to the mixture; the carbonate of ammonia is formed by double decomposition and sublimes into a glass receiver. This process however, I have reason to believe, is not the one adopted by any of the modern manufacturers, as being too expensive. They obtain an impure ammonia or carbonate of ammonia from the distillation of bones or other animal matter, saturate the ammonia by sulphuric acid and crystallize the sulphate of ammonia; then this sulphate is mixed with carbonate of lime and sublimation performed as above. The sulphate of lime remains in the retort, and the carbonate of ammonia sublimes.

When carbonate of ammonia is pulverized and spread upon paper it loses weight very rapidly; but in a few hours it ceases to lose any more weight, and at the same time has lost its pungent smell. A slight ammoniacal smell however still remains. This loss of weight and smell is effected in a few minutes if the temperature be upwards of 100°.

Wishing to know whether the carbonate of ammonia as manufactured by different people, is of the same quality; I procured specimens from three different shops, under the idea that they might probably have been supplied by different people, or by the same people at different times, and consequently different samples. One hundred grains of each of these were pulverized and exposed on paper in a temperature of 45°. The first sample lost weight as under:

	100	Grains
in 4 hours became	80	—
— 8 —	57	—
— 11 —	51½	—
— 13 —	50½	—
— 24 —	50	—

The second sample in like manner gradually lost weight till it became stationary in the same time; but the total loss of weight was only 24 grains. And in the third sample the loss was still less, being only 14 grains. The salts remaining were alike in appearance and almost destitute of smell. A certain portion of each was dissolved in a given quantity of water; they all gave the same specific gravity. They were then analyzed by lime water; they all required the same quantity to saturate them and yielded equal quantities of carbonate of lime; the ammonia was saturated by test sulphuric acid, and they again agreed in the quantities of acid required; so that the residuary salts in all the three samples were of the same quality in every respect.

As the carbonate of ammonia may, from the method in which it is formed, be suspected to contain some traces of sulphate or muriate of ammonia, I was careful to try the last mentioned solutions by the usual tests; but I could scarcely detect any portion of either sulphuric or muriatic acid. The salt may therefore be considered purely a compound of carbonic acid and ammonia, together with some portion of water.

From the above experiment, proving that all the three samples of carbonate of ammonia were ultimately reduced to the same pure salt, it is strongly to be presumed that they were originally the same, and that the differences observed were to be ascribed to the effects of greater or less exposure to the atmosphere by reason of their different ages and other circumstances.—However this may be, it is evident, that no agreement can be expected in the results of analyses made upon the common volatile salt; because it is always procured during its state of transition from one definite form to another, unless, which is very improbable, that the salt loses all its elements in the same proportion.

Properties of carbonate of ammonia.

1. This inodorous salt can scarcely be called volatile in ordinary temperatures (as has been already observed); it may remain for some days exposed to the atmosphere without suffering any remarkable loss of weight. It is however slowly and gradually dissipated. When the heat of a lamp is applied it loses part of its acid, and the rest of the salt sublimes in the state of a *subcarbonate.*

2. It is soluble in water, but much less so than the subcarbonate; the greatest specific gravity of the solution is 1.05; and 100 measures of this contain 12 grains of the salt, or 7.4 of real acid and ammonia. The solution has the alkaline taste, and gives a purple tinge to vegetable reds, the same as pure ammonia; nor does it seem possible to destroy this character of it by the addition of carbonic acid; so that the carbonate of ammonia cannot be made neutral if this is to be the test.

The solution can be made to imbibe its own bulk of carbonic acid gas, the same as water, but no more: so that a *supercarbonate* of ammonia does not seem to exist, at least in a liquid form. A saturated solution of 60° contains a little more salt than one at 32°, from which circumstance the salt may be crystallized in a small degree on cooling. If a solution be attempted above 80 or 100°, the salt is rapidly decomposed; part of its acid escapes and a subcarbonate remains in solution. When the solution is treated with solutions of salts of lime, no precipitation nor cloudiness ensues; this distinguishes the carbonate from the subcarbonate of ammonia. When a due proportion of carbonate of ammonia is added to lime water, the lime is precipitated a carbonate, and just half the test acid is required to saturate the ammonia in the liquor that would have been required to saturate the lime.

Such are some of the more remarkable properties of the carbonate of ammonia.

It is worthy of remark that carbonate of ammonia, which in the dry state is comparatively a *fixed* salt, yet when in solution it is easily decomposed by heat. The two elements, carbonic acid and ammonia, are both volatile and disposed to assume the elastic state, and the latter the more so of the two, being much the lighter, yet it is the *acid* rather than the alkali that escapes. But the reason is pretty obvious. The acid has no affinity for water; but the alkali has a strong affinity for the water and is therefore retained by it.

We now proceed to the *subcarbonate* of ammonia; or the salt with excess of base.

Equal weights of the three samples of subcarbonate mentioned above were dissolved in water and immediately saturated by test acid; the first took 17 grains of acid, the second, 15 grains, and the third, 12 grains. Hence we see that the most volatile of the salts was that which abounded most with ammonia, as indeed might be expected. Other portions of these samples were afterwards analysed in the same manner as the carbonate above. The first had nearly two atoms of ammonia to one of acid; the second had less than two, and the third had rather more than one atom of ammonia for one of acid; so that the salts were evidently mixtures of carbonate and subcarbonate (the last being here understood to signify a compound of 1 atom of acid to 2 atoms of base); it seems therefore most probable that they were all originally subcarbonates in the last sense of the word, but that they had lost less or more of the ammonia according to circumstances pointed out above.

That this idea is correct there seems little doubt from the following experiment.

I took a sample of salt that was about half carbonate and half subcarbonate, and having put it into a tube, I sublimed a portion of it by a gentle heat into a phial. The sublimed portion was immediately dissolved in water, and then analysed in the mode above mentioned. It proved to be a true subcarbonate, or a compound of 1 atom of acid and 2 of base, as I had apprehended. We may be assured that such a compound is formed by nature from the experiments on the combinations of these two elements in an elastic form. Dr. Priestley ascertained that 1 measure of carbonic acid gas absorbed nearly 2 measures of ammoniacal gas (Vol 2. 387, abr.), and when the ammoniacal gas was greatly in excess, nearly 3 measures. Judging from the specific gravities of the two gasses, 1 measure of carbonic acid should take $\frac{4}{5}$ of a measure of ammoniacal gas to form carbonate of ammonia, $\frac{8}{5}$ of a measure to form subcarbonate, and $\frac{12}{5}$ to form *subtricarbonate* (supposing such a compound to exist which we shall shew presently is highly probable). M. Gay Lussac indeed suggests an idea that carbonic acid gas unites, one measure with 2 of ammonia, and in that proportion only, when they are mixed together in a tube, introducing first a portion of one and then one of the the other alternately. But as this very delicate experiment does not appear to have been made by him with extraordinary accuracy, it is not entitled to much credit. (Mem. d'Arcueil, Vol. 2—211). Besides, what is meant by introducing the gasses alternately? Is it to be understood that if 1 measure of carbonic acid is put to 3 measures of ammoniacal gas, and *no more carbonic acid added*, that there will not be just 1 measure of ammoniacal gas left unabsorbed? If so, what becomes of the theory of the combinations of *measures* in this instance? But I shall have occasion to advert to this subject more particularly at some other time.

Another very important reason for considering the combination of 1 atom of acid to 2 of ammonia as a natural one is, that a complete mutual decomposition takes place when a solution of this character is mixed with a solution of neutral salt with base of lime. Thus an exact subcarbonate of ammonia and sulphate of lime being mixed in due proportion and heated to ebullition, sulphate of ammonia is formed and carbonate of lime; and the liquor when tested is found neutral. But if the common subcarbonate with defect of base be used, the decomposition is not complete, sulphate of lime and carbonate of ammonia remain in the liquor, which if tried by the colour test, is found alkaline. In this case, however singular the fact may appear, I find it to be true, that if *ammonia* be added to this *alkaline* liquor, it becomes neutral. For, the ammonia converts the carbonate into subcarbonate, in which state it is capable of acting on the sulphate of lime.

The true subcarbonate above described may be obtained in the solid form in two ways: first by subliming the common subcarbonate and taking the first product before it has had any communication with the air; and second, by taking a saturated warm solution of the common subcarbonate and adding a sufficient quantity of ammonia (to be found by trial) to raise the proportion of ammonia to that of acid, as 12 to 19.4, nearly; then suffering the solution to cool the salt will be precipitated copiously. But for purposes of chemical investigation it will be found sufficient to procure a liquid solution of the sp. gr. 1.086, and such that 100 measures of it are capable of saturating 100 measures of test acid; for then it will contain 6 grains of ammonia and 9.7 of acid per cent.

Properties of the subcarbonate of ammonia.

1. This salt is very volatile even in the ordinary temperature of winter; but it seems that the alkaline part is more so than the acid; it has a pungent smell, like pure ammonia, but not quite so strong. 2. It is soluble in water so as to give a specific gravity of 1.1 in a temperature of 50°, the solution contains about 27 per cent. of the salt, or about 12 acid, 7 ammonia and 8 or 9 water. It is consequently about twice as soluble as the carbonate. It tastes strongly alkaline. A solution of it absorbs carbonic acid plentifully. It gives a copious precipitate with salts of lime. The solution may be kept a long time in a phial without change in the proportion of its elements: it may be heated to ebullition without much loss or decomposition; but by continued ebullition it is distilled and partially decomposed. The proportion of its elements in a saline state as procured above, is 1 atom of acid, 2 of ammonia and 2 of water; or

19.4	acid, by weight =	41	acid
12	ammonia	25+am.	
16	water	34—water, per cent.	
		100	

The test subcarbonate solution, containing 6 ammonia and 9.7 acid per cent. is of the sp. gr. 1.086. That of the carbonate, containing 1.5 ammonia and 4.85 acid per cent. is of the sp. gr. 1.043. It must be observed that this last is *half* equal in acid to my other tests, but has only $\frac{1}{4}$ the power as an alkali, so that 400 measures of it are required to saturate 100 of acid.

Remarks on the subtricarbonate, subtetracarbonate, subpentacarbonate, &c. of ammonia.

The salts hitherto considered have been the *carbonate of ammonia*, that is, the compound of 1 atom of acid and 1 of base; the *subcarbonate,* or perhaps more properly *subbicarbonate,* which is one atom of acid and 2 of base: it remains now to enquire whether the salts with 1 acid and 3, 4, 5, &c. base, have any existence.

That the subtricarbonate exists I think there is little doubt. Dr. Priestley found that one measure of carbonic acid gas absorbed nearly 3 measures of am. gas, in some cases; if it absorb as much as $2\frac{2}{7}$, it would be sufficient to form a subtricarbonate. I find that 400 measures of subcarbonate solution of 1.083 added to 170 measures of ammoniacal solution, .97, will form a solution containing the elements in due proportion to constitute a subtricarbonate; if no chemical union took place the sp. gr. of the mixture ought to be 1.049; but it was in fact 1.058;

this increase cannot be ascribed to any other cause than the chemical union of the acid and the ammonia; for, Sir H. Davy and I have both found in constructing our respective tables of ammoniacal solutions, that no increase of gravity is observed in mixing these solutions with water.

Again, I mixed a solution of muriate of ammonia and carb. of potash (of commerce) together, in such proportion as mutually to saturate each other, so that muriate of potash and subcarbonate of ammonia must have been formed together; the mixture was distilled till all the ammonia was drawn over. The distilled liquid had its elements in the proportion of a subtricarbonate. I next took a solution of subcarbonate and distilled it in like manner; this also afforded a subtricarbonate and the residuary liquid in the retort was as near as possible a subpentacarbonate.

About the same time I distilled a solution of carbonate of ammonia; it afforded a subcarbonate and left in the retort a very weak carbonate of ammonia.

On all these occasions of distillation the receivers were more or less open to the atmosphere; of course there was loss both of acid and alkali; but it appears that in all cases where heat is applied to the liquid solutions, there is more loss of acid than alkali, in all probability for the reason already assigned, namely, the affinity of water for the alkali but not for the acid.

I observed in one experiment made to ascertain whether subcarbonate of ammonia in the dry state attracts carbonic acid, that the common opinion on this head is erroneous. A quantity of subcarbonate of ammonia was exposed over mercury to carbonic acid gas. It seemed to have no effect at first; but in the course of a few hours the gas was all absorbed by the subcarbonate.*

* The common subcarbonate of ammonia of the shops, when taken from the middle of a transparent hard cake, is, I find, tolerably uniform in its composition; it consists of,

59	carb. acid
24.5	ammonia
16.5	water
100	

consequently it may be accounted a compound of 1 atom subcarbonate with 2 atoms of carbonate and water.

The simple carbonate consists of 1 atom acid, 1 ammonia (6), and 1 water: or,

58	acid
18	ammonia
24	water
100	

but it generally contains rather less acid and more alkali.

Erratum. *Page* 23, *line* 22, *for* 7.4 *read* 8.5.

MEMOIRS

OF THE LATE

CHARLES WHITE, ESQ.

F. R. S. &c. &c.

With reference to his Professional Life and Writings.

BY

THOMAS HENRY, F.R.S. &c. &c.

(Read April 2, 1813.)

IT has been customary in the literary and philosophical societies of the continent, on the death of any of their distinguished members, that a surviving associate should commemorate, in what has been termed an *Eloge*, the principal events of his life; the objects of his studies and pursuits; and the claims which he may have derived to the esteem of posterity, from the improvement of any of the useful arts, or the extension of the boundaries of knowledge. This custom, if it had no other merit, is entitled to approbation by its influence in exciting efforts for similar distinction; for, by a happy provision of our nature, to contemplate excellence of any kind, is not only agreeable in itself, but where there is a congeniality of pursuit, furnishes a powerful incitement to active and persevering exertion.

In the early stage of the society which I am now addressing, more than one of our deceased members received this posthumous applause. Several of our first meetings were enlivened by the presence of a most amiable and accomplished man, whom it was impossible to know without feelings of the warmest affection, or to remember, even at this distant period, without lively regret. The talents and virtues of Mr. de Polier called forth from the pen of Dr. Percival, an elegant and merited tribute to his memory, which adorns the first volume of our memoirs. And in the succeeding volume, the character of another distinguished member of the Society, Dr. Bell, has been sketched by a friend whose esteem would alone have been a sufficient passport to honour. The Eulogist himself, (Dr. Currie), has since been cut off, in his useful and honourable career; and though no similar offering of respect has been paid through this Society to his memory, yet he has left a durable monument of his professional skill and knowledge in his "Medical Reports," and of the strength of his genius, and the refinement of his taste, in his masterly Life of the poet Burns.

It has frequently been a subject of regret to me, that a practice so auspiciously begun, should have fallen into almost entire disuse: and the decease of one of our original members, who held, during many years, a high rank in the Society, and was an early and zealous promoter of its interests, has incited me to attempt its revival, in the hope that this essay may be followed by more successful ones of a similar kind.

The late Charles White, Esq. was born in Manchester, on the fourth day of October, 1728. His father, Dr. Thomas White, was an eminent practitioner of the different branches of medicine, especially of surgery and midwifery. Mr. Charles White was educated in this town under the Rev. Mr. Russel, a respectable clergyman, a good scholar, and a polite and well bred gentleman. The pupil made a fair progress in classical learning; and, at a very early age, was taken under his father's professional tuition. In this situation he soon evinced great activity and talent, and began, while almost a boy, to practise in a line, which was then generally confided to men of mature age. This early introduction laid the foundation, and perhaps was a principal cause, of the high character which Mr. White afterwards acquired in that department of the medical profession.

In due time he was sent to attend lectures and hospital practice in London. He had there the good fortune to become the fellow pupil of the afterwards celebrated surgeon and anatomist, John Hunter, by whose older brother, Dr. William Hunter, a name well known, and honoured by every medical man, the first course of lectures, attended by either of the students, was delivered. Ardent in the pursuit of knowledge, the young men contracted a friendship, which ended only with the life of Mr. Hunter. That gentleman, when himself a celebrated lecturer, annually exhibited an aneurismal preparation as the work of his friend Mr. White, of whom he took the opportunity to speak in terms of great respect.

During his residence in London, Mr. White devoted his time, most diligently, to professional objects, scarcely allowing himself to partake of the amusements, so alluring to a young mind, with which the metropolis abounds. He afterwards passed a winter at Edinburgh, at a time when that University was rapidly rising into reputation, as a school of medicine.

Having availed himself, to the utmost extent, of these opportunities of professional improvement, Mr. White joined his father, and soon became an eminent practitioner in his native town.—Nor was his early reputation confined to this town and its immediate vicinity. He was frequently called upon to deliver his opinion, and to perform the higher operations of the art, at considerable distances in the surrounding counties; and was often consulted, by letter, from the most remote parts of the kingdom.

At that time, the advantages of hospital practice were confined, almost exclusively, to London. The infirmaries of Shrewsbury and Liverpool were, I believe, the only provincial ones; at least in this part of England. Such an institution was greatly to be desired in a town, even at that period, very numerously inhabited; the seat of a rising manufacture; and contiguous to populous districts, both in its immediate neighbourhood and in the west riding of Yorkshire, as well as to the mining part of Derbyshire. Beside the Messrs. White, Manchester possessed the advantage of other able and highly respectable surgeons, and of several experienced physicians. The surgeons, especially the young men, aware of the benefit that would accrue to the public from the institution of such a charity, and of the abundant means of professional improvement which it would afford to themselves, exerted all their influence in procuring subscriptions for its establishment and support. Thus was first begun to be erected in the year 1752, a structure, which at that period, was considered as amply sufficient for its objects, but which has since been liberally accommodated to the necessities arising from a vastly increased population.

It has been already mentioned that the benefits of the Manchester Infirmary were likely to be extended to some of the neighbouring counties. The lead mines of Derbyshire, and the coal mines of our own district, of Cheshire, and the confines of Yorkshire, supplied many accidents and cases in which capital operations were required. The town of Manchester (the spring water of which contains much calcareous earth) and the surrounding country, afford very few cases of stone;* but it is remarkable that those parts of Yorkshire, where the water is almost free from calcareous impregnation, are extremely productive of this terrible disease. Most of these cases were sent to the Manchester Infir-

* Chemical analysis has lately shewn that lime is not an *essential* constituent part of the human calculus.

mary; and Mr. White soon established an eminent reputation as a lithotomist.

During the early part of the last century, the art of midwifery was not so generally exercised by male practitioners as at present; and the female midwives were, too often, extremely ignorant, and were under the domi nion of inveterate prejudices. The injurious effects of these deficiencies were more felt in the subsequent treatment of puerperal women, than during the time of labour. In difficult parturition, the male accoucheur was consulted; but, when the child was born, the management of the mother reverted either to the female midwife, or to a person still more objectionable than herself, under the designation of a nurse. The lying-in woman was not allowed to rise from her bed before the ninth day; the curtains were drawn around her; the doors and windows were closed; every avenue to the external air effectually stopped; and a large fire was kept up in the room. She was loaded with blankets, and crammed with caudle, cordials, and broth. Of this absurd treatment, puerperal and miliary fevers were the frequent effects. But if the mothers of that generation died in childbed, the event was attributed to the dangerous process of nature, through which they had passed; they had been treated, (as I have heard it alleged) with every precaution against cold; and nothing had been omitted which the midwife had directed.

Under these circumstances, Mr. White commenced practice, and he directly set himself in opposition to usages so fatal in their consequences. The evil was, indeed, of great amount, and the mortality truly alarming. To overcome the influence of prejudice and long established custom, required a manly spirit and steady perseverance, united with real professional ability and the possession of the public confidence. With these requisites, Mr. White finally accomplished his object. His patients, instead of being confined to their beds for above a week, were permitted to rise on the second day; the room was well ventilated and kept cool; and no cordials or vinous liquors were allowed, except when absolutely necessary, and under proper restrictions. The good effects of these changes were so evident, as to carry conviction wherever they were introduced. The miliary fever almost entirely disappeared, and the puerperal soon became comparatively, of rare occurrence.

Of the febrile diseases which have been just noticed, the miliary fever was not confined to lying-in women, nor to the female sex. It became contagious; and, when thus propagated, both men and women fell victims to it. The change of treatment not only checked the progress of the infection, but happily promoted the cure of the malady, where it had taken place; thus proving that the fever, or at least its attendant eruption, had been the creature of hot unventilated rooms, foul air, profuse sweating, and over stimulating medicines.

In the year 1772, Mr. White presented to the Royal Society of London "an account of the topical application of the spunge in the stoppage of hæmorrhages."

The use of spunge, as Mr. White acknowledges, was not unknown to the ancients; but its application had been confined to the stopping of partial hæmorrhages from the smaller vessels. The difficulty, sometimes, of tying the vessels, when they were numerous; the want of permanency in the ligature, which occasionally occurred, especially when the parts were very soft and tender; and the pain during, and subsequent to, the operation, induced Mr. White to try the application of spunge in the greater operations, especially where the arteries were large and numerous. The experiment succeeded to his expectations. In a subsequent publication he says, "I made use of the spunge for the stoppage of hæmorrhages in all cases, indiscriminately, for near three years, in which time there were nineteen operations of the larger extremities, six of which were of the thigh, and in most of the principal operations of surgery, as lithotomy, castration, bubonocele, the trepan, schirrous and cancerous breasts, besides many accidental wounds, violent hæmorrhages from the extraction of teeth, where it was peculiarly useful, and after the application of leeches. For four or five hours after an operation, constant attention was necessary, but after that period, the vessels were found perfectly secure, even more so, than when the needle and ligature had been employed." This application was, in a great measure, superseded by Mr. Bromfield's invention of the *tenaculum*, which, by laying hold of the vessels, and drawing them out from the surrounding substances, facilitates the tying of them, and is generally attended with little or no pain. But still, cases not unfrequently occur, where the spunge is of important advantage. Mr. White himself candidly and promptly adopted, and has bestowed merited praise on Mr. Bromfield's invention.

In the 51st Volume of the Philosophical Transactions, Mr. White has given "an account of a remarkable operation on a broken arm." In this case the fracture had been treated, at first, by a country bone-setter; but after a lapse of several months, there were no signs of a callus being formed; and the patient, in other respects a healthy boy, was brought to the Manchester Infirmary. Mr. White advised a longitudinal incision to be made; the fractured ends of the bone to be carefully brought out; the extremities of both to be cut off, and then returned to their proper situation. The union, which had, probably, been prevented by the intervention of some extraneous substance, soon took place, and the boy was discharged perfectly cured.

The 56th Volume of the same Transactions contains the relation by Mr. White, of a complete luxation of the thigh bone of an adult person, by external violence; a case, at that time, supposed by many surgeons to have seldom, if ever, happened: And in the 59th Volume is inserted a communication, from Mr. White, giving an account of an instance in which the upper head of the Os Humeri was sawed off, and though a large portion of the bone afterwards exfoliated, yet the entire motion of the arm was preserved.

In the year 1770, Mr. White published a Volume containing, not only the papers already noticed, that on the spunge excepted, but several other very interesting "Cases in Surgery."

By this work his reputation was widely extended, and he was deservedly considered as at the head of his profession in the north of England. Some of the cases having occurred so early as the year 1748, shew the judgment with which he decided, and the skill with which he operated, when a very young practitioner.

His "Treatise on the management of pregnant and lying-in women" made its appearance early in 1773, and is dedicated to Dr. Hunter. In this treatise he has urged the opinions above alluded to, in favour of the cool regimen and free ventilation, with great force and success; has elucidated them by several cases; and has displayed extensive research, united with much original and valuable information. The knowledge of Mr. White's doctrines was thus generally diffused. Perhaps indeed few medical books have been productive of more important reform in practice, or of more comfort and safety to the subjects, for whose benefit it was intended. Nature was restored to the free exercise of her operations, and officious ignorance was prevented from converting into fatal disease, what was benevolently and wisely designed to be a process scarcely ever attended with danger.

The year 1789 produced Mr. White's "Inquiry into the nature and cure of the swelling in one or both of the lower extremities, which sometimes happens to lying-in women." To this occurrence Mr. White, whatever may be thought of the soundness of the opinions which he has advanced, or the propriety of the treatment which he has recommended, had, I believe, the merit of first drawing the attention of practitioners.

About the year 1761, Mr. White was elected a Fellow of the Royal Society, and he afterwards became a member of several other societies, founded for the promotion of medical science.

The Literary and Philosophical Society of Manchester, the origin of which had taken place some years before, as a private meeting held weekly at the houses of several gentlemen, was first organized in the winter of 1781, and regular officers appointed. Mr. White was elected one of the Vice Presidents; and the active part which he took in promoting the views of the institution, is evinced by the articles supplied by him to our printed Memoirs. Of these the following is a list:

VOL. I.

1. "On the Regeneration of Animal Substances."

2. "On the Natural History of the Cow, so far as is relative to her giving Milk, particularly for the use of man."

VOL. II.

"Observations on a Thigh Bone of uncommon length.*"

VOL. V.

"An account of three different kinds of Trees which are likely to prove a great acquisition to this Kingdom, both in point of profit, and as Trees for ornament and shade."

Besides the papers here enumerated, Mr. White presented to the society, "an Essay on the Gradation in Man and different animals." But this, on account of its extent, he withdrew, and printed separately in a quarto volume.

Mr. White's extensive professional engagements afforded him little leisure for the pursuit of general science or literature. He was fond, however, of botany, and indulged his taste by forming a collection of forest trees,

* Supposed to be the femur of the mammoth.

at his country villa, at Sale in Cheshire. These are regularly and beautifully arranged, and highly merit the attention of every lover of that science. He possessed too, a valuable collection of anatomical preparations, which, founded originally by himself, had been greatly enlarged by his son, the late Dr. Thomas White, during the course of his studies in London and Edinburgh. This Museum he presented, some years since, to the Lying-in Hospital; a charity, of which he may be regarded as the founder, and of which he was always a most zealous advocate.

In the year 1783, it was proposed, in this society, to form an institution, upon a plan, which has, since, been successfully and most advantageously established in London, for the delivery of lectures on several branches of literature and the arts. Mr. White, aided by his son, Dr. White, undertook the anatomical department; and filled the situation in a manner highly creditable to him. He had qualifications, indeed, which, in a public school of medicine, would have raised him to distinguished eminence as a teacher,—great correctness and clearness in his knowledge, and perspicuity in conveying it; an agreeable voice, and a distinct articulation; and a countenance enlivened by the interest which he felt in his subject, and with which he succeeded in inspiring his hearers.

For many years Mr. White was annually continued one of the Vice Presidents of this society, and exhibited unwearied zeal for its interests. But the advances of old age, and some important legal rights, which he found himself called upon to support in behalf of a near relative, left under his protection, necessarily diminished the frequency of his attendance at our meetings.

From the first opening of the Infirmary, Mr. White attended to the discharge of his duties, as surgeon to the charity, with the greatest assiduity and practical success. But, some years ago, the rapid increase in the population of the town and neighbourhood demanded, in the opinion of the trustees, an increased number of physicians and surgeons; and this measure not being satisfactory to the gentlemen, who then officiated as surgeons to the charity, they resigned their situations.

In the year 1803, our respectable associate was attacked with the Opthalmia, which was, at that time, epidemic. He suffered extreme and long continued pain of his left eye, and though the inflammation was subdued, the sight of that eye was permanently injured. He still, however, continued to practise, and even to perform operations; but in the last year (1812), the right eye became diseased, the total loss of vision ensued, and his general health rapidly declined. From the month of October, to the day of his death, he was confined, wholly, to his bed; and though at times, his mental faculties were impaired, yet he enjoyed intervals of unclouded and even vigorous judgment. At length on the 20th day of February last, when in the 85th year of his age, he finished a long life of unremitting exertion, and of great and extensive usefulness.

Mr. White was admirably fitted, both by his bodily constitution and by the qualities of his mind, for the successful exercise of the profession in which he was engaged. Even in advanced age, he was capable of performing, without fatigue, very long journies on horseback, and of bearing, without injury to his health, exposure to the most inclement weather. He required little sleep, and was, during the greater part of his life, an early riser. His mind was never unemployed; and when not actually engaged in attendance on his patients, he was generally occupied in some professional research. He had a complete disdain for every thing like empirical concealment; and, when he had any capital operation to perform at the Infirmary, he invited most of the respectable surgeons of this and the neighbouring towns to be the spectators of his practice. As an operator, he was steady and collected, and was prepared both by the natural firmness of his mind, and by the accuracy of his anatomical knowledge, to encounter, without dismay, those unforeseen difficulties, that sometimes occur even to the most sagacious surgeons.

Of his manner in the apartments of the sick, it would not be easy to speak too favourably: It was singularly calculated to inspire confidence; for he evinced, by the attention which he gave to the case, and by the promptitude and firmness with which he decided, that he was intent on employing all the resources of his skill and knowledge. In the common intercourse of life, he was an agreeable and instructive companion; he abounded with entertaining anecdotes; and having frequently mixed with the higher circles of society, his manners and his feelings were those of a well bred gentleman. Even for those branches of knowledge which he had not cultivated, he entertained respect; and his mind was awake to all those sources of casual information, which, to one extensively engaged in medical or surgical practice, must often supply the want of opportunities of retired and undisturbed study.

In Mr. White we have to lament one of the last of that school of surgeons,* among whom he was so much distinguished. It has passed away, but has left many records of the talents and attainments of those who composed it; and it has been succeeded by another containing many individuals, who unite to the knowledge which has been bequeathed to them, an ardent zeal for the improvement of their art, and who, I trust, will still farther extend the sphere of its usefulness.

* Consisting of Hunter, Pott, Bromfield, Earle, &c. &c.

REMARKS,

TENDING TO FACILITATE THE ANALYSIS OF SPRING AND MINERAL WATERS.

BY JOHN DALTON.

(Read April 1, 1814.)

IT cannot but fall under the observation of every one that the health and comfort of families, and the conveniences of domestic life are materially affected by the supply of that most necessary article, water. The quality of water is undoubtedly of great importance in the arts of brewing, baking, and various others connected with the preparation of food; as also in the washing and bleaching of linen and cotton, and in other operations where cleanliness is the object in view. Many of the manufactories are materially interested likewise in the qualities of water, and in the methods of rendering it subservient to their exigences when it happens to be presented to them in an obnoxious form. On all these accounts I thought it might be of some service to offer a few remarks on the subject, which perhaps may benefit those who have not made the science of chemistry a peculiar object of study.

Most writers consider the analysis of waters as a problem requiring great skill and acquaintance with chemistry; but the modern improvements in that science have rendered it much less so than formerly. It is true that the variety of elements sometimes found in water, and the extremely small quantities of them, are discouraging circumstances when the object of analysis is to ascertain both the *kind* and *quantity* of these foreign elements. They may both however be investigated without much labour, when proper means are used; and perhaps a little practice may render a person qualified to undertake the task, who is no great adept in chemical science in general.

Most spring water that is obtained by sinking some depth into the earth, contains lime held in solution by some one or more acids, particularly the carbonic and sulphuric acids.

It is to these salts, the carbonate and sulphate of lime principally, that spring water owes its quality of *hardness*, as it is called; a very singular and astonishing quality, when it is considered as produced by so extremely small a portion of the earthy salt. The other earthy salts, or those of magnesia, barytes and alumine produce the same effect nearly, but they are rarely met with, compared with those of lime.

When any earthy salt is dissolved in pure distilled or rain water, it increases the specific gravity of the water; but in the instance of spring water in general this test is rendered of little use, because the increase of sp. gr. is so small as almost to elude the nicest instrument that can be made. I have however an instrument, made by an artist in this town, which is nothing more than the common glass hydrometer, but with an unusually fine small stem, that shews the superior gravity of spring water. It cannot indeed be brought in competition with other methods for ascertaining the relative hardness of spring water, but it is a most useful instrument in other departments of chemical investigation, particularly in determining minute portions of residual salt after precipitations.* It may well

* The scale of the hydrometer is $1\frac{1}{2}$ inch long and it is divided into 25°, each degree corresponding nearly to .0004; the difference between distilled water and common spring water is usually about 1° on the instrument; and that between distilled or rain water and the strongest lime water is 4°.

be conceived that the sp. gravity cannot constitute a test of the hardness of water, when we find that *one grain* of earthy salt dissolved in 2000 grains of pure water converts it into the hardest spring water that is commonly found.

We shall now proceed to notice some of the most useful tests in the analysis of waters.

1. *Soap test.* When a piece of soap is agitated in distilled or pure rain water a part of it is dissolved, producing a milky liquid, which continues for many days unaltered. But when soap is agitated with hard spring water, the milkiness produced almost instantly degenerates into a curdy substance, which rises to the surface and leaves the liquid below nearly transparent. This curdy substance is understood to be the earth of the salt combined with the oil of the soap. It has a glutinous, unpleasant feel when rubbed upon the hands, and soils glass and other vessels so as to require hard pressure of a cloth to remove it. Though this test sufficiently distinguishes hard water from soft or pure water, it is not equal to form an accurate comparison of the hardness of two kinds of water.

2. *Lime water test.* Most spring water fresh from the well will exhibit milkiness by

lime water; this is usually occasioned by the water holding supercarbonate of lime in solution; the addition of lime water reduces the supercarbonate to carbonate, which is insoluble and falls down in the state of a white granular powder. When a spring contains nothing but supercarbonate of lime, which is the case with the water of an excellent pump in this neighbourhood,* lime water is the only test wanted to ascertain the proportion of salt in it. Let a given portion of the spring water be saturated by lime water, adding it as long as milkiness ensues; the carbonate of lime is precipitated and may be determined by the usual means. I find it however rather preferable to add a small excess of lime water to secure the precipitation of the whole acid; when the salt has subsided, the clear liquid may be poured off and tested by an acid, and the salt may be dissolved by test muriatic or nitric acids. Thus the whole quantity of lime will be found, from which, deducting that added in lime water, there will remain the lime in the spring water originally combined with carbonic acid. In this way I find the supercarbonate of lime in 5 oz. of the water abovementioned, to consist of

* George-street.

.48	lime
.77	carb. acid
1.25	

being about 1 grain of salt in 2000 of water. This kind of water is hard and curdles soap; but it is much softened by boiling, and deposits the incrustation so often found in kettles, &c. If water contains sulphate of lime along with supercarbonate, the same treatment may still be adopted as far as respects the supercarbonate. I have recently found, with some surprise, that the supercarbonate of lime as I call it, existing in waters, or made artificially, is rather an *alkaline* than *acid* compound.

3. *Acetate and nitrate of lead tests.* These salts are easily obtained in great purity, and are excellent tests for carbonic and sulphuric acid, which they precipitate immediately in combination with the lead. If the precipitate be treated with nitric acid, the carbonate of lead is instantly dissolved, and the sulphate of lead (if present) remains undissolved and may be collected and dried, from which the quantity of sulphuric acid may be determined.

4. *Nitrate and muriate of barytes tests.* When the object is to ascertain the presence of

sulphuric acid, either free or combined, these are the best tests. The sulphate of barytes is perhaps the most insoluble salt known. Even rain water collected from slated houses, though softer than spring or river water, exhibits by these tests 1 grain of sulphuric acid in 20 or 30 thousand grains.

5. *Oxalic acid test.* When the object is to obtain the lime, either free or combined, in any water, this is the best test. It may be proper to add a little ammonia in some cases of combined lime. The oxalate of lime slowly precipitates in the state of an insoluble salt. The quantity of lime may be ascertained, either by collecting the precipitate, or by carefully and gradually adding the due quantity of acid and no more, when the strength of the acid has been previously ascertained.

6. *Nitrates of silver and mercury tests.* These are tests of muriatic acid or of muriates; the muriates of silver and mercury are formed, both insoluble salts. It does not often happen that spring waters contain notable proportions of the muriatic acid either free or combined.

7. *Sulphuretted hydrogen water and hydro-sulphurets.* These are excellent tests for lead, mercury and several metals, giving peculiar insoluble precipitates of the sulphurets of

those metals. One grain of lead precipitated by sulphuretted hydrogen, would be sufficient to give a great many gallons of water a dark brown tinge. When sulphuretted hydrogen is found in mineral waters, as those of Harrogate, it may be known by the smell; but solutions of lead are much superior tests, giving a black or brown tinge to such waters immediately.

8. *Tincture of galls and prussiates of potash and lime tests.* These are proper for the detection of iron, the former giving a black precipitate and the latter a blue one; but a portion of the solution of oxymuriate of lime requires to be added previously to the water, if it contains the green oxide of iron in solution, in order to convert it to the red oxide.

There are many other tests than those I have enumerated, but they are more than can usually be wanted in the analysis of ordinary springs. My object is not to give a catalogue of tests, but to shew in what manner their application may be improved, and reduced to a system intelligible to moderate proficients.

The improvements I would propose in the use of tests are, that the exact quantities of the ingredients in each test should be previously ascertained and marked on the label of the bottle; this might easily be done in most of them in the present state of chemical science. We should then drop in certain known quantities of each from a dropping tube graduated into grains, till the required effect was produced; then from the quantity of the test required, the quantity of saline matter in the water might be determined without the trouble of collecting the precipitate; or if this was done the one method would be a check upon the other.

I shall now close this imperfect sketch by a few observations and experiments which I have noticed in the course of the present week relative to the subject before us.

I essayed the water supplied by the Manchester water-works, and found it nearly as I expected; river water is most commonly softer than spring water, and harder than rain water. This is the case with the water in question. It contains a very little sulphate of lime and some carbonate; but only one half of the earthy matter that the above mentioned pump water contains. It curdles a little with soap, but gives no precipitate with lime water. It contains about 1 grain of earthy salts in 4000 of water.

When spring water contains supercarbonate of lime, boiling it precipitates the greater part of the carbonate and expels the excess of acid. Hence the *furring* of pans and tea-kettles with this kind water. By boiling the water is of course rendered much softer than before. It may then be used for washing, scarcely curdling soap; but it still contains about $\frac{1}{3}$ of the earthy salt and gives milkiness with acetate of lead. If a water contain only sulphate of lime, boiling does not, I apprehend, soften it at all.

When spring water is used by manufacturers for washing, &c. it is advantageous to have it some time exposed to the atmosphere, in a reservoir with a large surface. This exposition suffers the carbonic acid in part to escape, and the carbonate of lime to precipitate; and in some degree supersedes the necessity of boiling the water. The more any spring is drawn from, the softer the water becomes, it should seem. I have this morning examined a spring which yields many thousand gallons every day. The water is comparatively soft; it does not curdle scarcely at all with soap: it is very nearly as soft as the before mentioned pump water boiled. The hardness in it arises from a little sulphate of lime and a little carbonate.

One of the most striking facts I have observed is, that all spring water containing carbonate or supercarbonate of lime, is essentially *limy* or alkaline by the colour tests. And this alkalinity is not destroyed till some more powerful acid, such as the sulphuric or muriatic is added, sufficient to saturate the whole of the lime. Indeed these acids may be considered as sufficient for tests of the quantity of lime in such waters, and nothing more is required than to mark the quantity of acid necessary to neutralize the lime. It does not signify whether the spring water is boiled or unboiled, nor whether it contains sulphate of lime along with the carbonate, it is still *limy* in proportion to the quantity of carbonate of lime it contains. Agreeably to this idea too I find that the metallic oxides, as those of iron or copper, are thrown down by common spring water just the same as by free lime. Notwithstanding this, carbonate of lime in solution with water, contains twice the acid that chalk or limestone does. I fully expected the supercarbonate of lime in solution to be *acid*. But it is strongly alkaline, and scarcely any quantity of carbonic acid water put to it will overcome this alkalinity. Pure carbonic acid water is, however, *acid* to the tests. I could not be convinced of the remarkable fact stated in this paragraph, till I actually formed supercarbonate of lime, by supersaturating lime water in the usual way, till the liquid from being milky became clear. It still continued limy, and was even doubtfully so when 2 or 3 times the quantity of acid was added. It should seem then to be impossible to obtain a *neutral* carbonate of lime, as it is to obtain a *neutral* carbonate of ammonia in the sense here attached to the word neutral.

ACCOUNT OF THE

Floating Island in Derwent Lake,

KESWICK.

BY MR. JONATHAN OTLEY.

In a Letter to Mr. Dalton.

(Read Nov. 4, 1814.)

THE appellation of Floating Island given to a phenomenon which sometimes appears in Derwent Lake, has led many to imagine it a detached piece of earth, floating upon the water, unconnected with any other thing; this however is not the case. A portion of earth about six feet in thickness, certainly rises from the bottom, to the surface of the lake; but it is still connected with the adjacent parts, by a gradual sloping descent; so that a much greater extent is raised from the bottom, than reaches to the surface of the water.

The place where it makes its appearance is in the south east corner of the lake, not far from Lowdore, generally about one hundred and fifty yards from shore, where the lake is of no great depth, as from three to six feet in an usual state: The times of its appearance are irregular and uncertain; it has sometimes been observed in two successive years; at other times with an interval of seven or eight years; and mostly, I believe, about the termination of a warm, dry season. Its figure and extent are also variable; it has sometimes contained near an acre of ground, at other times only a few perches: and when at rest in the bottom of the Lake, it cannot be distinguished from the adjoining parts: in fact, it is not always the very same piece of earth that forms the Island.

It is entirely covered with vegetation, principally the *Isoetes lacustris*, interspersed with *Lobelia dortmanna*, and other plants commonly found growing in the bottom of this and all the neighbouring lakes; its surface, to the depth of a few inches, is composed of a clayey or earthy matter, apparently deposited by the water; the rest is a kind of imperfectly formed peatmoss; the species of some of the vegetables composing it, may be still recognised, and appear of kinds not now growing upon the spot, but probably accumulated at a remote period when the surface of the lake was much lower than at present: a conjecture which is countenanced by the appearance of the roots of large oak and other trees yet remaining in different parts of the lake, at the depth of about four feet under water. A quantity of air or gaseous matter is distributed through the whole substance of the island; but not pretending to determine the particular species, I contented myself with ascertaining it to be inflammable. The thickness of earth, as before observed, is about six feet, under which is water of a depth corresponding to the height that the island has risen; in some places three, in others six feet, and more when the water is high: at the bottom of this subterranean water is found a very fine soft, whitish substance; the resemblance of which to peat ashes, almost persuaded me that vegetable matter may, under peculiar circumstances, be converted to a similar state, without a perceptible combustion. At the depth of eighteen feet, the clay becomes very stiff and not easily penetrated.

After an absence of eight years, it emerged about the 20th of July 1808, and increased for a few days till it might be about eighty yards in length, it then remained with little alteration for some weeks; although the depth of the lake varied during the time, its height above water continued much the same, (viz. about a foot;) it then lowered gradually for some time, till the beginning of October, when a flood covered it, and it was seen no more till the year 1813, when it rose about the 7th of September; but only of small extent, about twelve yards in length; and although its height above water never exceeded seven or eight inches, yet when the lake rose about three feet perpendicular, part of the island still remained above the surface, and continued so till the latter end of October, when it disappeared, and has not been seen above water since.

In the year 1808, an anonymous letter appeared in the Carlisle Journal, describing this island, attributing its ascent to the rarefaction of air under it by heat, and stating that when it first appeared, no water could be discovered beneath it, nor any thing but confined air.

This not corresponding with my observations, I took the liberty of enquiring through the same medium, who had ever examined the island, when no water but only confined air could be discovered beneath it: and at the same time submitting as my own opinion that the earth was rendered buoyant by its being impregnated with a kind of gas, produced under or rather within its substance. The subject was then taken up by a third person, who in concurrence with a formerly received opinion, ascribed its ascent to a small brook, or rill of water, which falling from the mountain opposite the place, meets with a subterraneous passage, by which it enters the lake unperceived: which passage he supposes may be continued to this place, and that during a dry season its outlet may be so far choked up, that on the commencement of rain, the confined water may force up this island, but what I think militates against this opinion is, that the earth in its rising, not admitting of the required extension, is frequently rent quite through; so that the water under it has a free communication with the lake; and on examining the place at different periods of the late severe frost, in expectation of discovering by some difference of temperature, the place where the beforementioned brook discharged itself; the only appearance of a spring that I could perceive, was in the very edge of the lake. And admitting it possible, that the ascent of this island may be aided by some spring underneath; yet I am of opinion that its continuance upon the surface, must be attributed to the quantity of gas produced within its substance (by a decomposition of the vegetable matter of which it is formed), and which the clayey coating I think assists it in retaining; and I have ventured to suggest, that at a certain stage of this decomposition, the effect must be at its maximum; after which the eruptions will become less extensive or less frequent.

[*Note, by Mr. Dalton*]. Being at Keswick in 1815, Mr. Otley and I procured an apparatus for collecting a quantity of the inflammable gas generated in the floating island, then sunk two or three feet below the surface of the lake. We anchored a boat over the island, and with a long piked staff penetrated to the depth of a foot or more below the surface of the island. Each time the pike was withdrawn, a gush of air ascended in large bubbles, to the amount of a pint, more or less; part of which we caught by a bottle and funnel, without suffering it to come in contact with the atmosphere. This gas I found afterwards to consist of equal parts carburetted hydrogen and azotic gasses with about 6 per cent. of carbonic acid. The carburetted hydrogen was such that 1 part required 2 of oxygen for its complete combustion by electricity. It seems most probable that this gas is generated by the decomposition of vegetable matter in contact with water; two atoms of carbone decompose two of water and form at the same instant an atom of carburetted hydrogen and one of carbonic acid. The carbonic acid, being absorbable by water, is mostly dispersed, and the other gas collects in small bubbles in the spongy texture of the soil till it is disturbed, when the bubbles unite and ascend in a body. The azotic gas is probably from the atmosphere by means of the water. The almost total absence of oxygen gas from such mixtures is remarkable.

Mr. Otley has, I apprehend, suggested the only plausible cause that can be assigned for the rising and sinking of the island. The generation and temporary adhesion of such immense quantities of elastic fluid, must have great influence upon the sp. gr. of any mass; and when the mass happens to be nearly of the same sp. gr. as water and immersed in it, it will sink or swim according as the adhering volume of air is less or more, just as a cork loaded with lead floating in water does, in the common experiment in pneumatics.

ON THE *REFRACTIVE POWERS* OF MURIATIC ACID AND WATER, *separate, and in a state of mixture.*

BY

MR. HENRY CREIGHTON.

Communicated by Mr. Ewart.

(Read Feb. 18, 1814.)

IN the years 1808 and 9, being engaged with some experiments upon the application of different fluids to the formation of compound lenses, for correcting the aberration arising from the refrangible properties, of the rays of light, and being led by Dr. Blair's enquiries on that subject (detailed in the Edinburgh Transactions), to notice particularly the effects of muriatic acid and its compounds, some rather unexpected results occurred, not altogether connected with the above subject.

These I take the present opportunity of detailing, and their tendency is to shew, that rays of light passing through lenses of muriatic acid, are so refracted that the focal distances of the lenses are proportional to the specific gravity of the acid, &c. forming the lens.

These facts were ascertained by introducing a portion of the fluid betwixt two double-convex lenses of crown glass; these were cemented into a frame, for the twofold purpose of holding them steady, and of containing the acid, &c. to be experimented upon, to leave room for which the lenses were separated about $\frac{3}{16}$ of an inch. By this means a double concave lens of the fluid was obtained, and its less, or greater refractive power, lessened, or increased the focal distance of the compound lens, (the refractive power of the glass lenses being uniform, the results are not affected thereby.)

The focal distances of the glass lenses were about 24 and 27 inches respectively; when placed together in the frame, about 13; when the space betwixt was filled with water, nearly 24; and when muriatic acid was introduced, the focal distance was 28 inches.

The following table is the result of experiments made with muriatic acid of the specific gravity 1.177, and with portions of the same diluted with water; and with water alone. The specific gravities of each fluid were taken in the usual way by weighing a small bottle full of each, and which contained just 1000 grains of water; these occupy the second column of the table; the third column consists of the observed focal distances of the compound lens when containing these different portions of acid, &c. The fourth column is obtained from the third by calculation, being proportional to it; for instance, as the number opposite to water, $23\frac{3}{4}$, is to 1, (the specific gravity of water,) so is any other number in column 3, to the specific gravity of the corresponding liquid. Column 1 needs no remark.

Liquids	Sp. Gr. per Expt.	Focal dist.	Sp. Gr. deduced from focal dist.
Water	1.000	Inches. 23.75	1.000
Do. with a little acid	1.055	25.00	1.053
Do. with more acid	1.087	25.70	1.088
Do. do. do.......	1.121	26.60	1.121
Do. do. do.	1.146	27.00	1.138
Muriatic acid	1.177	28.00	1.180

From a view of this table it will be seen that the two columns of specific gravities, the one obtained from experiment, and the other from the theory, differ from one another in no instance so much as one per cent. and correspond as nearly as can be expected.

I have lately repeated some of these experiments, making use of different lenses set into a frame in the manner already described. One of these was flint glass, the focal distance $10\frac{5}{8}$ inches; that of the other (which was of crown glass) $9\frac{1}{2}$; of the two together $5\frac{3}{10}$ inches. The following table shews the results obtained with these lenses; and being similar to the above, needs no further explanation.

Liquids	Sp. Gr. per Expt.	Focal dist.	Sp. Gr. deduced from focal dist.
Water	1.00	8.4	1.00
Do. with acid	1.07	9.0	1.07
Muriatic acid	1.17	9.8	1.17

These results are similar to the former; the acid used in either case was not highly concentrated, but such as I was able to procure at the times the experiments were made; there can however, I think, be no doubt, on inspection of the preceding tables, but that the refractive power would increase (so that the focal distances would continue to be proportional to the specific gravity) with the density of the acid.

Conceiving that the other mineral acids might have these properties in common with the muriatic, I made a trial of them, using the glass lenses last mentioned, when the focal distances appeared to be as given in the following table.

Liquids	Sp. Gr. per Expt.	Focal dist.	Sp. Gr. deduced from focal dist.
Water	1.00	8.4	1.00
Nitrous acid.........	1.33	9.6	1.14
Sulphuric acid	1.70	10.25	1.32

This at once shews that the law, noticed respecting muriatic acid, does not apply to the nitrous or sulphuric, as the specific gravity of each is very considerably more than in proportion to the focal distance of the lens, when containing either: this circumstance seems to point out some similitude between the constitutions of muriatic acid and of water; the idea once entertained of the constituents of both being of the same species (but in different proportions), is I believe however generally exploded.

Dr. Wollaston and others have ascertained the refractive powers of some of the acids (and other substances), but I am not aware that the detail I have ventured upon, regarding the muriatic acid, &c. has been given before: If such is the case, I have only to plead ignorance of the fact.

The ratio which the refractive power of

the muriatic acid bears to its density, affords an easy method of determining the strength, or specific gravity, and this I imagine may be done with a degree of accuracy, sufficient at least for all common occasions, where the acid is used for manufacturing purposes; and the simplicity of the process scarcely admitting of material error, is not the least circumstance in its favour.

No doubt the same might be done with regard to the sulphuric and nitric acids, but from what has been stated, it will be readily seen that a proper apparatus for this purpose will not be so easily constructed. For the muriatic acid the scale of graduation is obtained from a single experiment with water alone, the specific gravity of which is had without labour (except as far as regards its temperature.)

In constructing any apparatus for this purpose it is perhaps desirable to use lenses which are less convex than those noticed, in order to increase the focal distance, and thereby render any small uncertainty in it, of less importance, when compared with the whole, than it would be were that whole distance materially lessened; as it must be obvious that the accuracy of which an instrument of this kind is susceptible, depends upon the nicety with which the focal distances can be ascertained. A hollow glass prism to contain the acid might perhaps be substituted in place of the lenses, with advantage; though some uncertainty might arise from the variable refrangibility of the rays of light passing through acids of different densities, and which might create some difficulty in comparing the different situations of the spectrum; this objection applies of course in a certain degree in the use of lenses, but by making use of such as have a considerable focal length, it becomes so trifling as not to be of material consequence for this purpose.

AN ESSAY

ON THE ORIGIN OF *Alphabetical Characters.*

BY THE

REV. WILLIAM TURNER, JUN. A. M.

(Read April 15, 1813.)

IF we examine the various contrivances which have contributed to the improvement and the happiness of mankind; whether we consider the extent and importance of the effects, or the simplicity of the means by which these effects are produced, we shall find none more calculated to excite our astonishment, our admiration, or our gratitude, than the art of writing by means of *Alphabetical Characters.* It has been asserted, and perhaps with truth, that without this art no high degree of civilization is attainable. However this may be, it is at least certain, that if we were at present deprived of it, we should lose one of the most important and efficacious means of improvement which we possess.

The origin of this art, simple and elegant as it is, is buried in the remotest antiquity, and it is difficult, perhaps impossible, to assign the precise period to which it is to be referred. This difficulty, together with the beautiful simplicity of the art itself, apparently so inconsistent with the state of barbarism in which the world in early ages is generally conceived to have been plunged, seem to be the principal circumstances which have induced many learned and ingenious men to adopt the opinion that we are not indebted for it to any efforts of human ingenuity, but that it was the gift of immediate and express revelation. This opinion is sanctioned by the authority of various respectable writers, who have supported it by arguments which many have thought not unplausible. Among these writers, it may be sufficient to mention *Dr. Hartley*, who has stated some considerations which seem to him to favour this opinion, in the first volume of his Observations on Man,* and the celebrated *Gilbert Wakefield*, whose ingenious paper on the subject this society has inserted in the second volume of its Memoirs.† For this among other reasons, I shall adopt Mr. W.'s Essay for the most part as my guide to the principal arguments which have been adduced in favour of this doctrine.

* Observations on Man, Vol. 1 Prop. 83.

† Manchester Memoirs Vol. 2. p. 294.

It is a doctrine, I confess, in which I find myself unable to acquiesce. I cannot but consider the art of alphabetical writing as analogous in its origin and progress to all other arts; as originating in the same powers of reason and understanding, and differing from them only in so far as it may be supposed, from its superior excellence, to have required a greater exertion of the intellectual powers than most of these arts probably did.

In what follows, I propose to examine the arguments which have been brought forward in support of this hypothesis, and then to state those considerations, which induce me to refuse my assent to it. The arguments advanced by Mr. Wakefield in the Essay above referred to, in favour of the divine original of alphabetical characters, are nearly as follows. *First;* It is said that this invention, if it was a human invention, differs in one very remarkable circumstance from all others; namely, that the first effort brought it to perfection. All the attempts which have been made, to improve upon the Hebrew alphabet have failed of success. *Secondly*, it is said that if alphabetical writing was the result of the natural ingenuity and experience of man, we might reasonably have expected to hear of the invention having been made in more places than one; we might have expected to find different nations independently of each other falling upon the same expedient. This expectation however is disappointed. All the alphabets at present in use may be traced either by external or internal evidence to the same source. In the *third* place, it is contended that the uniform failure in devising any contrivance similar to the alphabetical characters, or at all comparable to them in simplicity and convenience, of all those nations who have continued for a great length of time unconnected with the rest of the world, and who appear nevertheless to have acquired considerable proficiency in various other arts and sciences, is a strong proof that the invention is not to be ascribed to any effort of human ingenuity. *Fourthly*, it is said, that whereas most of those who consider alphabetical characters as a human invention, conceive them to have originated in a supposed simplification of the Egyptian hieroglyphics, it would appear that this transition is by no means so easy and natural as it has generally been represented.

These arguments it shall now be my business to examine in their order.

I. In the first, which is derived from the supposed peculiarity of this art in having advanced at once to perfection, two things are assumed which are not conceded, and the proof of which, by any fair or plausible mode of reasoning, would be found, I suspect, rather difficult. In the first place, it is assumed, without proof, that the specimen of alphabetical writing which we have in the books of Moses is not only the most ancient now extant, but the very first that ever appeared; whereas it is both very conceivable and very probable that the art existed in an imperfect state long before this time, and that the more ancient monuments in which it was exhibited, have either perished through the lapse of time, or, having been transcribed according to a new and improved method afterwards invented, the originals, displaying the art in its less perfect state, were neglected and finally lost. If the opinion of Eichorn, and some other eminent critics, be correct, acccording to which the book of Genesis was not entirely the original composition of Moses, but was compiled by him from more ancient documents and records; evident traces of which compilation they imagine to be discernible in the sudden transitions of style, and in certain peculiarities of phraseology observable in particular parts of this book, it is obvious that the suggestion here hinted at, will be rendered still more probable.

But, granting that there is no evidence of the existence of any specimen of alphabetical writing prior to the Pentateuch, and this is all that can by any possibility be conceded, still the proposition which follows is by no means self evident. That the Hebrew alphabet is superior to all those which have since been adopted in other nations, and not only so, but completely and absolutely perfect, is an assertion which I imagine few will be inclined to admit, and which I own appears to me not only not so clear and indubitable as by some it seems to have been supposed, but absolutely false and unfounded.

What do we mean by the word *perfection?* When applied to the acceptation of means to ends, it may not improperly be defined the capacity of attaining these ends to their full extent, and in the completest manner possible. Now, what is the object of alphabetical characters? To convey ideas by means of visible signs corresponding to the audible signs of spoken language. And the method by which it proposes to effect this, is to denote by visible marks the elementary articulate sounds, which by their various and successive combinations, compose the words we employ in expressing our ideas. A *perfect* alphabet therefore, must be capable of denoting by distinc characters, all the articulate sounds which are employed for this purpose, and when it can be shewn that the Hebrew alphabet is capable of effecting this, then, and not till then, shall we be authorised to admit its alledged perfection. But that this is not the case, is so obvious as not to admit of a moment's dispute. Let any one consult his experience and his ear, to ascertain the number of simple sounds, as well vowels as consonants, which his organs of speech are capable of enunciating, and he will immediately perceive that an alphabet presenting only twenty-two letters, cannot possibly furnish separate characters for them all. If it be attempted to obviate this, by pleading that such an alphabet may nevertheless have been sufficient to express all the articulate sounds in use among the Israelites; I ask in reply, how do you know this? This is mere affirmation, unsupported by the shadow of a proof; and we cannot permit assertion to be substituted in the place of argument.

The second supposed peculiarity of this invention from whence an argument is drawn, to prove its divine original, is its having been made only once; all the nations among whom it is at present in use, being said to have derived it from one common source. It is certain at least, that all the modern European nations except the Russians, who probably received them direct from the Greeks, derived their knowledge of letters from the Romans. The Romans obtained them from the Greeks, who if we are to credit tradition, received them by the hands of Cadmus from the Phenicians.* The Coptic, Ethiopic, and Ara-

* Dr. Hartley, who is a zealous, and for the most part an able, advocate of the opinion we are now examining, supposes that the Phenicians learnt the use of letters from the Philistines, and must needs have it that these and all the other neighbouring nations continued in complete ignorance of the art, till the taking of the ark by the Philistines, put them in possession of the copy of the law there deposited, which Dr. Hartley, upon what grounds I know not, seems to think was the only specimen of alphabetical writing at that time in the world; although according to his own account, the art had then been in existence among the Israelites for some centuries. But this way of accounting for the communication of the method, seems to me very strange and unnecessary. Surely it is not at all likely that two nations should remain so long in the same country as the Philistines and the Israelites, between whom there seems to have been a constant intercourse going on, of one kind or another, without the communication of so remarkable an invention as this. If the Philistines had not learnt the use of letters before the taking of the ark, they were not likely to be induced to study them from such an occurrence. The true state of the case, I have no doubt, is, that long before this time, the practice of alphabetical writing was become very general among all the nations who inhabited this district.

bic alphabets, judging both from external and internal evidence, appear to be referable to the same quarter, so that all the alphabets of which we have any knowledge, may, it seems, be distinctly traced to one central point, not far distant from the borders of Egypt and Arabia. The facts brought forward by Mr. Wakefield and others in support of this position, are certainly singular and curious, and the evidence appears to be sufficient to authorise our assent. But that this circumstance, however undeniably it may be established, furnishes any argument for the divine original of letters, or that it is in any degree either extraordinary in itself, or peculiar to the invention in question, I do not at present see any reason to believe. On the contrary, it appears to be a circumstance attending this in common with various other arts, which yet, so far as I have ever heard, were never supposed to be revealed from above. To mention one remarkable instance, which I select from among many others, chiefly on account of the striking analogy which it bears in other respects to the art of alphabetical writing. I mean the mode of arithmetical notation, performed by what are commonly called the *Arabic* numerals. This invention, I believe, was never attributed to a divine revelation, and yet nearly all the arguments brought to prove the divine original of letters are applicable with equal if not with greater force to shew that the Arabic numerals are derivable from such a source.

An advocate of the divine origin of this art might argue thus. "As in the invention "of letters, so here, the first effort seems to "have brought it to perfection. No improve- "ments have ever been attempted on the "Arabic cyphers; they have been handed "down to us in their present state, through a "period of more than a thousand years, and, "in all that time, have scarcely been even "altered in their figure. If any one should "pretend that the less perfect attempts, in "the gradual progress of this invention, may "have been neglected and forgotten, and "that all monuments of them may have long "ago perished, I reply, "this is *mere affirma- "tion* unsupported by the shadow of a proof." "Again, if we trace the history of this art, "we shall find precisely the same phenome- "non which has been represented as so extra- "ordinary in the history of letters. These "characters seem never to have been inven- "ted but once, and all the nations who now "use them have plainly derived them from "the same source. All the modern European "nations acknowledge that they received them "from Spain. Did the Spaniards invent them "then? No; they got them from the Moors; "the Moors learnt them from the Arabians; "and even these do not seem to have been the "original inventors; but to have brought "them from the East Indies; a region in "which many of the arts and sciences flou- "rished in a very remote period of antiquity, "and which appears to have been the origi- "nal birth-place of this as well as of several "other important inventions. Accordingly "on examining the arithmetical figures em- "ployed in the East Indies with those in use "among ourselves, we find unquestionable "evidence of a similarity of origin, not only "in the identity of the principle upon which "they proceed, but in the striking resem- "blance observable in the characters them- "selves."

Thus it appears that both the arguments hitherto examined in favour of the doctrine now under consideration may be applied to at least equal advantage, in proving the divine origin of the Arabic numerals. If I mistake not, we shall presently find that the analogy is capable of being carried still farther. But what has already been advanced is I think abundantly sufficient to shew that the circumstance here mentioned and insisted on as a strong proof that letters could never have been the fruit of human contrivance, is so far from proving this, that it is not even peculiar to the invention in question, and is found to be equally the case with a multitude of other arts, which have never been attributed by the wildest stretch of the most visionary imagination, to any other source than the true one, namely, the natural vigour and ingenuity of the human understanding.

The third argument above stated is of a nature extremely similar to that which we have just been examining, and may be refuted in a similar manner. "Is it not strange," it is said, "if letters be a human invention, that a "people like the Chinese, so famous for their "discoveries, and for the mechanical turn of "their genius, though they have made some "advances towards the expression of their "ideas by arbitrary signs, should have been "so completely unable to accomplish so simple "a contrivance?" And is it not equally strange, it might be replied, that two such enlightened nations as the Greeks and Romans, a race of men vastly more improved in every respect than the Chinese ever were or are ever likely to be; who had made a considerable progress in the mathematical sciences, whose acquaintance with the properties of numbers was so extensive and profound, should have submitted for so many centuries to the inextricable difficulties and inconveniences occasioned by their unscientific notation? If the modern method of numeration were a human invention, is it not extraordinary that a people whose acuteness and ingenuity seem never to have been surpassed, should have been so long engaged in philosophical enquiries, without ever discovering it?

A fourth argument in favour of the opinion we are now examining, is derived from the alleged failure of all the attempts which have been made to trace the successive steps of the transition from the hieroglyphics in use among the Egyptians to alphabetical characters. From any knowledge we have, either of hieroglyphics, picture writing, or any other contrivance of a similar nature, or of the various nations who made use of them, we have no ground to infer any progress or tendency toward such an improvement. On the contrary, we see nothing in these methods of expressing *ideas immediately* by written characters, but a progress from bad to worse, from aukward and clumsy to unintelligible and inexplicable. Though originally modes, however imperfect, of communicating ideas, they degenerated into objects of superstition, and from their complexity and intricacy contributed to confine learning and knowledge of every kind, to the priests or other privileged orders, who alone had leisure and opportunity to make themselves masters of such complicated and laborious contrivances.

It must be recollected that all these methods differ essentially from that of alphabetical writing in their fundamental principle. It is their object to furnish visible symbols to denote things or ideas *immediately*, without any relation to the audible signs already in use; whereas the various combinations of letters express ideas only by forming written *words* analogous to the words used in spoken language. Agreeably to this distinction we find that the written symbolical character of the Chinese bears no relation whatever to diversities of language, but is equally understood, (so far as it is intelligible at all), in Tongquin, Tartary and Japan, the languages of which countries are totally different; precisely in the same manner as collections of Arabic numerals, tables of logarithms for instance, express the numbers they are employed to *represent equally in all countries and tongues.*

It has been supposed* not altogether inconceivable that when such characters were improved to all that variety and multiplicity which is necessary for representing all kinds of objects, they might be capable of resolution into their simple component parts, and rendered pronunciable, by affixing some simple or short sound to each of these parts, and that thus an association, being established between the sound and the mark, the sound would at length become the name of the mark; and the mark the picture of the sound. But it would seem more probable that a set of arbitrary characters (expressing, it must be always remembered, not words but ideas), would be found not merely difficult of analysis, but absolutely incapable of it, since from the very mode of their formation, they could not be supposed to present those elementary constituent parts, which are necessary for this purpose. At any rate, it is obvious that the marks being formed upon entirely different views, and in consequence of different analogies from those which regulated the formation of words, they would bear totally different relations of resemblance from those subsisting among the corresponding sounds. Hence

* See Hartley on Man, Vol. 1. p. 301.

it clearly follows, that if this hypothesis supposes the words already in use to express the same ideas to have been employed as names of the corresponding characters, such a procedure brings us no nearer to an alphabet; and on the other hand, if other sounds entirely different be conceived to have been adopted to render the character enunciable; this is supposing an undertaking to be attempted for which no imaginable motive can be assigned. According to this supposition, there would then be *three* entirely distinct methods of communication in use; first, the original language; secondly, the written character; and thirdly, another set of audible signs, altogether different, and bearing no resemblance or analogy whatever to the first. Whether such an hypothesis is possible, or conceivable, I leave to be considered.*

* "Though it is likely that all hieroglyphical languages were originally founded on the principle of imitation, yet in the gradual progress towards arbitrary forms and sounds, it is probable that every society deviated from the original in a different manner from others, and thus for every independent society there arose a separate hieroglyphical language. As soon as a communication took place between any two of them, each would hear names and sounds not common to both, and each reciprocally would mark down such names in the sounds of its own characters bearing, as hieroglyphics, a different name. In that instance conse-

From these considerations, it certainly seems to follow that the method of hieroglyphics is by no means fitted for leading men to conceive the possibility of such a simple contrivance as that of Alphabetical characters; but that on the contrary, from the total want of all

quently the characters ceased to be hieroglyphical, and were merely marks of sounds. This practice would lead imperceptibly to the discovery that with a few hieroglyphics, every sound of the foreign language might be expressed, and the hieroglyphics which answered this purpose best, either as to exactness of sound or simplicity of form, would be selected for this use, and, serving as so many letters, would form in fact what is called an alphabet. This natural progression has actually taken place at Canton, where, on account of the vast concourse of persons using the English language who resort to it, a vocabulary has been published of English words in Chinese characters, expressive merely of sounds, for the use of the native merchants concerned in foreign trade, who by such means learn the sound of English words." *Sir G. Staunton's Embassy to China, Vol. II. p.* 576

The fact here stated is singular, and the speculation founded upon it, and apparently suggested by it, with regard to the mode in which a transition may be conceived to have been effected from hieroglyphics to an alphabet is certainly ingenious. At the same time it must be recollected that the expedient of employing the Chinese characters to denote the sounds of English words or syllables similar to those of the corresponding Chinese words, seems to have been introduced by Europeans who were already familiar with alphabetical writing. The previous use of an alphabet may naturally enough be conceived to suggest this ex-analogy in its fundamental principle, it must necessarily tend to drive them farther and farther from it.

So far then I am disposed to acquiesce in the reasoning of Mr. Wakefield, and other defenders of the divine origin of letters. But when they proceed to maintain that upon the reality of this supposed transition, the cause of those who believe in the human origin of these characters *entirely* depends, they are surely running on too fast. Because one particular mode of expressing our ideas seems to have no tendency to terminate in this art, does it follow that no other circumstances can ever have led the mind of man to such a result? Because the hieroglyphical characters do not seem likely to facilitate the progress of the mind towards this discovery, are we reduced to the necessity of seeking for a divine, supernatural, miraculous interposition to account for it? Surely not. A thousand previous circumstances may be conceived, which

pedient; but it does not therefore follow that the previous adoption of this expedient would suggest an alphabet. Besides, this hypothesis assumes that the most difficult step in this transition had already been made, before the expedient in question was resorted to; namely, the transference of characters hitherto employed tc denote ideas merely, without reference to sounds, to express sounds merely, without reference to ideas.

might lead towards the desired point. Nor, in order to prove that any invention was the fruit of the unassisted efforts of the human understanding, is it necessary to point out exactly the steps of the enquiry which led to the device in the mind of the original inventor. If we could not point out one of these steps, it would not follow that the human powers were unequal to the discovery. Nor would this circumstance, singular as to some it may appear, be at all peculiar to the invention of letters. The whole history of the arts abounds with examples of it. Who can pretend to point out the circumstances which led Guttemberg, or whoever invented printing, to the discovery of that art? Who can delineate the progress of that man's mind who devised the application of water to machinery for the purpose of grinding corn? To point out with precision in either of these cases the successive steps of the progress so imperiously demanded, will perhaps be found no very easy undertaking; but no one, I apprehend, ever supposed on that account that the art of constructing water mills was a divine revelation.

Having thus disposed of the principal arguments which have been brought forward in support of this hypothesis, I shall now proceed to state as briefly as possible, the objections which weigh most strongly in my mind against it. The first of these I shall beg leave to state in the words of an often-quoted passage,

> Nec *Deus* intersit, nisi dignus vindice nodus
> Inciderit.

We do not find in the course either of common or extraordinary Providence, that the Divine Being interposes to perform any thing for us, which by the use of our own natural powers we can do for ourselves. To shew that there is *not* any thing in the art of writing which is beyond the natural capacity of man, is not necessarily a part of our present business; it might rather be demanded of our antagonists to shew that there is; for it is a well known rule of logic, that in all controversies of this nature, the *onus probandi* lies on the affirmative side of the question. However, before I conclude this essay, I flatter myself that I shall be able to shew, in a manner sufficiently satisfactory, that this is not really the case, and that the invention before us is no more beyond the reach of the human faculties, than a hundred others which have crowned with success the ingenuity and industry of man.

But the maxim of the poet is applicable to the present question in another point of view. I believe we shall find no instance, among all the divine revelations of which we have authentic accounts, of any which have not had an immediate reference to the duties and expectations of man as a moral and religious being. In every other case in which the author of our being has seen fit to open a direct supernatural communication with his creatures, it has been in order to make his will and their duty known to them in cases to which the light of reason did not extend, or to enforce the practice of it, by additional and more powerful motives. Indeed we should naturally expect to find, in every such revelation, an immediate reference to the relation which man bears to his Maker, and to the duties which arise out of that relation; and it would seem not only contrary to the analogy of the other divine dispensations, but beneath the dignity of revelation in general, to unravel to mankind the mysteries of any art or science, or to enter into a particular explanation of subjects which had no immediate connection with morality or religion. The directions given for the construction of the tabernacle, may perhaps be thought an exception to this rule; and indeed it seems to be alluded to by Dr. Hartley, as a case analogous to the revelation of letters; but it should be recollected that the form of this edifice and its various appurtenances, was necessarily connected with the ceremonial of the Levitical worship; and though the pattern may have been given from the mount, it does not appear that Bezaleel or Aholiab were inspired with the art to complete it according to the pattern; nor is it at all likely that this would be necessary, since they might very probably have learnt their respective arts in Egypt, where they had already been brought to such high perfection.

In the *second* place, it may reasonably be demanded, at what time this supposed revelation took place. If the art was actually communicated from above, there must surely be some historical evidence of so signal a miracle. For it is not conceivable that the Supreme Being should have made so important a revelation, and have left it entirely without notice or memorial. Indeed we might naturally expect that the very first employment of the art, if it had been so obtained, would have been to hand down to posterity, an exact account of an event pregnant with such important consequences to the whole race of mankind; in order that all subsequent ages might ascribe the glory and the praise where alone they were due. This we should certainly have expected from considering the conduct of Divine Providence with regard to other occasions of miraculous interference. Yet this we do not find to have been the case.

It is true indeed, that Dr. Hartley* and various other writers, have maintained that the revelation was made at Mount Sinai, when the ten commandments are said to have been written *by the finger of God.*† The use of this expression however does by no means warrant the inference which is here deduced from it. It is obvious that the finger of God is here spoken of in precisely the same way as in other places, the sacred writers speak of *the word of his mouth,* the *breath of his nostrils,* &c.; using a figurative language, accommodated to our imperfect conceptions. When it is said therefore that the decalogue was written by the finger of God, nothing more can be meant, than that it was done by his direction, or in consequence of an express act of his power; and it appears no more "harsh" to suppose that in making this communication, he should adopt a species of written characters already in use among his creatures, than that in his oral revelations he should convey

* Vol. 1. p. 308. † Exod. xxxi. 18.

ideas to the minds of his prophets by means of those articulate sounds which already made a part of human language. But a decisive objection to this hypothesis is derived from the circumstance that the art of writing is twice mentioned in this very book of Exodus *before* the account of the ten commandments;* to which it may be added, that in what is said to have been written on the two tables of stone, the whole Hebrew alphabet is not to be found. At this time therefore the art cannot have been revealed; and no other precise time has ever been pointed out; a very satisfactory proof to my mind that no such time ever can be fixed, and that no such revelation ever was made.

In the third place, although the chief excellence of this art doubtless lies in the principle on which it is founded, and is unconnected with the peculiar nature or form of the characters employed, yet I cannot bring myself to believe that in a divine revelation so little attention would have been paid to convenience or beauty. The main object certainly is to express our ideas; but it is also an object to do this expeditiously, and it is even desirable that the character should present an appearance pleasing rather than otherwise to

* Exod. xvii. 14. xxiv. 4.

the eye. Neither of these objects was in the smallest degree attended to in the ancient Hebrew alphabet, which if we may judge from coins and other relics, appears to have been very similar to the present Samaritan; and I think every one will readily admit on the slightest inspection, that a more rude, aukward, inconvenient set, could not easily be imagined.

I have now stated such observations as appear to my mind to afford a satisfactory answer to the arguments brought forward in favour of the supposed divine original of letters, and likewise those objections which seem to militate most strongly against such an hypothesis. It may now be worth while to endeavour to point out the gradual progress by which (since I have excluded the commonly supposed transition from hieroglyphics), we may conceive that the mind of the original inventor *may possibly* have been led to that train of thoughts which terminated in so fortunate a result.

But before we proceed to any enquiries of this kind, it may be proper to obviate a misconception into which we might otherwise be liable to fall. It has been observed that persons who have not learnt to read, have very imperfect notions of the distinction of words, and therefore cannot be supposed to have any conception of the nature or use of letters. Hence it is inferred that, all mankind being in this predicament before the introduction of letters, they would be equally incapable of analysing the sounds made use of in language. Now this mode of concluding from the illiterate of the present day to the whole human race, before the invention of letters, seems to me very hasty and erroneous. The class who know nothing of letters *now*, includes only those who are ignorant and unaccustomed to any kind of speculation or reflection; *then*, it comprehended all the wisdom and learning in the world. Because the philosophers of Egypt and Chaldea resemble the uneducated of the present day, in their ignorance of letters, we are not to suppose that they were therefore as ignorant in every respect; as destitute of reflection, as little likely to trace out the elementary sounds which compose the words of spoken language as these latter would be. Every one at all acquainted with the history of philosophy or of the world in general, must acknowledge that these nations at a very early period had made a considerable progress in various arts and sciences. The stupendous monuments of their skill in the mechanical arts which to this day excite the astonishment of every beholder, prove also that in very remote antiquity, mankind had acquired that degree of civilization which fits them for uniting together in large bodies, and for exerting their joint efforts in the prosecution of one vast undertaking. In the time of Abraham it appears that commerce was already become a separate profession, and had arrived at such a degree of maturity, that the precious metals were employed as a regular medium of exchange; a practice which evidently implies a considerable degree of refinement; since it could not have been introduced or even thought of, if mankind had not been led, by long experience of the inconveniences of the previous system of barter, to pay some attention to the general principles of commerce. These, and a variety of other historical facts, resting upon the most unquestionable evidence, clearly shew, that mankind in those days can by no means be represented as in a state of ignorance and barbarism, merely because they were unlettered. On the contrary, it appears manifest that such a degree of improvement had previously taken place, as to render the mutual intercourse between the different members of society very extensive. The progress of knowledge had already advanced to such a degree that the information which each person had to communicate was an object of interest and importance. Hence the want of some contrivance to facilitate this communication would be quickly felt; the inconvenience arising from the want of some means of fixing, in a durable form, the fleeting sounds which issue from the organs of speech, would frequently, present itself, and a strong desire consequently would be conceived, to devise some method of accomplishing so important an object.

Let us suppose then that such a desire was generated in the mind of some eminently enlightened and ingenious inquirer of those days, in sufficient strength to induce him to devote his thoughts to the subject. Two methods would, in all probability, present themselves to his choice. *The one,* to endeavour to express to the eye, by visible signs, the ideas which were expressed by means of spoken language to the ear, without any reference to, or connexion with the audible signs already in use; thus forming a completely new language addressed to the eye, altogether independent of the old one addressed to the ear;—*The other,* to devise a system of visible signs corresponding to the words used in spoken language, dependent upon them, and conveying ideas to the mind only in consequence of their connexion with them. Now, arguing from probabilities, the question is, which of these two methods he would be most likely to prefer, one by which he would be reduced to the necessity of framing an entire new language, and of devising a new system of signs altogether unconnected with the old ones; to be associated each with a separate object or idea, according as fancy or arbitrary caprice directed; or one in which the spoken language already in use was taken as the groundwork of the system; the visible symbols being associated with the objects of thought only according to that regular method which their previous connection with the words already employed to denote these objects, presented? On comparing these two projects, (without having any view as yet to alphabetical characters) I think no one who attends to the subject fairly and candidly, can hesitate to pronounce which way the judgment would be given. It will be objected that the Chinese nevertheless have fixed upon the former of these methods, in preference to the latter; be it so; is one man's having made an injudicious choice, a proof that no other man could ever make a better?

If then it appears that in consequence of this comparison, our supposed enquirer would be naturally led to prefer the plan of connecting his visible signs immediately with the words of spoken language, rather than with the ideas or external objects which those words denoted; the next enquiry will be into the principles upon which it is probable that the system of visible signs would be constructed.

But before we enter upon this enquiry it will be necessary to recall to our recollection the object which we had in view, in instituting the present examination. This was to substantiate one very powerful argument in favour of the opinion we are considering, derived from the observation that the Divine Being never interposes to do any thing for us, which, by the exertion of our own natural faculties, we can do for ourselves. For this purpose it is desirable to shew, if possible, that there is really nothing in the invention of letters which can be considered as beyond the reach of the unassisted powers of the human mind. In the present stage of our enquiry, therefore, it is not absolutely necessary that we should point out the very process according to which it *actually did*, or according to which it *probably would*, present itself to the mind; but merely to suggest a series of steps which *possibly might*, produce this effect.

This is all which can reasonably be required, and if this be done, it ought certainly to be considered as sufficient to overturn the supposition that the Deity miraculously interposed to disclose it.* Let us see then, if we can trace out any plausible conjectural history of the manner in which, by following such an opening as has just been described, the inquisitive mind might be insensibly carried forward to an *instantia crucis*, as Lord Bacon would have termed it, which would

* Such theoretical views of human affairs, as has been excellently observed by Mr. Stewart, speaking of a subject very analogous in its nature to that which we are at present considering, namely, the original progress of language, are not subservient merely to the gratification of curiosity. "In examining," says he, "the history of mankind, as well as in examining the phenomena of the moral world, when we cannot trace the process by which an event *has been* produced, it is often of importance to shew how it *may have been* produced by natural causes. Thus in the instance which has suggested these remarks, although it is impossible to determine with certainty what the steps were by which any particular language was formed, yet if we can shew from the known principles of human nature, how all its various parts *might* gradually have arisen, the mind is not only to a certain degree satisfied, *but a check is given to that indolent philosophy which refers to a miracle whatever appearances either in the natural or moral world, it is unable to explain.*"

point out his path to the desired end of this intricate investigation.

When those who first conceived the idea of expressing their thoughts by means of visible signs had determined, (as we have supposed would probably be the case) in favour of the method of connecting such signs immediately with the *words* by which these thoughts were already expressed in language, it is not at all improbable that their first plan would be to invent a separate mark for each individual word. In an age like the present, and with a language like ours containing fifty or sixty thousand words, this would of course be an endless undertaking; and would scarcely answer the purpose were it completed; but in an early and simple age, when languages were not so copious, the task might seem less arduous. Perceiving the inconvenience which would arise, if they left the allotment of these characters to the suggestions of arbitrary fancy, they would of course endeavour to devise some regular plan according to which certain varieties in the characters might be made to correspond with analogous varieties in the words which they were employed to represent. This regular plan might proceed either upon certain supposed resemblances in the ideas which the words conveyed, or upon a similarity of sound in the words themselves. Of these they would be much more likely to adopt the latter than the former; both because it is more consistent with the rest of the scheme, and because the circumstances of resemblance between objects of sense are more obvious, simple and determinate than the analogies subsisting among objects presented only to the mind.

Again, when in the course of time the gradual developement of new ideas and new discoveries led to the introduction of additional words, characters would be appropriated to them upon the same principles, according as their sounds appeared to have an affinity or resemblance to those of other words already provided for. Thus they would be led to observe the minuter shades of difference between the sounds of words, and to attend to the operation of the different organs of speech in giving utterance to the various articulate sounds which they had occasion to make use of in language. When once men were induced to pay this degree of attention to the subject, the rest of the process seems to me perfectly easy and obvious. That a man of only ordinary sagacity and penetration, when he had arrived at this point should think of analysing the words he made use of in language into the various articulate sounds of which they were composed, and that to a certain extent he should be successful in this analysis, is to my mind no more surprising than that a chemist or natural philosopher should think of examining the different bodies which were submitted to his notice, in order to discover the elementary ingredients which entered into their composition. *No more* surprising, did I say? On a comparison of the two I might rather have said, that the latter was the surprising and visionary attempt, and that the former, if not easy and obvious, was at least natural and reasonable. Would a naturally ingenious and inquisitive, though unlettered, Englishman, if any circumstances led him to attend to the nature of the sounds he uttered in speech, have any difficulty in perceiving that the words *man*, *mane*, *mate*, &c. began with the same conformation of the organs? Why then is it thought a thing incredible that a man should attend to the nature of the sounds used by him as audible signs in the expression of his ideas? Why should it appear so strange and unaccountable, that he should endeavour to analyse the elementary articulate sounds of which are composed the different words he has occasion to employ? Is there any thing peculiar in this? Is it in any respect different from those enquiries which the ever active and prying curiosity of man is constantly urging him to attempt?

The analysis, I grant, would at first be extremely imperfect; so are first attempts in all other cases. Some perhaps might be contented with reducing words to their *secondary* elements, if I may borrow such a phrase; and thus form a sort of syllabic alphabet.* Those who proceeded to the *primary*, or, as perhaps they ought rather to be called, *ultimate*, elements, would probably first distinguish from the rest the more prominent and remarkable consonants, (such as s, d, k, and several others;) these, when once marked and distinguished, would be recognised on all occasions, in all kinds of words, in every conceivable connection and combination, but that a complete and *perfect* analysis of the simple sounds which enter into language is not necessary for the formation of a system of alphabetical writing, our own alphabets at the present day may convince us; and however defective and imperfect the original analysis

* It is said, I know not on what authority, that the Tartarian alphabet is something of this kind. (*See Rees's Cyclop. art.* ALPHABET.

may have been, it was sufficient, I apprehend, to suggest the idea that characters substituted for these elementary sounds would, by their various combinations, form written words analogous to the words in language produced by the actual combination of the sounds themselves. That this idea is perfectly easy and obvious, I do not pretend to affirm; it is doubtless a very ingenious one, but that it is so wonderfully difficult, mysterious, and sublime as some have delighted in representing it, I do most strenuously deny. On the contrary, it does not seem in its own nature to have called for any thing like so astonishing an exertion of genius as many other inventions which have nevertheless been universally ascribed to mere human ingenuity.

When the art was once invented, the circumstances which would give rise to its improvement are perfectly evident; and, when the system was brought nearer to perfection, that the more rude and imperfect stages of it should be neglected and forgotten, and all monuments of them be lost, is surely not at all surprising. That this was the case at least with regard to some alphabets, we have it still in our power to prove by facts. The Greek alphabet, we are told, when originally introduced by Cadmus, consisted but of sixteen letters, four were afterwards added by Palamedes, and four others by Simonides.* During the period which intervened between the first of these improvements and the second, the poems of Hesiod and Homer appear to have been composed; yet in these works as we have them handed down to us at present we discover no traces of this imperfect alphabet; and the reason is obvious. After the improvement was introduced, they were again transcribed according to the new systems, and the older copies, written at a time when the art was in a less perfect state, were neglected, and, through the perishable nature of the materials, finally lost. The works of Moses may very possibly have undergone a similar process; and indeed, if the authority of the Greek historians can be relied upon, when they assert that letters were imported into Greece from Phenicia, it becomes extremely probable that the Hebrew alphabet itself did not at that time contain more than sixteen characters, and therefore that this was actually the case.

On the whole then, on a careful review of the foregoing arguments, I find myself under a necessity of believing that the art of al-

* Plin. Lib. vii. c. 57.

phabetical writing was in reality a human invention; and had its origin in those same powers of reason and understanding, to which all the other inventions that have contributed to the comfort and happiness of mankind, and which seem in many instances to have required even a greater exertion of ingenuity and contrivance, are universally and justly ascribed.

OBSERVATIONS

ON THE

RISE AND PROGRESS OF THE COTTON TRADE

IN GREAT BRITAIN,

Particularly in Lancashire and the adjoining Countiés.

BY JOHN KENNEDY, ESQ.

(Read Nov. 3, 1815.)

THE frequent complaints, both in public and private, against the manufacturing system, certainly demand an impartial investigation, and none are more called upon to take a part in such discussions than those who are interested in manufactures.

I am aware of my inability to throw much light upon this subject, but my present attempt will show at least a willingness to have the matter fairly investigated.

The few observations which I shall now submit to the society, relate chiefly to facts and circumstances that have taken place within my own experience and observation, or which rest on the testimony of men whose intelligence and veracity I could depend upon.

It will be proper perhaps to take a retrospect of 50 or 60 years, in order to shew the changes that have taken place from time to time to the present day, before we can appreciate the advantages or disadvantages, as well as the causes of these changes.

With regard to the operation of weaving, I believe it will be admitted that it remains nearly the same as it was 50 or 60 years ago, or indeed at any period, or in any country where the people have been in the habit of weaving for a subsistence; with the difference only of the application of the fly-shuttle, which was invented and introduced about the year 1750, by Mr. John Kay, of Bury;—at that time the cotton was carded and spun by hand in the weaver's family, and the manufactory was carried on to an extent sufficient to supply a limited home consumption.

Even then, however, there were frequent fluctuations in the demand for cotton fabrics; the causes of which may have proceeded from a variety of circumstances, such as an occasional scarcity of food, or any other obstruction to the progressive improvement of the country.

Under such circumstances, when a stagnation took place it was natural that the manufacturer would, rather than be out of employment, endeavour to find a market for his goods in other countries. And from this principle arose a foreign trade, with all its train of changes and fluctuations, much greater perhaps than the fluctuations of the home trade which preceded it; and the consequences were, I believe, nearly as follow. With their new connexions, the manufacturers soon found that they could not supply the increased demand for their cloths, and the first consideration was, how they were to produce a greater quantity in their respective families. It naturally occurred to them that, if they had another loom, or another hand to spin, they might be able to supply this additional demand.

But, if they were all employed before, this could not be done, unless they could make some arrangement by which the same number of hands might produce a greater quantity of cloth. By separating their different operations, and dividing them, with some order, between the different members of the family, they found, that more could be produced. But in the small compass of a single family, division of labour could not be carried far.

The next consideration was, could they get a neighbour to card or to spin for them, they might then be able to weave a still greater quantity. The attention of each being thus directed to fewer objects, they proceeded, imperceptibly, to improvements in the carding and spinning, by first introducing simple improvements in the hand instruments with which they performed these operations, till at length they arrived at a machine, which, though rude and ill constructed, enabled them to produce more in their respective families.

Here, then, commenced a great deviation from their former methods of proceeding; and invention and ingenuity found their reward, in the construction of machinery for carding and spinning.

But to return to the division of labour, and the small improvements made by the workers in the hand implements which they used. They found that, by these means, they could obtain more of the comforts of life and have more time to amuse themselves. Thus they were induced to go on, till their cottages were filled with their little improvements, and they were, in some measure, forced out of their dwellings by the multiplication of their implements.

Here commences the factory system, and the dividing one branch of the trade into two distinct parts (carding and spinning). The first of these improvements was in the carding, by means of which one boy or girl could work two pairs of stock-cards, so as to produce more than they did formerly. This continued for a short period when further improvements followed, until one person could work four or five pairs, by holding hand-cards against stock-cards fixed to a cylinder revolving on its axis, now called a *carding-machine*; the inventor of which we have no account of. It was, however, partially in use, in this rude state, about 60 years ago.

About ten years afterwards, this was followed by another machine, called the *Spinning Jenny*, invented (in 1767) by Mr. Hargreaves, of Blackburn; by means of which a young person could work 10 or 20 spindles at once.

It must be admitted, that these discoveries were followed by some encrease of dissipation, on the part of the people employed about them. Some finding themselves raised by their ingenuity above their neighbours, naturally exulted a little when they were conscious of having done something that had not been done before, and they frequently betook themselves to the ale-house to talk over their various achievements. Some of the moral and domestic duties were of course neglected. Both sexes being employed in the manufactory, they were both affected in some degree, by the consequences of the changes which had taken place. It could hardly be expected indeed, that they could be effectually restrained by moral motives, composed as they were of the lowest order of the people; and being in the habit of seeing such restraints disregarded by many of their superiors in various ranks of society. Exercising however great labour and ingenuity in carrying into execution their various inventions, they soon found that if they could readily get a blacksmith's or a carpenter's assistance, they would be able to get their little apparatus more substantially made. This induced them to remove to villages, where such men were to be found, and receiving from them the assistance so much wanted, the improvements made more rapid progress. By having their implements more mechanically constructed they soon discovered that they might be so far enlarged, that twenty pairs of hand-cards (adjusted by screws to a cylinder and taken off alternately by hand), might be wrought by one person, and forty spindles by another.

During all this time there was little to disturb the Continent of Europe. Their improvements could scarcely keep pace with the increased demand for manufactures, and of course there was but little diminution in their price.

In following up these important improvements, it was found that rotatory motion might be applied with advantage to almost all the new machines, and the turning of these separate machines by hand required, as they were multiplied, a great expenditure of human labour.

The next grand step, in the extension of the manufacture, was the application of the power of horses to this purpose. But this also soon found its limits, and water-falls were resorted to.

About this time the ingenious and enterprising manufacturer had to encounter the opposition of inveterate prejudices, and mistaken notions, in the higher, as well as in the lower ranks of society. The latter, under the apprehension of being deprived of their means of subsistence, broke out into the most outrageous excesses; wantonly destroying the machines, and threatening the lives of the inventors. The former, dreading the increase of poor-rates, took measures to prevent the extension, through the country, of the new methods of shortening labour. The excesses of the one, however, were repressed by the civil power; and the prejudices of the other were abated by the example of some intelligent men of landed property, who saw the advantages resulting to a country from an encreased productive population, as well as from the encouragement afforded to agriculture, by the encreased consumption of the products of the land.

But in resorting to water-falls to avail themselves of their power, the manufacturers were again removed from the experienced workmen in wood and metal, as well as from their neighbours, whose families had become essential parts of their enlarged establishments.

In some instances men of landed property stepped forward, and joining the industrious manufacturers with their capital, colonized near the water-falls for their convenience.

Notwithstanding these aids, the principal manufacturers, being further removed from the markets where they used to dispose of their manufactures and purchase what they wanted, found much difficulty in procuring an increase of machinery, or in repairing that already in use.

Mean while the little manufacturers, who had remained in their cottages and villages, did not abandon their exertions; but, making the most of the means they had, by employing the power of the lame and the blind in turning their machinery, they also found the advantage of the division of labour. Being supplied in this way with power to a very limited extent, they found that children could perform some of the more delicate parts of their operations. These were the children of indigent people (already employed in a similar way), who had removed from different parts of the kingdom, often at the expence of their respective parishes, which were thus relieved from the charge of supporting them.

In the progress of these changes it was observed that much advantage was derived from the people associating together. Improvements in mechanism, and the means of putting these improvements into use, increased in proportion to the size of the village and its population.

The people, being placed in a new situation, having food better in quality and in greater abundance, and the means of increasing almost all their other comforts, began to feel their independence, acquired new wants, and endeavoured to gratify those wants, each according to his taste.

A desire for better dwellings, as well as a demand for a greater number of them, became general; and the land-owners found their advantage in supplying portions of land for building upon, at moderate chief rents.

This created a new demand for artificers in various branches, and these men felt their interest likewise in the encreasing manufactures. The artificers also improved rapidly in their respective branches and became very useful in the construction of machinery.

With the advance of wages their dissipation encreased; yet, with their encreased dexterity, the produce of their labour was much greater in the same time, and much better in quality, than formerly. By degrees, a higher class of mechanics, such as watch and clock-makers, white-smiths, and mathematical instrument-makers began to be wanted; and, in a short time, a wide field was opened for the application of their more accurate and scientific mechanism. These workmen were first chiefly employed in constructing the valuable machines invented by Mr. Arkwright. A description of these machines would extend this paper beyond my present intention. It is but justice however to observe, that Mr. Arkwright's inventions introduced principles entirely new, into both carding and spinning (now commonly called perpetual carding and spinning), by first disengaging, then laying, the fibres of the cotton parallel to each other, and disposing them much more uniformly in the thread than it was possible to dispose them by any of the methods previously in use. At that period, millwrights, as well as the superior workmen above mentioned, were more generally employed in the establishments for spinning cotton than formerly, and a new stimulus was given to almost all kinds of manufactories then in the country, extending their circle wider and wider, and encreasing the general demand for labourers, until it became very difficult to find a supply for the extensive works erecting. Any common observer might easily perceive the effect of this great demand for labour on the morals of those employed. Having lost their attachment to rural employment and the avocations they had left, and being united by slender ties to their new employers, they became unsettled, and more indifferent than formerly to the good opinion of their neighbours; and consequently became less respected by them. Every one had it more in his power to consult his own inclinations as to the occupation he should follow; but all were compelled to exert themselves to procure a livelihood, having no claims on their employers unless they did so. Although their morals were in some degree impaired, their industry was accompanied with an activity of mind, and a dexterity of workmanship, which are seldom found in the more solitary employment of agriculture, or domestic avocations; and which tended in no small degree to promote the general improvements of our manufactures.

Those who have been in the habit of superintending their labour, have noticed, that the observations of even the least of the children, have often led to important improvements, by pointing out what to them was an inconvenience.

Sometimes the children themselves succeeded in applying the remedy. More frequently they prompted others of greater experience to find it out; and it is remarkable that these improvements tended, almost invariably, to simplify the machines about which the children were employed.

During a period of ten or fifteen years after Mr. Arkwright's first mill was built (in 1771) at Cromford, all the principal works were erected on the falls of considerable rivers; no other power than water having then been found practically useful. There were a few exceptions where Newcomen's and Savary's steam-engines were tried. But the principles of these machines being defective, and their construction bad, the expence in fuel was great, and the loss occasioned by frequent stoppages was ruinous.

In the year 1780, a new and valuable machine appeared, called, at that time, the *Hall-in-the-Wood machine*, from the name of the place where the inventor, Mr. Samuel Crompton lived, near Bolton in Lancashire. It is now called the *mule*, from its uniting the principles of Mr. Hargreave's jenny, and Mr. Arkwright's water frame. This machine, by producing, at a small expence, much finer and softer yarn than any that had ever been seen before, gave birth to a new and most extensive trade. The mule commenced much like the jenny, and its operations were carried on for many years in the country, and in private families, but without the benefit of Mr. Arkwright's patent machinery for carding and roving, which required constant and regular motion.

The want of regular power, and of the skilful mechanics already mentioned, soon brought the mules to the neighbourhood of towns; and about this time (1790) Mr. Watt's steam-engine began to be understood and introduced into this part of the kingdom, and it was applied to the turning of these various machines. In consequence of this, waterfalls became of less value; and instead of carrying the people to the power, it was found preferable to place the power amongst the people, wherever it was most wanted.

The introduction of this admirable machine imparted new life to the cotton trade. Its inexhaustible power, and uniform regularity of motion, supplied what was most urgently wanted at the time; and the scientific principles and excellent workmanship, displayed in its construction, led those who were interested in this trade to make many and great improvements in their machines and apparatus for bleaching, dying and printing, as well as for spinning*. Had it not been for this new accession of power and scientific mechanism, the cotton trade would have been stunted in its growth, and, compared with its present state, must have become an object only of minor importance in a national point

* The operation of bleaching had continued for centuries without any material improvement, until Scheele discovered the oxymuriatic acid and its effects, and Berthollet applied it successfully (in 1786) to accelerate the process of bleaching. Mr. Watt first introduced this great improvement into this Island; and our venerable President, Mr. Henry, was one of the first who carried it into practice, and gave to some of the principal bleachers in the country the first instruction they received respecting the new process.

of view. And, I believe, the effects of the steam-engine have been nearly the same in the iron, woollen, and flax trades.

Before the year 1790, the mules were turned by hand, and were confined chiefly to the garrets of cottages.—About that time Mr. Kelley of Lanark first turned them by machinery. The application of the steam engine to this purpose, produced another great change in this branch of the trade.—The mules were removed from the cottages to factories, were constructed more substantially and upon better mechanical principles, and produced yarn of a more uniform quality and at less expence.

The fine fabrics, made of the yarn spun upon mules, surpassed in beauty and cheapness every thing that had been produced before, and the demand for them was consequently great. Larger establishments were erected, and order, system, and cleanliness in their arrangement and management, became more necessary and more generally cultivated.

This has been attended with good effects on the habits of the people.—Being obliged to be more regular in their attendance at their work, they became more orderly in their conduct, spent less time at the ale-house, and lived better at home. For some years they have been gradually improving in their domestic comforts and conveniences.

In the year 1797 a new machine for cleaning cotton was constructed by Mr. Snodgrass, and first used at Johnston near Paisley by Messrs. Houston and Co.

This is called a scutching or blowing machine.—Its merits were but little known till 1808 or 1809, when it was introduced into Manchester*. It is now generally adopted for cleaning cotton.—The labor of that operation, formerly performed by women, in a most fatiguing manner, and always considered as degrading; has been reduced by this machine to about one twentieth of what it used to be.

Weaving looms moved by machinery, or what are called *power-looms*, are now become objects of considerable importance. These were constructed with great ingenuity, and with some prospect of success, so early as the year 1774, by Dr. Cartwright at Doncaster. Dr. Cartwright's looms made good cloth; but so much time was lost in dressing the warp in the loom, that they on the whole possessed

* Mr. Arkwright and Messrs. Strutts have added most useful improvements to this machine.

no important advantage over the common looms.

In 1803, Mr. Thomas Johnson, of Bradbury, Cheshire, invented a beautiful and excellent machine for warping and dressing warps, preparatory to weaving; by which this operation is performed much better and cheaper than it can possibly be done by hand. It is now known by the name of Ratcliff's Dressing Machine, from the unremitting exertions of that gentleman to have it made effective. This is a great advantage to power-looms, for without it they never could have been made practically useful, and, during the last 10 years, some large manufactories of this kind have been established, first in Scotland and afterwards in England. It is found however, that one person can not attend upon more than two power-looms, and it is still problematical whether this saving of labor counterbalances the expence of power and machinery, and the disadvantage of being obliged to keep an establishment of power-looms constantly at work; whilst, in the common way, the looms might be stopped, or turned to a different kind of weaving, if the demand for the particular kind of goods they were weaving should change or fall off.

Several improvements in the construction of power-looms have lately been brought forward, and some of them appear to have important advantages over any other construction that has hitherto been in use. Their real value however can be determined only by time and experience.

It should be recollected that thousands of ingenious contrivances have been tried and laid aside, before spinning machines were brought to their present state of perfection. To give more particular descriptions of them, or of the operations performed by them, would extend this paper beyond its proper limits. I may observe however that their united effects amount to this, that the labor of one person, aided by them, can now produce as much yarn, in a given time, as 200 could have produced 50 years ago.

By these means, cotton fabrics have been produced, of such variety and usefulness, as to be sought after by the people of every climate, and of every stage of civilization.

I have not been able to obtain any information respecting the circumstances that first led to the establishment of the cotton manufactory in this part of the island. After it was once commenced, however, its extension in this quarter appears to have been promoted by various circumstances. The abundance of excellent fuel, could not fail to encourage it.—The humidity of the climate, and the unfitness of the soil for agriculture, would induce the inhabitants to seek, in preference, for employment within doors.—But I believe the rapid growth and extension of the trade, in this particular district, is chiefly to be ascribed to the great ingenuity and the persevering, skilful, laborious disposition of the people. In these qualities I believe they surpass the inhabitants of every other part of this island, or of the whole world.

We have the satisfaction of observing also, that they are gradually becoming better informed, and more regular in their conduct. Their employers see the advantage of this, and many of them take great pains to promote the welfare of the people, and the education of their families.—The people themselves begin to take a pride in this, and value themselves on the proficiency of their children in education.

Emulation of this kind has been known to produce the best effects in other countries, and we may, in a few years, hope to reap the fruits of it amongst ourselves.

Having taken some pains to ascertain the present state of the cotton manufactory, as far as regards the spinning of the article, I apprehend it may be acceptable to have the result subjoined.

In the year 1817, from authentic documents and the best estimates I could draw from them, the quantity of raw cotton consumed or converted into yarn, in Great Britain and Ireland was.......... 110.000.000lbs.

Loss in spinning, estimated 1½ oz. per lb.	10.312.500lbs.
Quantity of yarn produced	99.687.500lbs.
Number of hanks (supposing the average to be 40 per lb.)....	3.987.500.000
Number of spindles employed, (each spindle being supposed to produce 2 hanks per day, and 300 working days in the year.)................	6.645.833
Number of persons employed in spinning, (supposing each to produce 120 hanks per day.).....	110.763
Number of horses' power employed, (supposing 4½ oz. of coal to produce 1 hank of No. 40, and 180 lbs. of coal per day, equal to 1 horse's power.)......	20.768

NOTE.

SINCE the foregoing paper was sent to the press, the following information has come to my hand. See a letter in the Repertory of Arts for December 1817, entitled "The origin of Cotton Spinning," from Mr. Charles Wyatt to his brother; it comes in a very creditable shape, and is borne out, in many respects, by facts and circumstances which have partly come under my own knowledge. The following is an extract.

"The brief history of the invention, which my superior "years, and the circumstance of my being in possession "of his papers and memorandums on the subject, gives me "an advantage over you, as far as I am able to trace it, is "this; In the year 1730, or thereabouts, living then at a "village near Litchfield, our respected father first conceived "the project, and prepared to carry it into effect; and in "the year 1733, by a model of about two feet square, in "a small building near Sutton Coldfield, without a single "witness to the performance, was spun the first thread of "cotton ever produced without the intervention of the hu-"man fingers, he, the inventor, to use his own words, "*being all the time in a pleasing but trembling surprise.*" "The wool had been carded in the common way, and was "passed between two pair of cylinders, from whence the "bobbin drew it, by means of the twist.

"This successful experiment induced him to seek for a "pecuniary connection equal to the views that the project "excited; and one appeared to present itself with a Mr. "Lewis Paul, which terminated unhappily for the projec-"tor; for Paul, a foreigner, poor and enterprizing, made "offers and bargains which he never fulfilled, and contrived "in the year 1738 to have a patent taken out in his own name "for some additional apparatus, a copy of which I send "you; and in 1741 or 42, a mill, turned by two asses "walking round an axis, was erected in Birmingham, and "ten girls were employed in attending the work. Two "hanks of the cotton then and there spun, are now in my "possession, accompanied with the inventor's own testi-"mony of the performance. Drawings of the machinery "were sent, or appear to have been sent, to Mr. Cave, for "insertion in the Gentleman's Magazine.

"This establishment, unsupported by sufficient property, "languished a short time and then expired: the supplies "were exhausted, and the inventor much injured by the "experiment, but his confidence in the scheme was unim-"paired. The machinery was sold in 1743. A work upon "a larger scale on a stream of water was established at "Northampton, under the direction of a Mr. Yeoman, but "with the property of Mr. Cave. The work contained 250 "spindles, and employed 50 pair of hands. The inventor "soon after examined the state of the undertaking, and "found great deficiency and neglect in the management. "At that time they had spun about 3300 pounds of cotton. "On the observations which he then made, he composed "what he entitled "A Systematick Essay on the business of "Spinning," which exhibits a clear view of the mechanical "considerations on which an undertaking of that nature, of "whatever magnitude, must be established, and apparently "confines his humble pretensions to the profit on 300 spin-"dles. It was not within human foresight to calculate the "richness of the harvest to come from this little germ."

The abilities of Mr. J. Wyatt as an ingenious Mechanic, were unquestionable; and his various inventions are still well known in the neighbourhood of Birmingham, particularly in the ingenious implements in that Town's Manufactures. The circumstance of Paul's patent for the carding machine, is a strong corroborative proof of the existence of the little establishment at Sutton-Coldfield, and afterwards in Birmingham, and last of all at Northampton, where report says the Carding Machine had its origin, but it does not assign any date, or give the name of the Inventor. This uncertainty appears to be removed by the publication of the above letter. The author of it, Mr. Wyatt, was so obliging as to send me a specimen of yarn spun about the year 1741 by a machine invented and erected by the late Mr. J. Wyatt of Birmingham, as well as a memorandum book of his, containing calculations on the twisting and other processes relating to yarn.

From examining the yarn I think it would not be said by competent judges that it was spun by a similar machine to that of Mr. Arkwright; for, the fabric or thread is very different from the early productions of Mr. Arkwright, and is, I think, evidently spun by a different machine, the ingenuity of which we can not appreciate, as the model mentioned in the paper alluded to is unfortunately lost.

With respect to originality of invention, it always has been and always will be difficult to give decided proof.

Two persons may invent machines, acting upon very different principles, the design of which is to produce the same effect. But the circumstances of time and place contribute no little to give an extraordinary stimulus to genius and a perseverance to overcome those obstacles which are in the way of a first effort to obtain some desirable object. Had Mr. Wyatt's machine been introduced into Lancashire or Nottinghamshire, where the product of a spinning machine was so urgently wanted, it is very probable that with his own ingenuity and the exertions of those wanting the thread, a Spinning Machine might have been introduced into general use at a much earlier period than that of Mr. Arkwright's. Yet taking all the circumstances together, and the lapse of nearly 30 years, it is probable Mr. Arkwright derived little more from Mr. Wyatt's or Mr. Paul's inventions than the knowledge that machines for such purposes had been invented, and might think there was opened a fair field for the exercise of his talents in the extension and improvement of the principle.

MEMOIR

ON A NEW SYSTEM OF

COG OR TOOTHED WHEELS.

BY

MR. JAMES WHITE,*

ENGINEER.

COMMUNICATED BY T. JARROLD, M. D.

(Read Dec. 29th, 1815.)

THE subject of this paper, though merely of a mechanical nature, cannot fail to interest the Philosophical Society of a town like Manchester, so eminently distinguished for the practice of mechanical science; unless as I fear may be the case, my want of sufficient theoretic knowledge or of perspicuity in the explication, should render my communication not completely intelligible. To be convinced of the importance of the subject, we need only reflect on the vast number of toothed wheels that are daily revolving in this active and populous district, and on the share which

* N. B. A patent was taken out for the Invention some years ago.

they take in the quantity and value of its productions; and it is obvions that any invention tending to divest these instruments of their imperfections, whether it be by lessening their expense, prolonging their duration, or diminishing their friction, must have a beneficial influence on the general prosperity. Now I apprehend that all these ends will be obtained in a greater or less degree, by having wheels formed upon the new system.

I shall not content myself by proving the above theoretically, but shall present the society with wheels, the nature of which is to turn each other in *perfect silence*, while the friction and wear of their teeth, if any exist, are so small as to elude computation, and which communicate the greatest known velocity without shaking, and by a steady and uniform pressure.

Before I proceed to the particular description of my own wheels, I shall point out one striking defect of the system now in use, without reverting to the period when mechanical tools and operations were greatly inferior to those of modern times. Practical mechanics of late, especially in Britain, have accidentally hit upon better forms and proportions for wheels than were formerly used; whilst the theoretic mechanic, from the time of De la Hire (about a century ago,) have uniformly taught that the true form of the teeth of wheels depends upon the curve called an epicycloid, and that of teeth destined to work in a straight rack depends upon the simple cycloid. The cycloid is a curve which may be formed by the trace of a nail in the circumference of a cart wheel, during the period of one revolution of the wheel, or from the nail's leaving the ground to its return; and the epicycloid is a curve that may be formed by the trace of a nail, in the circumference of a wheel, which wheel rolls (without sliding) along the circumference of another wheel.

Let AB (fig. 1.) be part of the circumference of a wheel ABF to which it is designed to adapt teeth, so formed as to produce equable motion in the wheel C, when that of the wheel ABF is also equable. Also, let the teeth so formed, act upon the indefinitely small pirs, *r*, *i*, *t*, let into the plane of the wheel C, near its circumference. To give the teeth of the wheel ABF a proper form (according to the present prevailing system), a style or pencil may be fixed in the circumference of a circle D equal to the wheel C, and a paper may be placed behind both circles, on which by the rolling of the circle D on

AB, will be traced the epicycloid *d, e, f, g, s, h*, of which the circle ABF is called the base, and D the generating circle. Thus then the wheel to which the teeth are to belong is the base of the curve, and the wheel to be acted upon is the generating circle; but it must be understood that those wheels are not estimated in this description at their extreme diameters, but at a distance from their circumferences sufficient to admit of the necessary penetration of the teeth; or, as M. Camus terms it, where the *primitive circles* of the wheels touch each other, which is in what is called in this country the *pitch line.*

Now it has been long demonstrated by mathematicians, that teeth constructed as above would impart equable motion to wheels, supposing the pins *r, i, t,* &c. indefinitely small. This point therefore need not be farther insisted upon.

So far the theoretic view is clear; but when we come to practice, the pins *r, i, t,* previously conceived to be indefinitely small, must have *strength,* and consequently a considerable *diameter,* as represented at 1, 2; hence we must take away from the area of the curve a breadth as at *v* and *n* = to the semidiameter of the pins, and then equable motion will continue to be produced as before. But it is known to mathematicians that the curve so modified will no longer be strictly an epicycloid; and it was on this account that I was careful above, to say that the teeth of wheels producing equable motion, *depended* upon that curve; for if the curve of the teeth be a true epicycloid in the case of thick pins, the motion of the wheels will not be equable.

I purposely omit other interesting circumstances in the application of this beautiful curve to rotatory motion; a curve by which I acknowledge that equable motions can be produced, when the teeth of the ordinary geering are made in this manner. But here is the misfortune:—besides the difficulty of executing teeth in the true theoretical form, (which indeed is seldom attempted), *this form cannot continue to exist*; and hence it is that the best, the most silent geering becomes at last imperfect, noisy and destructive of the machinery, and especially injurious to its more delicate operations.

The cause of this progressive deterioration may be thus explained: Referring again to fig. 1, we there see the base of the curve AB divided into the equal parts, *ab*, *bc*, and *cd*; and observing the passage of the generating circle D, from the origin of the curve at *d*, to the first division *c* on the base, we shall find no more than the small portion *de*, of the curve developed, whereas a second equal step of the generating circle *cb*, will extend the curve forward from *e* to *f*, a greater distance than the former; while a third equal step *ab*, will extend the curve from *f* to *g*, a distance greater than the last; and the successive increments of the curve will be still greater, as it approaches its summit; yet all these parts correspond to equal advances of the wheel, namely, to the equal parts *ab*, *bc* and *cd* of the base, and to equal ones of rotation of the generating circle. Surely then the parts *sg*, *gf*, of the epicycloidal tooth will be *worn out* sooner than those *fe*, *ed*, which are rubbed with so much less velocity than the other, even though the *pressure* were the same. But the pressure is not the same. For, the line *ag* is the direction in which the pressure of the curve acts at the point *g*, and the line *p q* is the length of the lever-arm on which that pressure acts, to turn the generating circle on its axis (now supposed to be fixt;) but, as the turning force or rotatory efforts of the wheels, is by hypothesis uniform, the pressure at *g* must be inversely as *pq*; that is, inversely as the cosine of half the angle of rotation of the generating circle; hence it would be infinite at *s*, the summit of the curve, when this circle has made a semi-revolution.

Thus it appears that independently of the effects of percussion, the *end* of an epicycloidal tooth must *wear out* sooner than any part nearer its base, (and if so, much more may it be supposed of a tooth of another form ;) and that when its form is thus changed, the advantage it gave must cease, since nothing in the working of the wheel can afterwards restore the form, or remedy the growing evil.

Having now shewn one great defect in the common system of wheels, I shall proceed to develope the principles of the new system, which may be understood through the medium of the three following propositions.

1. The action of a wheel of the new kind on another with which it works or *geers* is the same at every moment of its revolution, so that the least possible motion of the circumference of one, generates an exactly equal and similar motion in that of the other.

2. There are but two points, one in each wheel, that necessarily touch each other at the same time, and their contact will always take place indefinitely near the plane that passes through the two axes of the wheels, if the diameters of the latter, at the useful or

pressing points are in the exact ratio of their number of teeth respectively; in which case there will be no sensible friction between the points in contact.

3. In consequence of the properties above-mentioned, the epicycloidal or any other form of the teeth, is no longer indispensable; but many different forms may be used, without disturbing the principle of equable motion.

With regard to the demonstration of the first proposition, I must premise an observation of M. Camus on this subject, in his Mechanics, 3d part, page 306, viz. "if all wheels could have teeth infinitely fine, their *geering*, which might then be considered as a simple contact, would have the property required [that of acting uniformly,] since we have seen that a wheel and a pinion have the same *tangential* force, when the motion of one is communicated to the other, by an infinitely small penetration of the particles of their respective circumferences."

Now suppose that on the cylindrical surface of a spur wheel B *c* (Fig. 3), we cut oblique or rather *screw formed teeth*, of which two are shewn at *ac*, *bd*, so inclined to the plane of the wheel, as that the end *c* of the tooth *ac* may not pass the plane of the axes AB*c*, until the end *b* of the other tooth *bd* has arrived at it, this wheel will virtually be divided into an infinite number of teeth, or at least into a number greater than that of the particles of matter, contained in a circular line of the wheel's circumference. For suppose the surface of a similar, but longer cylinder, stripped from it and stretched on the plane ABCE (fig. 4,) where the former oblique line will become the hypothenuse BC, of the right angled triangle CAB, and will represent *all* the teeth of the given wheel, according to the sketch EG at the bottom of the diagram. Here the lines AB and CE, are equal to the circumference of the base of the cylinder, and AC and BE to its length; and if between A and B, there exist a number, m, of particles of matter, and between A and C a number, n, the whole surface ABCE will contain mn particles, or the product of m and n; and the line BC, will contain a number $= \sqrt{m^2+n^2}$, from a well known theorem; whence it appears that the line BC is necessarily longer than AB, and hence contains more particles of matter.*

* It need hardly be observed, that whatever is true of the whole triangle CAB (fig 4,) is true of every similar part of it, be it ever so small: and in fact, when the hypothenuse BC, is folded again round the cylinder, from which we have supposed it stripped, the acting part will

It is besides evident, that the difference between the lines BC and AB, depends on the angle ACB; in the choice of which, there is a considerable latitude. For general use however, I have chosen an angle of obliquity of 15°, which I shall now assume as the basis of the following calculations. The tangent of 15°, per tables, is in round numbers 268 to radius 1000; and the object now is to find the number of particles in the oblique line BC, when the line AB, contains any other number, *t*.

By geometry, BC $(x) = \sqrt{r^2 + t^2} = \sqrt{\overline{1000}|^2 + \overline{268})^2} = 1035$ nearly; and this last number is to 268, as the number of particles in the oblique line BC is to the number contained in the circumference AB, of the base of the cylinder. Hence it appears, that a wheel cut into teeth of this form, contains (virtually) about four times as many teeth, as a wheel of the same diameter, but indefinitely thin, would contain. And the disproportion might be increased, by adopting a smaller angle.

be very small indeed; but it will still act in the way here described, and give tendencies to the wheel it acts on, and to its axis precisely proportionate, to the quantities here mentioned.

Thus I apprehend it is proved, that the action of a wheel of this kind, on another with which it geers, is perfectly uniform in respect of swiftness; and hence the proof that it is likewise so, as to the force communicated.

Before I proceed to the second proposition, I ought perhaps to anticipate some objections that have been made to this system of geering, and which may have already occurred to some gentlemen present. For example, it has been supposed that the *friction* of these teeth, is augmented by their inclination to the plane of the wheel; but I dare presume to have already proved, that it is this very obliquity, joined to the total absence of motion in direction of the axes, that *destroys* the friction, instead of *creating* it. I acknowledge however, that the *pressure* on the points of contact, is greater than it would be on teeth, parallel to the axes of the wheels, and I farther concede that this pressure tends to displace the wheels in the direction of the axes, (unless this tendency is destroyed by a tooth, with two opposite inclinations.) But supposing this counteraction neglected, let us ascertain the importance of these objections. First, with regard to the increase of pressure on the point D on the line BC, (representing the oblique tooth in question,) relative to that which would be on the line BE, (which represents a tooth of common geering:) let AD be drawn perpendicular to BC. If the point D can slide freely on the line BC, (and this is the most favourable supposition for the objection,) its pressure will be exerted perpendicularly to this line; and if the point A, moves from A to B, the point D, leaving at the same moment the point A, and moving in direction AD, will only arrive at D in the same time, its motion having been slower than that of A, in proportion of AB to AD; whence by the principle of virtual velocities, its pressure on BC is to that on AC, as the said lines AB to DA.

To convert these pressures into numbers, according to the above data; we have AC $= 1000$, AB $= 268$, BC $= 1035$; then from the similar triangles BAC, BDA, it will be BC : AC :: AB : AD $= \frac{268000}{1035} = 259$ nearly. Therefore the pressure on BC, is to that on AC, as 268 to 259, or as 1035 : 1000.

To find what part of the force tends to drive the point B, in the direction BE, (for this is what impels the wheels, in the direction of their axes, we may consider the triangle BAC as an inclined plane, of which BC is the length, and AB the height; and the total pressure on CB, which may be represented by

CB (1035,) may be resolved into two others, namely, AB and AC, which will represent the pressures on those lines respectively, (268 and 1000.) Hence the pressure on BC, is augmented only in the ratio of 1035 to 1000, or about $\frac{1}{29}$ part by the obliquity ; and the tendency of the wheels to move in the direction of their axes, when this angle is used,) is the $\frac{268}{1000}$ of the original stress, that is, rather more than one quarter. But since the longitudinal motion of an axis can be prevented by a point almost invisible applied to its centre, it follows that the effect of this tendency can be annulled, without any sensible loss of the active power. It may be added, that in vertical axes, these circumstances lose all their importance, since whatever force tends to *depress* the one and increase its friction, tends equally to *elevate* the other, and relieve its step of its load ; a case that would be made eminently useful, by throwing a larger portion of the pressure on the *slow-moving* axes, and taking it off from the more rapid ones.

We now proceed to the second proposition. The truth of the assertions, contained in this proposition, must, I should suppose, be evident, from the consideration of two circles touching each other, and at the point of contact, coinciding with their common tangent at that point. Let A and B be two circles, tangent to each other (Fig. 3) in *e*. AC is the line joining the centres, and DF the common tangent of the circles at *e*; which is at right angles with AC; and so are the circumferences of the two circles at the point *e*. For the circles and tangent coincide for the moment. Hence then I conclude, 1st, that a motion (evanescently small) of the point common to the three lines, can take place without quitting the tangent DF: and 2d, that if there is an infinite number of teeth in these circles, those which are found in the line of the centres, will *geer* together in preference to those which are out of it, since the latter have the common tangent, and an interval of space between them.

The truth of this proposition (or an indefinite approximation to truth,) may be deduced from the supposition that the two circles do *actually* penetrate each other. To this end let AB, *ab*, in fig. 5, be two equal circles, placed parallel to each other in two contiguous planes, so as for one to hide the other, in the indefinitely small curvilinear space *d f e g*. I say that if the arc *d g* is indefinitely small, the rotation of the two circles will occasion no more friction between the touching surfaces, *g e f* and *f d g*, than there would be between the two circles placed in the same plane, and touching at the point n the same common tangent.

For draw the lines DE, fd, dg, gf, ge and gD; and adverting to the known equation of the circle, let $dn = x$, $gn = y$ and $Dg = a$, the absciss, ordinate and radius of the circle; we have $2ax - x^2 = y^2$. From this equation we obtain $a = \frac{y^2 + x^2}{2x}$, the denominator of this fraction ($2x$) being the width, de, of the touching surfaces, fdg, and feg of the two circles. But the numerator ($y^2 + x^2$) is equal to the square of the chord $g\ d$ of the angle E D g, which chord I shall call z; then we have $a = \frac{z^2}{2x}$; from which equation we derive this proportion, $a : z :: z : 2x = \frac{z^2}{a}$. But in very small angles, the sines are taken for the arcs without sensible error; and with greater reason may the chords; if then we suppose the arc dg, or the chord z, indefinitely small, we shall find the line $de = 2x = \frac{z^2}{a}$, indefinitely smaller; that is, of an order of infinitessimals one degree lower; for it is well known that the square of evanescent quantities, are indefinitely smaller than the quantities themselves. And to apply this, if the chord z represent the circular distance of two particles of matter found in the screw-formed tooth *ac*, of the wheel B*c*, fig. 3, (referred to the circle *ab*, fig. 5), that distance *z* will be a mean proportional between the radius D*g* of such wheel, and the double versed sine of this inconceivably small angle.*

I am aware that some mathematicians maintain, that the smallest portion of a curve cannot strictly coincide with a right line; a doctrine which I am not going to impugn. But however this may be, it appears certain that there is no such mathematical curve exhibited in the material world, but only polygons of a greater or less number of sides, according to the density of the various substances, that fall under our observation. I shall therefore proceed to apply the foregoing theory, not indeed to the ultimate particles of matter, (because I do not know their dimensions,) but to those real particles which have been actually measured. Thus, experimental phi-

* I ought perhaps to have introduced this reasoning on the 5th figure by observing, that every projection of every part of a screw, on a plane at right angles with the axis of such screw, is a circle; and that therefore the chord *z*, or the line *gd*, is the true projection of a proportionate part of any line, BC, fig. 4, when wrapped round a cylinder of equal diameter, with the circle *ab*, fig. 5.

losophy shews, that a cube of gold of $\frac{1}{2}$ inch side, may be drawn upon silver to a length of 1442623 feet, and afterwards flattened to a breadth of $\frac{1}{100}$ of an inch, the two sides of which form a breadth of $\frac{1}{50}$ of an inch: so that if we divide the above length by 25, we shall have the length of a similar ribbon of metal of $\frac{1}{2}$ an inch in breadth, namely, 47704 feet; which cut into lengths of $\frac{1}{2}$ of an inch (or multiplied by 24, the half inches in a foot,) give 1144896 such squares, which must constitute the number of lamina of a half inch cube of gold, or 2289792 for an inch thickness. Let us suppose then a wheel of gold, of two feet in diameter, the friction of whose teeth it is proposed to determine. We must first seek what number of particles are contained in that part of the tooth or teeth, that are found in one inch of the wheel's circumference; this we have just seen to be 2289792 thicknesses of the leaves, or diameters of the particles, such as we are now contemplating,

We shall now have this proportion (See Fig. 4,) 268 (AB) : 1035 (BC) :: 2289792 (no of particles in one inch of circumference of base) : $x = 8843040$ particles in that part of the line BC, which corresponds with *that* inch of the circumference. Thus each of the latter particles measured in the direction AB, is equal to the fraction $\frac{1}{8843040}$ths of an inch. And if that fraction be taken for the arc *gd* (fig. 5), then to find the length of the line *de*, (on which the friction of *this* and all other geering depends,) we must use this analogy; 12 inch. (rad. of wheel) : $\frac{1}{8843040}$ of an inch (chord *gd*) :: $\frac{1}{8843040}$ of an inch (*gd*) : *de*, the line required $= \frac{1}{938.392.277.299.200}$ of an inch. This result is still short of the truth, as we do not know how much smaller the ultimate molecules of gold are.

To advert now to some of the practical effects of this system, I would beg leave to present a *form* of the teeth, the sole working of which would be a sufficient demonstration of the truth of the foregoing theory. A, B, (fig. 6) are two wheels of which the primitive circles or pitch-lines touch each other at *o*. As all the homologous points of any screw-formed tooth, are at the same distance from the centres of their wheels, I am at liberty to give the teeth a rhomboidal form, *o t i*; and if the angle *o* exists all round both wheels, (of which I have attempted graphically to give an idea at DG,) in this case, those particles only which exist in the plane of the tangents *fh*, &c. and infinitely near that plane passing at right angles to it, through the centres A and B, will touch each other; and there, as we have already proved, no sensible motion of the kind producing friction, exists between the points in actual contact. I might add, as the figure evidently indicates, that if any such motion did exist, the angles *o* would quit each other, and the figure of such teeth become absurd in practice; but on the other hand, if such teeth can exist and work usefully (which I assert they can, nay that all teeth have in this system a tendency to assume that form at the working points;) this circumstance is of itself a practical evidence of the truth of the foregoing theory, and of what I have said concerning it.

It must have been perceived that I have in some degree anticipated the demonstration of my third proposition, namely, that the epicycloidal or any other given form of the teeth, is not essential to this geering. It appears that teeth formed as epicycloids, will become more convex by working; since the base of the curve is the only point where they suffer no diminution by friction; whilst those of every other form, that likewise penetrate beyond the primitive circles of the wheels, will also assume a figure of the same nature, by the rounding off of their points, and the hollowing of the corresponding parts of the teeth they impel; and that operation will continue till an angle similar to that at *o* (fig. 6,) but generally more obtuse, prevails around both wheels; when all sensible change of figure or loss of matter will cease, as the wheels now before you will evince.

On the right of the drawing (fig. 6,) the teeth of the wheel B are angular (suppose square,) and those of the wheel C rounded off by any curves, *within* an epicycloid. All that is necessary to remark in this case is, that the teeth of the wheel B must not extend *beyond* its primitive circle, whilst the round parts of those of the wheel C, do more or less extend beyond its primitive circle; whence it becomes evident, that the contact of such teeth (if infinite in number,) can *only* take place in the plane of the common tangent at right angles to AB; also that if these teeth are sufficiently hard to withstand ordinary pressure. without indentation in these circumstances, there is no perceptible reason for a sensible change of form; since this contact only takes place where the two motions are alike, both in swiftness and direction. A fact I am going to mention may outweigh this reasoning in the minds of some, but cannot invalidate it. I caused two of these wheels made of brass, to be turned with rapidity under a considerable resistance for several weeks together, keeping them always anointed with *oil* and *emery*, one of the most destructive mixtures known for rubbing metals; but after this severe trial, the teeth of the wheels, *at their primitive circles* were found as entire as before the experiment. And why? Certainly for no other reason than that they worked without sensible friction.

Hitherto nothing has been said of wheels in the conical form, usually denominated *mitre and bevel geer*. But my models will prove, that they are both comprehended in the system. The only condition of this unity of principle is, that the axes of two wheels, instead of being *parallel* to each other, be always found in *the same plane*. With this condition every property above-mentioned, extends to this class of wheels which my methods of execution also include, as indeed they do every possible case of geering.

Being afraid of trespassing on the time of the society, I have suppressed a part of this paper, perhaps already too long; but I hope I may be indulged with a few remarks on the application of those wheels to practical purposes. And first, as to what I have myself seen; these wheels have been used in several important machines to which they have given much swiftness, softness or precision of motion as the case required. They have done more; they have given birth to machines of no small importance, that could not have existed without them. In rapid motions they do all that band or cord can perform, with the addition of mathematical exactness, and an important saving of power. In spinning factories these properties must be peculiarly interesting; and in calico-printing, the various delicate operations require great precision of motion. In clock-making also, this property is of great importance in regulating the action of the weight, and thus giving full scope to the equalizing principle whatever it be. I may add, it almost annuls the cause of anomaly in these machines, since a given clock will go with less than $\frac{1}{4}$ of the weight usually employed to move it. Another useful application may be mentioned; in flatting mills, where one roller is driven by a pinion from the other, there is a constant combat between the effort of the plate to pass equally through the rollers, and the action of the common geering, which is more or less convulsive. Whence the plate is *puckered*, and the resistance much increased, both which circumstances these wheels completely obviate; and many similar cases might be adduced.

I shall only add, that my ambition will be highly gratified if, through the approbation of this learned society, I may hope to contribute to the improvement and perfection of the manufactures of this county; and if the invention be found of general utility to my much loved country.

A

Fig. 1

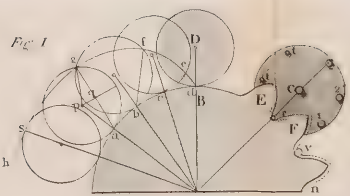


Fig. 3.

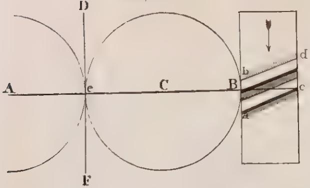


Fig. 4

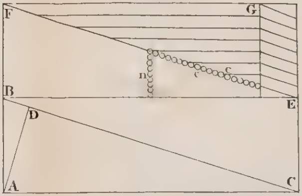


Fig. 5

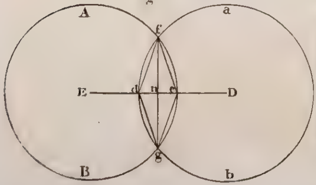
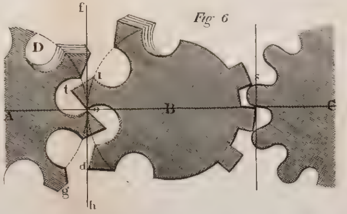


Fig. 6



ON THE

FLEXIBILITY

OF

ALL MINERAL SUBSTANCES;

AND THE CAUSE OF

CREEPS AND SEATS IN OLD COAL MINES.

BY

MR. JOHN B. LONGMIRE.

COMMUNICATED BY DR. HOLME.

(Read Feb. 9th, 1816.)

IT is well known, that a few mineral substances can be easily bent by the hands, and that they have, in consequence, been called flexible minerals. All minerals possessing this property are laminated, and those which have the greatest degree of flexibility, can be separated into the thinnest lamina: thus mica and talc are some of the most flexible bodies, and are very finely laminated.

Only a few minerals are flexible in hand specimens, but many do, and probably all will bend, when acted on in large tabular masses. The bending of strata is very familiar to coal-miners. When they make greater hollows than usual, the stratum immediately above the coal, gives way in a little time, in consequence of its being either pressed on by the incumbent matter, or, not being able, if only a thin stratum, to support its own weight. In both cases it bends downwards into the hollow, till it either separates at intervals into small layers, or falls all at once to the pavement. A stratum of sand-stone several yards thick, but divided into separate layers, will bend six feet in a hollow space, whose sides are ten by nine yards. This rate of bending, or degree of flexibility, may be reckoned more than that of the sand-stone strata in general, and more than that of the coaly strata; but it is less than that of the slate clay strata. The ratio of the flexibility of all strata however, is modified very much by the existence or want of the seams of upright distinct concretions: for if these seams are numerous, as soon as a stratum is bent a little, it falls to pieces. We have not an opportunity of determining if any strata or rocks but what belong to the coal formation, are flexible on the great scale, but I have seen some varieties of limestone, and primitive slate, which bend considerably, when in the act of being separated into small parts by wedges and other tools. Now the inference which may be drawn from these premises, is, that all kinds of mineral matter, however hard and brittle they appear to be in hand specimens, will bend, less or more, when formed into large flat pieces.

Very intimately connected with this subject, is the sinking of the strata above old coal works. In the common way of working coals, care is taken to get as much of the coal as is possible, and none is intended to be left, but under towns, villages, houses, canals, harbours, or rivers. If about two thirds of the coal be taken away, the remainder cannot support the incumbent matter, of course it sinks, squeezes the pillars into small coals, and forces them out sideways, till all the hollows are filled, and the coal ground becomes so firm as to support its burthen. When as much coal is left as will nearly bear up the roof-strata, the sinking takes place slowly, and the coaliers then say, the *creep* has come upon the mine. The weight forces pieces of coal from the pillar corners, and other weak places, and the pillars themselves appear to be forced into the pavement; they are then separated into smaller parts, which are soon crushed into small pieces, and forced out sideways till the hollows are filled with coal, and the matter of the pavement; lastly, it only remains for the pressure to squeeze the loose matter as firmly as it can. Now, by the time that the coal is separated from the weak parts of the pillars, the sinking of the roof-strata has become evident; and it continues till the stratum next above the coal, is only from one half to one third of the original height of the coal from the pavement. During this process, the first stratum above the coal, or if a thick one, the lower part of it, bends downwards till it is rent from that above it; but the coal pillars not being able to support the disengaged layers, it sinks, and leaves the second layer or stratum at liberty to bend; this layer now bends till it leaves the third, and adds to the weight that the coal pillars have to bear. In this way the separation of the strata goes on, while it can be heard in the hollows of the coal works: when for instance, one layer begins to separate, a small part only is detached at once, with a noise that coaliers call a *thud.* The thuds shift their situations at every repetition of sound: sometimes they move alternately, from dip to rise; they often spread in every direction from the weakest part; they occasionally commence at the middle, and extremities of the excavated ground at the same time, and meet half way; and they appear to proceed without any order, being heard in every part at the same time. When in full activity, they are heard every minute, sometimes two or three at a time, then a small pause takes place; very often they go off in irregular succession, but most frequently at irregular intervals. In this manner they may continue for a few days or till one layer is separated, then they will not be heard, or only heard sparingly for a day or two; they will then recommence, and be as active as ever. These alternations of activity and rest continue for a few weeks, and then the thuds cease or nearly cease for a few weeks; they will then begin again with greater force than before. I may remark, that the small cessations are in consequence of the bending of every stratum before it separates from that which is incumbent on it, and the larger cessations happen, when the pillars, having been squeezed out to a greater surface, are just capable of bearing the weight then upon them for a certain time; hence, the falling layers close up the space to the firm strata, and suspend their bending; but as the pillars very soon yield a little, room is again made for the then firm strata to bend, and to separate; therefore, the sinking recommences, and continues, with such like interruptions, till the pillars are squeezed out and fill the hollows. The layers after being once separated, cannot be forced again into their original space; hence the sinking has a limit, and if the distance between the coal and the surface be more than that to which the sinking can extend upwards, the surface strata are not acted on. But if the sinking do reach the surface, small hollows appear which slowly expand, walls separate into parts and fall down, and streams of water enter into small upright seats instead of continuing in their usual course. But the sinking acts upon houses in the most extraordinary manner: the windows break without any visible cause; the window stones crack; afterwards the house sides are rent; and, if the creep be strong at the surface, a part of, if not all, the house falls to the ground.

When the coal mine has been excavated very fast, and most of the coal taken away, a rapid sinking of the roof strata is sure to follow soon after. This sinking is called a set or seat by the coaliers, and differs only from the squeeze in closing up the hollows in a much shorter time. The whole area of the roof strata in a mine will sometimes sink down at once, but in general only parts of it sink at a time. In the last instance, the air is forced out of the part affected, and if it be mixed with inflammable air, as it often is in fiery coaleries, the most fatal accidents happen to the miners then in the pit; for the mixture is sure to reach some of their candles, and to produce a terrible explosion.

ACCOUNT OF THE

Black-lead Mine in Borrowdale,

CUMBERLAND.

BY

MR. JONATHAN OTLEY.

(Read Dec. 27th, 1816.)

To Mr. Dalton.

SIR,

THE following account of the Black-lead mine in Borrowdale contains some particulars not generally known: if you think it worthy the attention of the Literary and Philosophical Society, I shall be obliged by your communicating it.

I remain, Sir,

Your obedient Servant,

JONATHAN OTLEY.

OF the first discovery of this mine we have no account; but from a grant made in the beginning of the seventeenth century, it appears to have been known before that time. The manor of Borrowdale is said to have belonged to the abbey of Furness, and having, at the dissolution of that monastery, in the reign of

Henry the Eighth, fallen to the crown; it was by James the First granted to William Whitmore and Jonas Verdon; including among other things, *the Wad Holes and Wad, commonly called black cawke, of the yearly rent or value of fifteen shillings and fourpence.* The said William Whitmore and Jonas Verdon, by a deed bearing date the 28th day of November, 1614, sold unto Sir Wilfred Lawson, of Isel, and several inhabitants of Borrowdale, all the said manor of Borrowdale with the appurtenances of what nature or kind soever; "EXCEPT the wad holes and wad, commonly called black cawke, within the commons of Seatollor, or elsewhere within the commons and wastes of the said manor." In consequence of which reservation, the wad, or Black-lead mine, is held distinct from other royalties of the manor; one half thereof belonging to Henry Banks, Esq. M. P. and the other half subdivided in several shares.

I shall not enter into a discussion on the derivation or importance of the several names of wad, black-cawke, black-lead, plumbago and graphite, by which this mineral has been successively designated; but shall generally make use of the primitive monosyllable *wad*; which has at least brevity to recommend it. The mine is situated near the head of the valley of Borrowdale, in the side of a steep hill facing the South East, and has been opened in different places where the wad had probably appeared at the surface; it has only been worked at intervals, and when a sufficient quantity was procured to answer the demand for a few years, the mine was strongly closed up till the stock was reduced. Bishop Nicholson who visited this mine in 1710, says, in a letter to Dr. Woodward, that "on opening the old level in that year great discouragements appeared; for no search having been made in thirty two years, they found that some pilfering interlopers had carried it on till they had lost it in the rock: but after a few days trial, a new belly was happily discovered before the forehead of the old man, which proved so rich, that in less than twenty-four hours they had filled several sacks with fine and clean washed mineral."

On opening one of the old workings in 1769, it was found to have been carried to a great extent, without the help of gunpowder; and this vein being pursued to the depth of one hundred yards and upwards, much inconvenience was experienced in working it: to obviate which, in the year 1798, an adit or level was begun in the side of the hill, which, at the length of 220 yards, communicated with the bottom of the old workings. Through this level the water passes off, and the produce is brought out to be dressed, and on its mouth a house is built, where when the mine is open the overseers dwell, and the workmen are undressed and examined as they pass to and from work.

As the mountains in this neighbourhood are not composed of regular strata, so this mineral does not lie in a continued vein; but in pipes, or bellies, at a considerable distance from each other. This mountain consists principally of that kind of rock called *grey wackê*, a stratum of a darker coloured stone runs through it, containing more iron, the joints strongly tinged with oxide of iron; this is traversed in various directions by strings or small veins exhibiting traces of wad, and it is generally at the intersection of two of these veins that the valuable bellies are met with; in one of which, opened in 1803, upwards of 500 casks of the best quality were procured, containing about one hundred weight and a quarter each, besides a greater quantity of an inferior sort: since that time two of these bellies have been met with, which have produced about 100 casks each.

It comes from the mine in pieces of an irregular shape, and of various sizes; some weighing a few pounds, but the greater quantity in smaller pieces; it requires no smelting or refining, the pieces are only cleared from any stony or extraneous matter which may adhere to them, and assorted according to the different fineness and sizes; and thus sent up in casks to the warehouse in London, which is open on the first Monday in every month, for the retailing out to customers.

The great value of the wad, and the facilities afforded for disposing of it in an unmanufactured state, being strong temptations to pilfering, it has sometimes been necessary to keep a strong guard upon the place; and an act was passed, 25th Geo. 2d. cap. 10, by which an unlawful entering of any mine or wad hole, of wad, or black cawke, commonly called black-lead, or unlawfully taking or carrying away any wad, &c. from thence; as also the buying or receiving the same, knowing it to be unlawfully taken, is made felony. In the preamble to this act wad is stated to be "necessary for divers useful purposes, and more particularly in the casting of bomb shells, round shot, and cannon balls." However, its use in cleaning and glossing cast iron work, such as stoves, grates, &c. is now well known: and being capable of enduring a great heat, it is used in the manufacture of crucibles; it is also excellent for diminishing friction in wooden screws and other machinery; and it is probably an ingredient in most of the anti-attrition compositions: but its principal use is in pencils, the manufacturing of which is carried on at Keswick to a considerable extent, and has lately received great improvements: but although in the vicinity of the mine, the pencil makers here are obliged to send to London for all the wad they use; as the proprietors will not permit any to be sold till it has first been lodged in their own warehouse.

It has generally been used without any other preparation than merely cutting it with a saw to the scantlings required, and thus enclosing it in a suitable casing of cedar wood; but as it varies greatly in quality, both as to purity and hardness, considerable skill is required in the chusing, and assorting it, according to the different uses for which it is intended; and most of it being found too soft for some purposes, a method of hardening it, had long been a desideratum; and it has at length been accomplished. This has been a source of considerable emolument to those pencil makers who first possessed the art; they of course have endeavoured to keep it a secret, and it has not yet that I know of been published: but the most extraordinary fact is, that the method should remain so long undiscovered. Great improvement has also been made in the composition pencils; these are made of the saw dust, and of pieces too small for use in the common way; which, being ground to an impalpable powder, is mixed with some adhesive substance, and the mixture is sometimes in a soft state forced into the grove of the pencil: but a better kind, being made up with a cement of a more resinous nature, is formed in a mould, into square blocks, consolidated by a strong pressure, and then treated in the same manner as solid black lead: and this method is now brought to such a degree of perfection, that the pencils answer for some purposes as well as those made from the best black-lead, and can be afforded much cheaper; they can scarcely be distinguished by cutting, but by holding the point to the flame of a candle they are easily detected.

The specific gravity of the best wad, is to that of water as two to one nearly; the coarser is heavier as it contains more stony matter; black-lead is incapable of fusion, and was formerly thought to be incombustible: but it is found to be a carburet of iron, in the proportion of about nine parts carbon to one of iron; and being heated in a crucible, with nitre, or other substances affording a great quantity of oxygen, its texture is weakened, and it is finally decomposed, the carbon disappearing, and a little ochreous matter being left behind. By keeping it for some time immersed in melted sulphur, the black-lead becomes impregnated with sulphur; and being first reduced into slices of about one twentieth of an inch in thickness (the proper size for pencils), *it may thus be brought to the degree of hardness required.* Pencils treated in this way may be known by the sulphureous odour, and phosphorescent light, emitted on rubbing the point upon a moderately heated iron.

By an account drawn up in 1804, the stock then on hand was valued at £54,000, and the annual consumption about £3500; the best black-lead was then sold at 35s. per pound; since that time the price has been from 30 to 45 shillings, and no doubt the consumption has greatly increased. And this mine which 200 years ago, was valued at fifteen shillings and fourpence; has lately, on assessing the property tax, been estimated at two thousand seven hundred pounds sterling a year.

KESWICK,
Nov. 28, 1816.

ACCOUNT OF

A WHITE SOLAR RAINBOW.

BY

THE REV. R. SMETHURST.

COMMUNICATED BY MR. DALTON.

(Read Jan. 24th, 1817.)

ON the 28th of November, 1816, about 2 P. M. my attention was arrested by a Phænomenon new to me, a *white* Solar Rainbow. During the preceding part of the day, there had been a very dense fog, which at the time I observed the Phænomenon continued undiminished on the surface of the earth, though it seemed to be clearing away in the higher regions of the atmosphere. There were no visible drops of rain. The sun was visible through the fog, but its rays were not sufficiently powerful to occasion shadows of objects. When I first observed the Rainbow, the whole, with the exception of a small portion of the centre of the arch, was very dis tinctly defined. As I continued to observe it, which I did during at least five minutes, the fog seemed to rise, and the centre also became visible. I never saw a more perfect bow, nor one more distinctly defined in every part. Its apparent distance from me was about one hundred yards; and its span did not appear to exceed one hundred and twenty yards. Its breadth (apparently) exceeded that of any Rainbow I ever noticed: I should say it was double the breadth of rainbows in general, or nearly so. It was of a very greyish cast, similar in appearance to a lunar rainbow. It never assumed any of the usual colours of the rainbow. Near the ground, the colour was brighter than it was nearer the centre. In each leg, about an equal distance from each edge, was a streak of white, reaching apparently to the height of sixteen or eighteen yards, of peculiar brightness. During a considerable part of the time, at least while the bow was best defined, these streaks might be said to be rather brilliant. They continued more or less visible during at least twenty minutes.

Such were the particulars connected with this phænomenon which chiefly attracted my notice. I have simply stated facts; and leave it to scientific men, should any such deem the subject worth their notice, to explain them. If the phænomenon which I have attempted to describe, be of more frequent occurrence than

I suppose, still the above statement may not be superfluous. If it be as rare, as judging from the mention made of it in any books which I have had an opportunity of consulting I imagine it to be, the statement which I have given may not be unacceptable to men of science.

R. SMETHURST.

STAND, near MANCHESTER,
January 17th, 1817.

REMARKS

(CHIEFLY AGRICULTURAL)

Made during a short Excursion into

WESTMORELAND AND CUMBERLAND,

In August 1815.

BY

JOHN MOORE, JUN. ESQ.

(Read Feb. 21st, 1817.)

MUCH need not be said to justify those short excursions which so many of us are in the habit of annually taking, to some favorite watering place or more interesting part of the country.

The invalid has the authority of his physician, the student and man of business may well plead the acknowledged utility of occasional relaxation; and by those in easy or affluent circumstances, these delightful recreations have always been considered among the chief advantages of their condition in life.

Our excursions are usually projected in consequence of some agreeable representations; we anticipate them with pleasure, and whether undertaken from motives of health or amusement, they seldom fail to afford us much real gratification.

The company we meet with on these occasions is also, not unfrequently, very interesting. Like ourselves, they have been advised to leave their cares and anxieties behind them, and although we meet with some who have unfortunately forgot their lesson, whilst the majority attend to such instructions, the society is sure to be agreeable. The amiable and the accomplished are never seen to greater advantage; and the peevish and morose, witnessing the real value of politeness and civility, are compelled to soften down their manners to a more acceptable standard.

But it is chiefly when we are stationary, that society is of consequence; when travelling from one place to another, we have not always such resources; and as it would be unreasonable to expect, that every part of the country through which we pass should be very interesting, we must often depend upon ourselves to make the journey pleasant.

On many occasions a person fond of agricultural pursuits has a decided advantage in in travelling. At every step he finds something to imitate or avoid, and his attention is kept alive by the continual exercise of his judgment on the variety of management which he sees. The value of his remarks will, however, be very differently estimated; such as are fond of rural life, may perhaps excuse the following observations.

From Bolton to Blackburn we noticed the frequent failure of attempts to grow corn and potatoes in situations and upon soils very unfit for them. Little attention seemed to be paid to the seed potatoes, as it was not uncommon to observe three or four varieties of blossom in one row: and in some corn fields the weeds were almost equal in bulk to the straw. There are few good houses to be seen from the road, and the cottages appeared filthy and uncomfortable. The farms are small, the fences bad, and the timber mostly sickly and ill grown.

We saw only one specimen of drill husbandry, which was in a field of turnips, and not very promising. The clover was very poor, but the meadows and pastures looked much better than the plowed land, and are improvable at half the expence of growing corn. Draining seemed well attended to, materials being found in almost every farm, but weeding was much neglected in a great proportion of the arable lands and gardens near the road.

In consequence of the late high price of corn, much grass land had been converted into tillage; and it is very unfortunate for this neighbourhood, that the land is, in general, of such a nature that it is not easy to return it to grass again. Very stiff as well as very light soils are liable to lose their young grass roots during winter, and hence there is great difficulty in replacing them. Few sheep are here to be seen, and the stock of cattle seemed a mixture of all sorts. The roads were rough but firm. Having the advantage of canal conveyance, and a considerable population, it is in every respect better fitted for a manufacturing than a farming district.

From Blackburn to Preston the ride is more interesting to the farmer, though little is to be learnt. The farm houses and cottages appear more comfortable, and the crops of corn and potatoes seemed to improve in proportion to the higher quality of the land. Some good houses are to be seen from the road, with a fair proportion of well grown timber. We noticed another specimen of drill husbandry, but like the former, six weeks, at least, too late in sowing the turnips.

We passed some very good upland meadows, and observed a few of the real Lancashire long horned cows. This fine breed of cattle has been much overlooked by modern agriculturalists. They are generally good milkers; and from their large size, such of them as do not milk freely, are very well calculated for profitable feeding.

The roads are here repaired by breaking large stones which are collected from the bed of a river in the neighbourhood. This practice answers tolerably well for light weights, but was found of very little use on the roads near Manchester. We were informed that most of the smaller farmers have looms in their houses, upon which their families are chiefly employed during the winter months.

At Horton Tower, about four miles from Preston, we had a fine view of the estuary of the Ribble, and an amazing extent of the richest low land in the county.

Near Walton we saw a field of potatoes, about a Lancashire acre, in which the sets had all been planted whole. This method has been often recommended, but I had never before seen it followed to any thing like this extent. My experiments have convinced me, that, although a greater weight may occasionally be obtained in this way, nothing near the usual quantity of marketable produce is ever yielded.

By the common practice of cutting the potatoes, we generally get four or five good sized ones from every root, and about the same number of small ones; but when they are set whole, the proportion of the small ones to the larger has with me, never been less than four to one, and sometimes much more. This overbalances the advantage in point of weight, to say nothing of the encreased labour, the plow being of little use when potatoes are planted whole.

From Preston to Lancaster very little is still to be learned by the experienced farmer: there is however an appearance of greater attention to a more uniform course of crops. The wheat being chiefly summer worked, is cleaner, and as the second crops of clover are generally eaten off, the land is left in good condition. Some large houses are seen in the distances, and a few of them are well sheltered by extensive plantations. Many of the cottages and some of the farm houses, have mud walls, but they are kept cleaner and neater, on the outsides at least, than those on the road we had passed.

The fences, particularly after leaving Garstang, have a great proportion of hazel in them, which independently of its being naturally tender, is often broken by children seeking for nuts, and the consequence is, that in many places you see nearly as much naked cop as fence.

Wheat during this ride was far riper than oats, and led us to suppose the oats must be sown too late, or the seed not properly attended to.

It is the practice with many farmers in the north of England to procure their seed corn from the more southern counties, apprehending that it is there grown in higher perfection. This is however in direct opposition to what has often been stated, viz. that the seeds of the colder climates ripen sooner.

We are informed that in Lapland, barley ripens in 60 days; whereas in the south of France it requires 130 to 140 days. It is also stated that the same holds good in a great degree with respect to the seeds brought from these countries.

The potatoe culture during this part of our journey was at a low ebb. No attention was paid to good seed. Runners were as common as the true potatoes.

At Lancaster we had an opportunity of seeing the salmon fishery; and we found that a single fish could not be bought cheaper there than in Manchester. Salmon of one, two, and three years growth, as well as the older ones which go up the rivers to spawn, leave the sea for fresh water at this season of the year, and afford excellent sport to the angler in this neighbourhood. We were well informed that when the young brood go down to the sea, they are always preceded by some of the parent fish, whilst others follow close in their rear, to protect them on their journey.

From Lancaster to Burton the road was very good, being well repaired by breaking the stones collected from the adjoining fallows. Not many good farm houses were to be seen, and very few cottages. The fences are chiefly white thorn of very vigorous growth, and little interrupted by hedgerow timber of any kind.

The grass land near Lancaster, as is the case near most large towns, was highly improved, and the corn and potatoes looked very well. Summer fallowed wheat, oats, and clover, or turnips, barley, and clover, are the usual rotation of crops; but we noticed a greater proportion of barley than of any other kind of grain. A few of the turnips were drilled, but the corn was invariably sown broad-cast. The wheat was of the old red lammas kind, and it is perhaps worth while here to remark the decided advantage it had the last season, over all the new and more fashionable varieties. As the lammas or red wheat ripens, it begins to hang its head, and the rain falls very readily from it: whereas the white and bearded wheats generally support their heads perpendicularly during the whole of their growth, and being downy and disposed to retain moisture, are, of course, more liable to sprit in this position than when the head is more pendant from the stalk. We were particularly struck with the bright yellow colour of the wheat straw, which seemed to resemble in quality that which is grown upon the chalk lands of Bedfordshire. We could not however learn, that it had been at all applied to the same beautiful manufacture. Near Dunstable it is a source of the greatest profit to the farmer, as it is not unusual for a crop of wheat straw to sell for much more than the grain.

Adjoining the Burton road, so great a proportion of the land is in tillage, that we did not expect to see much live stock. The plows were drawn by two horses abreast, led by a single one, and of course requiring a driver.

After leaving Burton, we had a rich view of an extensive tract of corn land, apparently under very productive management.

Although the country from Lancaster to

Burton is by no means hilly, it is finely varied by a succession of gentle swells. Almost every field partakes more or less of some of them, but they do not appear to hinder the cultivation in any respect. The surface of a great part of the noted Filde land is similarly marked. In the celebrated vale of Evesham, it is the constant aim of the farmer to imitate these natural undulations, on his low grounds, by plowing his broad lands invariably in one direction, and thereby gradually drawing the soil higher into the centre of them.

From Burton to Kendal the road passes through a well cultivated country not remarkable for any peculiarity of management. We met no other but single horse carts: one driver has generally the care of two or three of them, and the horses are so accustomed to follow each other in line, that when the direction of the leading one is changed, a carriage has no difficulty in passing the rest. With a view to the protection of the roads, an attempt was made a few years ago, to introduce these carts generally, without success. Those we met were mostly laden with lime, which appeared the chief article used in dressing the summer fallows. The clover was very good, and generally fed off by well sized sheep. Gate posts, an expensive article in many places, were here supplied by two stone pillars of similar dimensions. The approach to Kendal is very pleasing; but we could not help regretting there should still remain any stone walls as fences in so rich a valley.

From Kendal on the high road to Penrith, for several miles there was some spirited farming, and we saw good crops of barley on very high ground; but these hills appeared naturally fertile, and lime is readily procured in the neighbourhood.

Turning round, about three miles from Kendal, we had a most pleasing view of the country we had just passed. The remainder of our ride to Shap is interesting only to the sportsman.

From Shap to Lowther Castle we had an opportunity of witnessing the decided superiority of the drill system, in the cleanness and excellence of the turnip and corn crops. Indeed we had seldom seen better corn; but owing to its uncommon luxuriance, it was much beaten down by the rains, which had recently fallen.

Lowther Castle has been recently rebuilt in the old stile, and when a little more softened by time, will be quite in character with the immense forest which surrounds it.

In the neighbourhood of Penrith the farmers seemed to have availed themselves of the instructions of their famous agriculturalist, Mr. Curwen. Near the residence of such a character, one naturally expects some proof of the influence of good example, and we were not disappointed. Some excellent crops of the bullock turnip, were shewn to us. It was here preferred to the Norfolk and the Globe, the latter of which it resembles in its conical roof. It has an advantage over the Norfolk, in its confined crown, and being less disposed to lodge snow or rain within it, which occasions much injury to this useful root.

The land in the neighbourhood of Penrith, is varied in its management; some of the farmers pertinaciously adhering to the old system, and others with spirit adopting the new. We should have supposed this contrast could not have remained more than a few years, had we not been aware of the difficulty of introducing the most obvious improvements among farmers.

From the extent of sheep walk attached to most of the farms in Cumberland and Westmoreland, a greater proportion of the low lands is in tillage, than would otherwise be allowed. There is seldom seen in any county, more arable land at one view, than we often meet with on the banks of the Eden.

Our first introduction to the lakes was at Pooley Bridge, the lower end of Ulswater. The Emont, which runs from the lake under this bridge, was literally alive with trout, and our impatience to begin our sport made us curtail some very beautiful walks near this station.

There are some very healthy plantations on the borders of Ulswater, consisting of varieties of the fir, with larch, oak, beech, chesnut, poplar and plane-tree ; and on the lower grounds, as might be expected, the willow is preferred. Perhaps there is no tree to which our best sylvan scenery is more indebted than to the beech. In the sheltered situations, which the country round the lakes so often furnishes, its fine hanging branches acquire a luxuriance, and at the same time a delicacy of foliage, which is not surpassed by any other tree.

The farming near Ulswater was not equal to what we had just seen ; but the farm houses were in good order, and the cottages very neat and comfortable in their appearance.

From the battlements of the bridge we had an opportunity of watching the trout feed, without disturbing them ; but the water was so very clear, that whenever we attempted to throw a line upon it, they retreated in a moment. As the brightness of the day declined we began to have charming diversion; we had however great reason to regret, that none of our fishing books contained a proper classification of the flies, which trout feed upon at different seasons of the year. The provincial names of our best authorities were of no use to us; and we soon found that old father Walton scarcely notices one of the flies which procured our sport. Having caught some live ones, they proved to be varieties of ephemeræ, and were easily imitated; but as the night approached, we had more difficulty to copy some of the phalænæ, as we could not so easily make out their colours. The large trout were taking these moths very boldly, before darkness put an end to our diversion.

There being no pike in Ulswater, it abounds with trout; and it is impossible to conceive better quarters for the angler than this place affords.

The Emont, as it runs through the vale of that name, is every thing which Walton himself would have wished for. In May there is excellent sport upon the lake, and in the streams, with the various ephemeræ and phryganeæ, and in June their famous coleopterous fly, the bracken clock, draws together a joyous fraternity of anglers.

The larvæ of flies are so numerous in some rivers, and so easily obtained by the trout, that whenever they begin to feed upon them, there is a suspension of sport with the fly, which continues till the insect bursts its prison and escapes from the water. The angler who is not aware of this circumstance, is often at a loss to account for his blank days, when the weather appears favourable, and the water is acknowledged to be in fine order for fishing .Water-bred flies seem to be most relished by trout; but unfortunately, this branch of entomology, so interesting to the disciple of Walton, has hitherto been but little attended to, and is yet very imperfectly understood.

From Ulswater to Keswick it rained almost incessantly, so that we had seldom an opportunity of seeing either the country or its management. Some tourists however, make up these disappointments very easily. A celebrated agriculturist having dined at ——— left that house on his road to ——— after dark. As this was an interesting part of ——— and it would be expected that he should say something of it in his journal, he gets over the difficulty in the following brief remark—" From ——— to ——— I saw no sheep."

The road to Keswick is very hilly, but firm. The sun broke through the clouds, just as we passed the rich vale of St. John, and we had an opportunity of contrasting the highly cultivated landscape on our left, with the bold, and varied ruggedness of the mountains on our right. The crops of corn looked very well, but the wet weather having set in during their hay harvest, had rendered most of it unfit for use. We were very sorry to find the cottages and their little gardens near the road so very slovenly; as we could not help wishing for the appearance of comfort and happiness amidst such delightful scenery.

Lime being an expensive article, we observed few turnips and little clover.

The mountains were studded over with sheep; many were browsing upon rocks and precipices, which seemed quite inaccessible to man. Most of these sheep were of the true mountain breed, and very small. The half bred Leicester have been found to stand the winter very well in Derbyshire, but they would not be active enough for the craggy walks of Cumberland.

The rain having entirely ceased, we saw the late Bishop of Landaff's extensive plantations to great advantage. Such as were tolerably well sheltered, seemed to be in a very thriving state. It was easy to perceive that the larch was the favourite tree with this celebrated planter, as it equalled in quantity almost all the rest. Where new enclosures are anticipated, this is certainly a most useful plant, the thinnings being so admirably calculated for railing, but on other accounts it seems to be daily losing ground in the estimation of the public. Like the black Italian poplar, its uncommonly vigorous growth for the first ten or twelve years, has not in many instances been followed up by a proportionate improvement afterwards; and although both these fashionable plants far outstrip our more sturdy forest trees in their early thriving, and bring a quicker profit to the owner; such as plant for their children as well as themselves begin to find that the oak, the ash and the common poplar, ultimately pay them better.

I apprehend there are few large plantations on low ground, in which the oak, judiciously placed, has not in thirty years overtaken the larch. I made an average a few years ago, where it was decidedly in favour of the oak, and an eminent northern planter informed me that on a most careful examination of some of the largest enclosures in Scotland, he had frequently found a similar result. Where the bulk is equal, the comparative value is obvious.

The approach to the Derwent from Keswick is very beautiful. Our introduction to this lake was particularly fortunate. A mild summer's evening had succeeded a very rainy day. The sky was without a cloud, and the atmosphere free from mist. The remoter hills had assumed a variety of soft purple tinges which formed a fine distance to the dusky foreground of those mountains which immediately overhang and protect the beautiful scenery of the Derwent. The lake itself was smooth as glass, and the softened reflections of the surrounding landscape upon its surface, were almost as pleasing as their rich originals. The birds which inhabit the fine hanging woods on the borders of the lake, were challenging their rivals upon the islands, and the distant responses of the latter, often resembling an echo, had a most bewitching effect upon the ear.

These islands are covered with the finest specimens of our ornamental trees. Even the ash and the alder, which in common growth are not often remarked for their beauty, here acquire a delicacy of foliage that entitles them to a companionship with the beech, the larch, the willow and the acacia.

The fine slopes of sheep walk rising from the lake, are completely sheltered by plantations of most luxuriant growth, and above these, an astonishing succession of lofty mountains are changing their mighty profiles and fantastic features at every step you take. In their nearer approaches to each other, they form dark and dismal glens: receding, they open into the most enchanting valleys, the sides of which are often fringed with coppice, and almost all of them are enlivened by some brilliant little trout stream, returning its crystal water to the larger rivers on the lakes below.

That beautiful ephemera the May fly, or as it is called in Derbyshire the drake, makes its appearance on these rivers from the 1st to the 12th of June. Finding its wings then at liberty, it rises from the bottom of the water with the husk of the pupa still attached to the lower part of the body; but on reaching the surface it disentangles itself entirely, and flies, with much difficulty, to the first tree or hedge it can meet with, and immediately creeps under the leaves. Here it remains without food for two or three days, when another very wonderful change takes place. The whole of its outer green coloured skin is then stripped off, and the fly emerges in a much more active and delicate form. If the day be fine, it immediately sallies forth in search of its mate, and they are seen dancing together in the air, and mostly over the rivers or lakes, in immense quantities. The ova of the female being protruded, are jerked from them, in their up and down flights, and their specific gravity being much greater than water, they sink quickly to the bottom, where they are hatched by the warmth of the succeeding months, and the larvæ seek their winter quarters in the sand beds, and stiller parts of rivers. The parent fly not appearing to have taken any food, becomes perfectly transparent and dies. This I apprehend is also the history of the lesser ephemeræ, known to anglers by the name of duns.

Most of the valleys in Cumberland afford excellent pasturage for sheep, and are chiefly appropriated to such as are immediately intended for market. In these natural meadows, many of the best esteemed grasses are to be found, and excepting where the land has been over tilled, the acrid and sourer ones are seldom seen.

Our second day at Keswick was spent upon the lake, from which we had a perpetual succession of the most enchanting views. We had also excellent sport with pike, the wind being easterly. Our books on angling tell us that pike seldom feed on perch, and advise us to keep at home, when the wind is easterly. Here a live perch is the usual bait, and an east wind the most desirable.

We were much surprised to see the promising crops of oats and barley which were growing upon Latrig, according to Mr. Otley, 880 feet above Keswick and 1100 higher than the sea. As this is about one third of the height of Skiddaw, it would be fair to conclude that it is higher than any of those situations in Lancashire, where we had seen the growth of corn so unsuccessfully attempted. We could not however help thinking, notwithstanding these more fortunate experiments, that it is wiser to plant the better aspects of high hills, and in the end more profitable. Manure is in every instance carried up at a great expence, and very liable, with the crop, to be washed away by the storms of the county which so often prevail in mountainous districts.

A short distance from Keswick, on the Cockermouth road, we had a charming view of the rich vale of Newlands and Bassenthwaite water. Here again we saw the whole of the corn grown on Latrig, surrounded on every side by heath. It was impossible not to admire the spirit of the gentleman who had undertaken these improvements, whilst we very much doubted his remuneration. Considerable allotments of Whinlatter, nearly upon the same elevation, were also covered with excellent oats.

Taking the higher road, along the side of Swinton, we had a commanding prospect of the vale of Lorton, and looking towards Cockermouth, over Solway frith, distinctly perceived the foremost of the hills in Scotland.

A great part of our road was through sheep walk of very unequal quality. Down in the vale there appeared much more corn land than pasturage; wheat was generally followed by barley or oats sown with clover and other artificial grass seeds. Some of the hill sides had been much injured by paring and burning, the temptation to which no doubt had arisen from a desire in the farmers to avail themselves of the ashes of the fern, which grows here most luxuriantly.

Having refreshed ourselves at Scale-hill, we ascended a station near the house, which enabled us to overlook the whole of Crummock-water, and part of Lowes-water. The barren mountains, which surround the former, rise immediately from the borders of the lake, and with the exception of a few patches on the warmer sides, have no vegetation whatever upon them. This lake is considered much deeper than the Derwent or Bassenthwaite, and produces the finest char. Unlike most others, these delicious fish forsake the shallows during the warmer months, and retreat to the deepest water out of the reach of nets, and are seldom tempted by baits of any kind. In the month of October, they begin to return to the creeks on the east side of the lake, and become a considerable source of profit to the proprietors.

On our return to Keswick, we visited the extensive nursery gardens, and were much surprised to find, that the evergreens had sustained little injury during the uncommonly severe winters of 1813—14. Apples and pears were in great abundance, but the plum trees did not appear equally healthy or prolific. Many of the cottagers keep bees, which, from the abundance of heath blossom on the neighbouring hills, afford the choicest honey. Stall or pen feeding is not here much practised: the sheep are brought up by browsing upon the young heath of the mountains, and are fed off chiefly by the sweet herbage of their sheltered pasturage; hence the decided superiority of the Cumberland mutton. Having understood that the fine flavour of their bacon arose from its being chiefly fed on peas, we were surprized to see so few grown in the country. On enquiry, we found that barley and oats were as much used here in feeding as elsewhere, we had therefore reason to believe that this acknowledged excellence arises chiefly from the pigs being much less confined. From their birth they are suffered to go at large in the woods, and if the season be favourable, they make themselves half fat with acorns.

The clipped or plashed hedge-row is not often seen near the lakes, and the trees which grow in the fences are seldom pruned. This neglect certainly softens the landscape to the eye of the painter, but cannot be justified to the prudent farmer.

The road from Keswick to Ambleside, affords a continual variety of the most interesting scenery. Grasmere, by many esteemed the most beautiful of the lakes, forms a very important part of it, but as I have already trespassed upon the time of the Society, I will not, at present, trouble you with the remainder of the remarks which were made to preserve the recollection of this delightful excursion.

I cannot however conclude without observing, that, during our ramble among the lakes, we had frequent occasion to apply to the lower orders of the inhabitants as guides and assistants in our sports; and that we were invariably pleased with their attention and civility, and especially with their very reasonable expectations. Considering their encreased intercourse with strangers, their habits appeared much less contaminated than one might have expected; and if they do not in their manners exactly come up to that pastoral simplicity and purity, which one would fain attach to such a country, they are certainly very free from the vices, and any thing like the rudeness and incivility, of some of our manufacturing districts.

A TRIBUTE

TO THE MEMORY OF THE LATE

PRESIDENT

OF THE LITERARY AND PHILOSOPHICAL SOCIETY

OF MANCHESTER;

BY

WILLIAM HENRY, M.D. F.R.S.

&c. &c.

(Read April 11th, 1817.)

THE following Tribute to the Memory of the late President of the Literary and Philosophical Society has been drawn up, in compliance with a request, expressed to the writer from the chair, at an early meeting during the present session. It would, on some accounts, have been more satisfactory to him, that the office should have fallen into other hands. But, conceiving a compliance with the requisition to be a duty, which he was not at liberty to decline, he has endeavoured to execute it with all the impartiality and fidelity in his power; and he trusts to the candour of the Society for that share of indulgence, which he may reasonably claim, in speaking of one to whom he was so nearly allied.

The late Mr. Henry was descended from a respectable family, which, for several generations, had resided in the county of Antrim. His paternal grandfather commanded a company of foot in the service of James the Second; and during the disturbed times, which, in Ireland, succeeded the revolution, was shot by an assassin in his own garden. The father of Mr. Henry, then an infant scarcely a year old, was taken under the generous protection of a neighbouring nobleman,* who continued it to him during the remainder of his life; and, after being educated in Dublin at his lordship's expence, was brought over by him into Wales, when he had nearly attained the age of manhood. Having there, a few years afterwards, married the daughter of a respectable clergyman of the establishment, they sought the means of support by jointly engaging in the educa-

* Viscount Bulkley.

tion of females, and for many years conducted a respectable boarding school, first at Wrexham in North Wales, and afterwards in Manchester.

It was at the former place that Mr. Henry was born, on the 26th of October, O. S. in the year 1734. For some years, he remained under the tuition of his mother, who was admirably fitted for the task, and of whom he was always accustomed to speak with the warmest affection and gratitude. At a proper age, he was sent to the Grammar School of Wrexham, at that time in considerable repute. There he was fortunate in having, for his first classical instructor, the Rev. Mr. Lewis, whose virtues and talents are the subject of an elegant Latin epitaph, copied by Mr. Pennant into his Tour through Wales.* At this school, Mr. Henry remained for several years, and made such proficiency in his classical studies, as to have attained the foremost station, with the exception only of Mr. Price, who was afterwards well known as the keeper of the Bodleian Library in the University of Oxford.

The inclination of Mr. Henry, from early life, led him to the church; and it was

* Page 293.

determined that, on leaving school, he should remove to Oxford. The day of his departure was accordingly fixed, and a horse was provided for the journey. But as the time drew near, his parents, who had a numerous family, and were far from being in affluent circumstances, grew discouraged at the prospect of expences that were unavoidable, and at the uncertainty of eventual success. While they were thus hesitating, Mr. Jones, an eminent apothecary of Wrexham, decided the point, by proposing to take Mr. Henry as an apprentice; and to this measure, though deeply feeling the disappointment of long indulged hopes, he could not deny the reasonableness of assenting. With Mr. Jones he continued, till that gentleman died suddenly from an attack of gout, when he was articled, for the remainder of the term, to a respectable apothecary at Knutsford in Cheshire.

In neither of these situations did Mr. Henry enjoy any extraordinary opportunities of improvement. The only book, which he remembered to have been put into his hands, by either of his masters, was the Latin edition of Boerhaave's Chemistry, in two vols. quarto, a work, which, whatever may have been its merits, was certainly not calculated to present that science to a beginner under a fascinating aspect. His reading was, therefore, entirely self directed; and, by means of such books as chance threw into his way, he acquired a share of knowledge, creditable both to his abilities and his industry.

At the expiration of his apprenticeship, he engaged himself as principal assistant to Mr. Malbon, who then took the lead as an apothecary at Oxford. In this situation, he was treated by Mr. Malbon with the indulgence and confidence of a friend; and his time was chiefly spent in visiting patients of the higher class, a majority of whom were members of the University. Among the students at Oxford, were several, who recognized Mr. Henry as a former associate, and who, though holding the rank of gentlemen-commoners, renewed their acquaintance with him, and afforded him the most friendly countenance. His leisure hours were, therefore, spent most agreeably and profitably in the different colleges; and his taste for literary pursuits was encouraged and confirmed. At Oxford, he had an opportunity of attending a course of anatomical lectures, in which the celebrated John Hunter, then a young man, was employed as demonstrator.

From Mr. Malbon, who was become affluent, Mr. Henry received a strong mark of esteem and confidence in the offer of a future partnership. To have accepted this, it would have been necessary that he should have qualified himself to matriculate, which would have required the completion of a residence of seven years. But other views in life, which were inconsistent with so long a season of expectation, induced him to decline the proposal; and in the year 1759, he settled at Knutsford, where he soon afterwards married. After remaining five years at this place, he embraced the opportunity of succeeding to the business of a respectable apothecary in Manchester; where he continued, for nearly half a century, to be employed in medical attendance, for the most part on the more opulent inhabitants of the town and neighbourhood.

Soon after Mr. Henry's settlement in Manchester, the late Dr. Percival removed to the same town from Warrington. That eminent physician was early inspired with the same ardent zeal for the cultivation of professional and general knowledge, which afterwards so much distinguished him. Between Dr. Percival and the subject of this memoir, congeniality of taste and pursuits led to a frequent intercourse; and the moral qualities of both cemented their connection into a friendship which continued, without interruption, until it was terminated by the death of Dr. Percival, in 1804. It was about the same early period, that he formed an acquaintance with that excellent man, and upright magistrate, the late Mr. Bayley of Hope-Hall, and much of the happiness of his future life was owing to the mutual esteem and confidence, and to the frequent intercourse, which continued to exist between them for more than thirty years.*

During his apprenticeship, Mr. Henry had manifested a decided taste for chemical pursuits, and had availed himself of all the means in his power, limited as indeed they were, to become experimentally acquainted with that science. This taste he continued to indulge after his settlement in life; and, after having made himself sufficiently master of what was ascertained in that department of knowledge, he felt an ambition to extend its boundaries. In the year 1771, he communicated, to the Royal College of Physicians of London, "An Improved Method of Preparing Magnesia Alba," which was published in the second volume of their Transactions. Two years af-

* An interesting biographical sketch of Mr. Bayley, written by Dr. Percival, appeared in one of the volumes of the Monthly Magazine for the year 1802.

terwards it was reprinted, along with essays on other subjects, in a separate volume, which was dedicated by Mr. Henry to his friend Dr. Percival.

The calcination of magnesia had, at that time, been practised only in connection with philosophical inquiries. Dr. Black, in an Essay which is still perhaps not surpassed in chemical philosophy, as an example of inductive investigation, had fully established the differences between magnesia in its common and in its calcined state; but he does not appear to have made trial of the pure earth as a medicine, though several inconveniences, from its use in the common form, had long before been pointed out by Hoffman.* On this subject Mr. Henry's claims extend to the free disclosure of his improvements; to the early and strenuous recommendation of the medicinal use of pure magnesia; and to the discovery of some of its chemical agencies. It is but justice to him to state that his recommendation of its employment as a medicine was perfectly disinterested; for it was not till his work was printed, and on the eve of issuing from the press, that the preparation of magnesia for sale was suggested to him by

* Hoffman. Oper. Tom. 4. p. 381.

a friend, in a letter relating to the intended publication, which is still preserved as a part of his correspondence. Before carrying this suggestion into effect, he thought it proper to consult Sir John Pringle, Sir Clifton Wintringham, Dr. Warren, and some other leading members of the College of Physicians, as to their opinion of the propriety of the measure ; and he did not adopt it, until those gentlemen had each declared it to be not more advisable on his own account, than on that of the public.

Soon after the publication of the small volume of Essays, Mr. Henry found himself involved in a controversy, arising out of some remarks in the appendix, respecting which, as the subject was of temporary interest, it is unnecessary to enter into particulars. It is sufficient to state that the accuracy of some of his experiments, which had been called in question, was confirmed by the concurrent testimony of Dr. Percival and Dr. Aikin ; and that the chemical properties, first ascertained by him to belong to pure magnesia, were considered, by Bergman and by Macquer, as worthy of being incorporated into their respective histories of that earth.

It was probably in consequence of the publication of these enquiries, that Mr. Henry was admitted into the Royal Society of London, of which he became a Fellow in May 1775. The persons, most active in promoting his election, were Sir John Pringle and Dr. Priestley; and he had the advantage not only of the vote, but of the favourable influence of Dr. Franklin, who happened at that time to be in London. Several years afterwards, the same venerable philosopher, when in the 81st year of his age, presided at the meeting of the American Philosophical Society, at which Mr. Henry was elected a member, and again honoured him with his suffrage.*

The writings of the celebrated Lavoisier were introduced by Mr. Henry to the notice of the English reader in 1776. The earliest work of that philosopher was a volume, consisting partly of an historical view of the progress of pneumatic chemistry from the time of Van Helmont downwards; and partly of a series of original essays, which are valuable as containing the germs of his future discoveries. To this work, Mr. Henry added, in the notes, occasional views of the labours of contemporary English chemists. A few years afterwards he translated, and collected into a

* This circumstance is stated in a letter from Dr. Rush to Mr. Henry, dated Philadelphia, 29th July, 1786.

small volume, a series of Memoirs, communicated by M. Lavoisier to the Paris Academy of Sciences, when the views of that philosopher, respecting the anti-phlogistic theory of chemistry, were more fully unfolded. In undertaking the translation of these works, he was influenced by a desire to place within the reach of English readers, among whom the knowledge of the French language was then confined to comparatively few, the pleasure and conviction which he had himself derived from these beautiful models of philosophical enquiry.

Notwithstanding the large share of professional employment, to which Mr. Henry had now attained, he still continued to engage frequently in experimental pursuits, the results of which, at this time, were communicated to the world, chiefly through the publications of his friends Dr. Priestley and Dr. Percival. Of these, the most important were some Experiments on the Influence of Fixed Air on Vegetation, by which he endeavoured to shew that though fixed air is injurious, when unmixed, to the vegetation of plants, yet that when mingled in small proportion with common air, it is favourable to their growth and vigour. The facts, established by this enquiry, were communicated to Dr. Priestley; and it is creditable to the candour of that distinguished philosopher, that he was anxious to make them public, not only for their general merit, but because in one or two points the results disagreed with his own. "I am much pleased," Dr. Priestley replies, "with the experiments mentioned in your letter, and if you have no objection, shall be glad to insert the greatest part of it in my Appendix, which I am just sending to the printer's. I the rather wish it, as a few of the experiments terminate differently from those that I shall publish, and I wish to produce all the evidence I can come at on both sides. The other experiments are very curious and will give much satisfaction."* The investigation was afterwards resumed by Mr. Henry, and made the subject of a paper, which is printed in the second volume of the Memoirs of this Society.

The occasion of Mr. Henry's next appearance, as the author of a separate work, arose out of an accidental circumstance. He had found that the water of a large still tub was preserved sweet for several months by impregnating it with lime, though, without this precaution, it soon became extremely putrid.

* Letter from Dr. Priestley to Mr. Henry, dated Jan. 5, 1777.

This fact suggested to him an eligible method of preserving water at sea;* but as lime water is unfit for almost every culinary purpose, some simple and practicable method was required of separating that earth from the water, before being applied to use. This, he ascertained, might be accomplished at little expence by carbonic acid, the gas from a pound of chalk and 12 ounces of oil of vitriol being found sufficient for the decomposition of 120 gallons of lime water.† The only difficulty was in the mode of applying the gas on a large scale; but this was overcome by the contrivance of an apparatus, which Mr. Henry described in a pamphlet dedicated to the Lords of the Admiralty. The proposal, in consequence of the zealous personal exertions of Mr. Wedgwood, who was then in London, met with due attention from the Commissi-

* Dr. Alston of Edinburgh appears, however, to have been the first who proposed impregnation with lime, as a mean of preventing the putrefaction of water; and to precipitate the lime, he suggested the use of carbonate of magnesia.

† The water, however, for which these proportions were sufficient, could not have been completely charged with lime, for fully saturated lime-water would have required for decomposition nearly three times that quantity of chalk and oil of vitriol.

oners for victualling his Majesty's Ships. The chief obstacle to its adoption in the Navy was an apprehension, probably well grounded, that persons would scarcely be found on ship-board, possessing sufficient skill for conducting the process successfully. Since that time, the preservation of water at sea has been accomplished by the simple expedient of stowing it in vessels constructed or lined with some substance, which is not capable of impregnating water with any putrescible ingredient; for good spring water, it is well known, contains essentially nothing that disposes it to putrefaction.

The philosophical pursuits of Mr. Henry, not long after this period, received an additional stimulus by the establishment of the Society to which these pages are addressed, and by his anxious desire to fulfil his duties as a member of it. To him, on its being first regularly organized in the winter of 1781, was confided the office of one of the Secretaries. At a subsequent period, he was advanced to the station of Vice-President, and in the year 1807, on the vacancy occasioned by the death of the Rev. George Walker, F.R.S., he received from the Society, and retained during the rest of his life, the highest dignity which it has to bestow.

The "Memoirs of Albert de Haller," which were published by Mr. Henry in 1783, and dedicated to this Society, were derived partly from a French *Eloge*, and partly from information communicated by the late Dr. Foart Simmons. A more complete view of the life and acquirements of that extraordinary man might have been collected, at a subsequent period, from other publications of the same kind, which were addressed to different learned societies on the continent. In one respect, Mr. Henry appears to have taken too favourable a view of the character of Haller, in ascribing to him gentleness of disposition; for that illustrious, and in the main excellent, person, seems to have been a man of quick passions, and not sufficiently reserved in the expression of them; as may be gathered from his controversy with Dr. Whytt of Edinburgh. Haller is represented, also, by his biographer, as afflicted with the personal defect of weak eyes; which, from a passage in his Physiology,* appears not to have been correct. "Aquæ puræ," he says," qua ab anno ætatis 18 sola utor, tribuo, quod post tot in fulgido sole susceptos microscopicos labores, omnibus sensibus, *et oculis*

* Tom. vi. p. 240. Edit. 2. Lausannæ.

potissimum, non minus valeam, quam puer valui."

During the long season of Mr. Henry's activity as a member of this Institution, his communications to it were very frequent. Many of these were intended only to excite an evening's discussion, and having served that purpose were withdrawn by their author; but the number is still considerable, which are preserved in the Society's published volumes. As might be expected, they are of various degrees of merit, but there are among them two papers, which have contributed greatly to his reputation as a chemical philosopher.*

* The following is a list of Mr. Henry's papers, that are dispersed through the printed Memoirs of this Society.

In Vol. I. (1.) An Essay on the Advantages of Literature and Philosophy in general, and especially on the Consistency of Literary and Philosophical with Commercial Pursuits.

(2.) On the Preservation of Sea Water from Putrefaction by means of Quicklime.

(3.) On the Natural History and Origin of Magnesian Earth, particularly as connected with those of Sea Salt and Nitre, with Observations on some Chemical Properties of that Earth, which have been hitherto unknown or undetermined.

In Vol. II. (1.) Experiments on Ferments and Fermentation, by which a Mode of exciting Fermentation in Malt Liquors, without the aid of Yeast, is pointed out; with an attempt to form a new Theory of that Process.

The Essay on Ferments and Fermentation is valuable, not for the theoretical speculations which it contains, for these have been superseded by subsequent discoveries; but for a few facts of considerable importance. It was at that time believed that the infusion of malt, called *wort*, could not be made to ferment, without the addition of yeast or barm; but Mr. Henry discovered that wort may be

(2.) Observations on the Influence of Fixed Air on Vegetation, and on the probable Cause of the Difference in the Results of various Experiments made for that purpose.

In Vol. III. Observations on the Bills of Mortality for the Towns of Manchester and Salford.

(2.) Case of a Person becoming short sighted in Advanced Age.

(3.) Considerations relative to the Nature of Wool, Silk and Cotton, as Objects of the Art of Dyeing; on the various Preparations and Mordants requisite for these different substances; and on the Nature and Properties of Colouring Matter—Together with some Observations on the Theory of Dyeing in general, and particularly the Turkey-Red.

New Series, Vol. II. Remarks on Mr. Nicholson's Account of the Effects produced at Swinton by a stroke of Lightning.

And a paper, printed in this volume, entitled Memoirs of the late Charles White, Esq. F. R. S. chiefly with a Reference to his Professional Life and Writings.

brought into a state of fermentation, by being impregnated with carbonic acid gas. By a fermentation thus excited, he obtained not only good beer, but yeast fit for the making of bread; and, from separate portions of the fermented liquor, he procured also ardent spirit and vinegar, thus proving that the fermentative process had been fully completed. He found, moreover, that flour and water, boiled to the consistence of a thin jelly, and impregnated with carbonic acid in a Nooth's machine, passed into fermentation, and by the third day had assumed the appearance of yeast, for which it served as a tolerable substitute in the baking of bread.

The other memoir, which is distinguished by its value and importance, is entitled "Considerations relative to the Nature of Wool, Silk, and Cotton as Objects of the Art of Dyeing; on the various Preparations and Mordants requisite for these different Substances; and on the Nature and Properties of Colouring Matter."

After having given a general view of the history of the art of Dyeing, Mr. Henry, in this elaborate Essay, examines the theories, that had been framed to account for the various facility and permanency, with which different substances attract colouring matter. He demonstrates the futility of those hypotheses, that explained the facts by supposed peculiarities of mechanical structure in the materials to be dyed; and suggests the probability, that the unequal powers of absorbing and fixing colouring matter, manifested by wool, silk, linen, and cotton, depend on the different attractions, inherent in those substances as chemical compounds, for the various colouring ingredients. All the preparatory operations, though differing for each material, have, he apprehends, one common object, viz. the removal of some extraneous matter, which, being already united with the substance to be dyed, prevents it from exerting its attraction for colouring matter. The ultimate object of these preliminary steps, he states to be the obtaining a white ground, that may enable the colours to display the full brilliancy of their several tints. To explain the preparation of cotton for the Turkey-red dye, he endeavours to prove that cotton requires, for this purpose, to be approximated, in composition, to the nature of an animal substance. He next offers a classification of the *Materia Tinctoria*, and some general speculations on the nature of colouring matter.

In the second part of the Essay, Mr. Henry investigates the mode of action of those substances, which, though themselves destitute of colour, are important agents in the processes of Dyeing. Substances of this kind had received, from the French dyers, the name of *Mordants*, because it was imagined that they corroded and removed something, which mechanically opposed the entrance of the colouring matter into the pores of the material to be dyed. To destroy this erroneous association, Mr. Henry proposes that the word *basis* should be substituted, as a general term, to denote every substance, which, having an affinity both for the colouring matter, and for the material to be dyed, is capable of serving as an intermedium between the two; and that a specific epithet should be added, to distinguish each particular variety. In this essay, Mr. Henry, for the first time, explained the true nature of the liquor, which is employed for affording the aluminous basis, prepared by mixing the solutions of alum and of sugar of lead. This liquor he shewed to be essentially a compound of pure clay or alumine with acetic acid; and its superiority over a solution of common alum, for yielding the earthy base in dyeing, he ascribes partly to the less affinity of the acetic acid, than of the sulphuric, for alumine, and partly to the greater volatility of the acetic acid, when exposed to a moderate increase of temperature. The remainder of the paper is chiefly occupied with the details of the operations, then practised, for dyeing Turkey red; with a theory of the process; and with a general view of the mode of action of the individual mordants or bases. The methods of dyeing Turkey red have been since much improved and simplified, though its theory is, even yet, far from being well understood. But the opinions, inculcated by Mr. Henry respecting the action of mordants, evince a remarkable superiority to the prejudices, with which he found the subject encumbered, and are, indeed, those which are still held by the latest and best writers on the principles and practice of Dyeing.

In the year 1783, an Institution arose out of this society, which had great merit, not only in its plan and objects, but in the ability exerted by the several persons, who were concerned in their fulfilment. It was destined to occupy, in a rational and instructive manner, the evening leisure of young men, whose time, during the day, was devoted to commercial employments. For this purpose, regular courses of Lectures were delivered on the Belles Lettres, on Moral Philosophy, on Anatomy and Physiology, and on Natural Philosophy and Chemistry. Mr. Henry, assisted by a son, whose loss he had afterwards to deplore, and whose promising talents and attainments obtained for him, at an early period of life, a mark of the approbation of this Society,* delivered several courses of Lectures on Chemistry to numerous and attentive audiences. From causes, which it is not easy to trace, but among which, I believe, may be reckoned, a superstitious dread of the tendency of science to unfit young men for the ordinary details of business, this excellent Institution fell into decay. Mr. Henry, however, continued his lectures long after its decline, until deprived of the services of his son, by the prosecution of views at a distance, when he found that his own leisure was not, of itself, adequate to the necessary preparations.

That the scheme of establishing in Manchester a College of Arts and Sciences (for so it was entitled) was not a visionary project, but one, which appeared feasible and promising to men of sense and knowledge at a distance, is shewn by the following extracts from letters addressed to Mr. Henry, in reply to

* See Dr. Percival's eloquent address to Mr. Thomas Henry, Junior, on presenting to him the silver medal of the Society.—Memoirs of the Society, Vol. II. page 513.

his communication of the plan. "An attempt of this kind," the late Dr. Currie of Liverpool observes, "I think most praiseworthy; and for this, however the matter may terminate, the projectors will always be entitled to public favour and esteem. It is a bold enterprize, and of course in some degree doubtful. One thing appears to me probable;—that if the business is taken up as it ought to be by the public, you will soon find the propriety of extending your plan, so as to make it embrace every object of general education." Mr. Wedgwood, also, strongly expressed his approbation of the undertaking. "The plan of your College," he says, "I think an excellent one, and from the populous and commercial state of your town—from the apparent utility of the Institution—from the elegance and propriety with which it is announced—and from the known characters of the gentlemen who are engaged in it, I can scarcely entertain a doubt of its meeting with success." Greater perseverance would, perhaps, have gradually softened, and finally subdued, the prejudices that seem to have existed against the union of commercial with literary or philosophical pursuits,—an union which, under proper regulation, adorns and dignifies the character of the merchant, without, it may be hoped, diminishing his usefulness, or interfering with the prosperous management of his affairs.

Besides the Lectures on the general principles of Chemistry, Mr. Henry delivered a course on the arts of Bleaching, Dyeing, and Calico-Printing; and to render this course more extensively useful, the terms of access to it were made easy to the superior class of operative artisans. It was at this period, that the practical application was made in France of a philosophical discovery to one of the Arts which Mr. Henry was engaged in teaching, that shortened, by several weeks, the duration of its processes. In 1774, Scheele, a Swedish chemist, distinguished by the number and great importance of his contributions to chemical science, discovered, in the course of some experiments on manganese, the substance known successively by the names of dephlogisticated marine acid, oxymuriatic acid, and chlorine. During several years afterwards, its properties were not applied to any practical use, until its power of discharging vegetable colours suggested, to M. Berthollet of Paris, its employment in the art of bleaching. The first successful experiments with that view were made by M. Berthollet in the year 1786, and with a liberality, which confers the highest honour upon him, he freely communicated his important results, not only to his philosophical friends, but to those who were likely to be benefited by them in practice. Among the former was Mr. Watt of Birmingham, who happened at that time to be in Paris, and who was the first person in this country to carry the discovery into effect, by bleaching several hundred pieces of linen by the new process, at the works of a relative near Glasgow. Mr. Henry, also, having received an indistinct account of the new method, but not knowing precisely in what it consisted, immediately set about investigating the steps of the operation; and in this he was fortunate enough to succeed. Soon afterwards, an attempt was made by some foreigners, who themselves had acquired their information from Berthollet, to turn the process to their own advantage, by obtaining a patent; and having failed in that, by applying for a parliamentary grant of an exclusive privilege of using it for a certain number of years. Against the former, a strong Memorial (which is now before the writer,) was presented by Mr. Henry to the Attorney and Solicitor General; and effectual opposition was made to the latter, by a public meeting of the inhabitants of

Manchester, on the ground that the whole process had been successfully carried into effect by Mr. Watt, Mr. Henry, and Mr. Cooper.*

Having satisfied himself of the practicability and advantages of the new method of bleaching, by carrying it on upon a scale of sufficient extent, Mr. Henry prepared to embark in a much larger establishment for the purpose. The connection, however, which he entered into with this view, having disappointed his just expectations, and the further prosecution of it being inconsistent with his professional employments, he abandoned the project; and contented himself with imparting the knowledge he had gained to several persons, who were already extensively engaged in the practice of bleaching, by the then established methods.

Mr. Henry had now reached a period of life, when the vigour of the bodily powers, and the activity of the mind, begin, in most persons, to manifest a sensible decay. From this time, however, though he did not embark in new experimental enquiries, yet he conti-

* The reader, who is interested in the history of the introduction of chlorine and its compounds into use in bleaching, is referred to a note in Dr. Brewster's Edinburgh Encyclopedia, art. Bleaching; and to Dr. Thomson's Annals of Philosophy, Vols. 6 and 7.

nued, for many years, to feel a warm interest in the advancement of science; and to maintain an occasional correspondence with persons highly eminent for their rank as philosophers, both in this and other countries.* His medical occupations had greatly increased, and, for a further interval of fifteen or twenty years, he had a share of professional employment, which falls to the lot of very few. This, and the superintendance of some chemical concerns, prevented him from attempting more than to keep pace with the progress of knowledge. He was in no haste, however, to claim that exemption from active labour, to which advanced age is fairly entitled, and it was not till a very few years before his death, that he retired from the exercise of the medical profession.

The summers of the years 1814 and 1815

* A considerable collection of letters to Mr. Henry from persons of this description has been preserved; but the subjects of them have, for the most part, been long ago brought before the public by their respective writers. The letters are chiefly valuable to the family of the deceased, as unequivocal proofs of the respect and esteem, felt towards him by those who were best qualified to judge of his merits. Many of them are from learned foreigners, with whom he had enjoyed opportunities of personal intercourse during their visits to Manchester.

were spent by Mr. Henry in the country, a mode of life, which, now that his season of active exertion was passed, was peculiarly suited to him, not only by the tranquil retirement which it afforded, but by its enabling him to indulge that sensibility to the charms of rural scenery, which can, perhaps, only exist in a pure and virtuous mind. His perception of these pleasures was at no period more lively, than after he had entered his 81st year. In a note, addressed to the writer of these pages, in the autumn of 1815, he describes, in animated language, one of those events, which so agreeably diversify the face of nature in the country. "Yesterday" he says, "we had one of the most beautiful appear-"ances in the garden I ever witnessed. Every "leaf—every petal—every projecting fibre—"was beset with a minute globule of water, "and when the sun shone upon the flowers "and shrubs, they seemed as if studded with "myriads of brilliants. The gossamer, too, "with which the hedges were covered, was "adorned with the same splendent appenda-"ges. The cause," he adds, "of this de-"position of moisture must, I suppose, have "been electrical."

The winter of the year 1815, which Mr. Henry passed in Manchester, was a season of greater suffering than was usual to him; for though of a delicate constitution, yet he happily, even at this advanced time of life, enjoyed an almost entire exemption from painful diseases. During this winter, he was much distressed by cough and difficult breathing, and his bodily strength rapidly declined. In the spring of the following year, he returned into the country, but not to the enjoyments which he had before derived from it. He was unable to take his customary walks, and was oppressed by feelings, which induced him to look forwards to the close of life, with the certainty of its near approach, but with calm and dignified resignation. The event, which he had anticipated, took place on the 18th of June, 1816, when he had nearly completed his 82d year.

In estimating the intellectual character and attainments of the subject of this memoir, it is proper to revert to a period, several years remote from the present, but still within the perfect recollection of many, to whom these pages are addressed. At that time, the quality of Mr. Henry's mind, which was perhaps most conspicuous, was a readiness of apprehension, that enabled him to acquire knowledge with remarkable facility. To this was joined a quickness in his habits of association, that peculiarly fitted him to perceive those analogies which, in chemical investigations, were chiefly relied upon as leading to the discovery of truth, before it was sought to be established on the firmer basis of an accurate determination of quantities and proportions. Without claiming for Mr. Henry the praise of great original genius, we may safely assert for him a very considerable share of that inventive talent, which is commonly distinguished by the term *ingenuity*. This was especially displayed in the neatness and success, with which he adapted, to the purposes of experiment, the simple implements that chance threw in his way; for it may be proper to observe that, at no period of his life, was he in possession of a well furnished laboratory, or of nice and delicate instruments of analysis or research. With these qualifications, he united a degree of ardour in his pursuits, which enabled him to triumph over obstacles of no trivial amount. And when it is considered that his investigations were carried on, not with the advantages of leisure, ease, and retirement, but amidst constant interruptions, and with a mind harassed by frequent and painful anxieties,—it will be granted, that he accomplished much more than might have been expected, from one so little favoured by external circumstances.

The acquirements of Mr. Henry were not limited to that science, in which he obtained distinction. It was the habit of his mind, when wearied by one occupation, to seek relief, not in indolent repose, but in a change of objects. In medical knowledge, he kept pace with the improvements of his time, and he occasionally, by original publications,* contributed to its advancement. He had a share of general information, and a flow of animal spirits, that rendered him an instructive and agreeable companion. To the rich sources of enjoyment, which are opened by the productions of the fine arts, he was extremely sensible, not so much from an acquaintance with critical rules, as from a natural and lively susceptibility of those emotions, which it is the object of the poet and the artist to excite. By the native strength of his memory, unassisted by any artificial arrangement, he had acquired a knowledge

* Chiefly in the periodical Journals, and in the Transactions of some Medical Societies to which he belonged.

of history, remarkable for its extent and precision; and he was always eager to discuss those questions of general policy, which are to be decided, partly by an appeal to historical evidence, and partly by a consideration of the nature of man, and of his claims and duties as a member of society. No representation of him would, indeed, be complete, that failed to notice the animation, with which he entered into arguments of this kind, or the zeal and constancy with which he defended his political opinions,—opinions which, in him, were perfectly disinterested and sincere, but which perhaps disposed him to allow more than its due weight to the aristocratical part of our mixed government. It would be unjust to him, however, not to state, that no man could more cordially disapprove, or more unreservedly condemn, every undue exertion of power; or could more fervently desire the extension of the blessings of temperate freedom to all mankind. It was this feeling, that led him to use his strenuous exertions as a member of one of the earliest Societies for procuring the abolition of the African Slave Trade; and when that great object was at length accomplished, he was affected with the most lively joy and gratitude on the downfal of a traffic, which had long been a disgraceful stain on our national character.

Of his moral excellencies, there can be no inducement to offer an overcharged picture to a Society, by many of whose surviving members he was intimately known and justly appreciated. Foremost among the qualities of his heart, was a warmth of generous emotion, which evinced itself in an enthusiastic admiration of virtue; in an indignant disdain and unqualified reprobation of vice, oppression, or meanness; and in the prompt and unrestrained exercise of the social affections. In temper, he was frank, confiding, and capable of strong and lasting attachments; quick, it must be acknowledged, in his resentments; but remarkably placable, and anxious, whenever he thought he had inflicted a wound, to heal it by redoubled kindness. No man could be more free from all stain of selfishness; more moderate in his desire of worldly success; or more under the influence of habitual contentment. This was in a great measure the result of his having early weighed the comparative value of the different objects of life, and of his steady and consistent pursuit of knowledge and virtue, as the primary ends of an intelligent being.

In very advanced age, though his body was enfeebled, his mind retained much of that wholesome elasticity and vigour, which always belonged to it. He was still enabled, by the almost perfect preservation of his sight, to spend a great portion of every day in reading; but, at this period, he derived greater pleasure from works of literature, than from those of science, and especially from his favorite study of history. During the winter immediately preceding his death, beside several standard historical works, he read with avidity one which had been recently published;* and entered into a critical examination of its merits, with a strength of memory and judgment, that would not have discredited the meridian of his faculties. In his moral character, no change was observable, except that a too great quickness of feeling, of which he had himself been fully conscious, was softened into a serene and complacent temper of mind, varied only by the occasional glow of those benevolent feelings, which continued to exist in him, with unabated ardour, almost to his latest hour. He still continued to receive great pleasure from the society of the young; and to them he was peculiarly acceptable, from

* Dr. Stanier Clark's Life of James the Second.

the kindness and success, with which he studied to promote their rational enjoyments. It was his constant habit to take a cheerful view of the condition of the world; and on all occasions, when the contrary opinion was advanced, to assert the superiority of the times in which he had grown old, over the season of his youth, not only on the unquestionable ground of an increased diffusion of knowledge; but on that of the wider spread of virtuous principles, and the more general prevalence of virtuous habits.

Without encroaching on topics, which are wisely forbidden by the rules of this Society, it may be permitted to me to state, that Mr. Henry was, from enquiry and conviction, a zealous advocate of christianity.—About the middle period of life, a change of opinion led him to separate from the established church, to whose service he had early been destined; and to join a congregation of Protestant Dissenters. But in discussing differences of religious belief, he was always ready to concede to others that free right of judgment, which he had claimed and exercised for himself; convinced, as he was, that no conclusion, to which the understanding may be led, in the honest and zealous search after religious truth, can, without the highest injustice, be made the ground of moral crimination or reproach.

Such is the view of the character of our late President, that has been taken by one, who, in forming it, may be supposed to have been influenced by feelings and recollections, not altogether favorable to an unbiassed exercise of the judgment. That it is coincident, however, with the estimate of others, from whom impartiality may be more reasonably expected, will appear from the following document, which, at the time when it was presented to the Society, declared the sentiments of all those members, who were in the habit of attending its meetings, or of taking an interest in its proceedings.

"To the Literary and Philosophical Society of Manchester.

"We, the subscribed, beg leave to present, to the Literary and Philosophical Society, a portrait of our President, painted by Mr. Allen, which, having been in a public exhibition, has been declared by

" competent judges, to be not only a correct
" resemblance, but likewise an excellent pro-
" duction of art. Our wish is, that a suit-
" able place may be assigned to it, in the
" room where our meetings are held; and,
" that if approved by the Society at large, it
" may be inscribed by them as an affectionate
" tribute of respect and gratitude to a man,
" universally beloved for his conciliating qua-
" lities and private worth, and peculiarly en-
" deared to us, by the relation in which he
" stands, as one of the very few founders of
" the Society, whom an indulgent Provi-
" dence has still spared to us;———a Phi-
" losopher, to whose talents we owe much
" of the approbation, which the public has
" bestowed on our labours;—and a Member,
" whose zeal has, for a period of nearly
" thirty years, been uniformly exerted, in
" every station, to promote the peace and
" prosperity of the Institution, over which
" he presides."

AN ESSAY

ON THE

SIGNS OF IDEAS;

OR, THE

Means of conveying to others a Knowledge of our Ideas.

BY EDWARD CARBUTT, M. D.

Physician to the Manchester Infirmary,
&c. &c.

(Read Dec. 13th, 1816.)

At *idola fori* molestissima sunt, quæ ex fœdere tacito inter homines, de verbis et nominibus impositis, se in intellectum insinuarunt. Verba autem plerunquè ex captu vulgi induntur; atque per differentias, quarum vulgus capax est, res secant; cum autem intellectus acutior, aut observatio diligentior, res melins distinguere velit, verba obstrepunt.

Bacon: *De Dignitate et Augmentis Scientiarum:*
Liber V. Caput IV.

THE subject of the paper which I have now the honour of reading before the society, is, the Signs of Ideas,—that is, the means employed, on the needful occasions, to impart to others a knowledge of the ideas of which we are conscious. In order, however, to be fully understood, it appears proper to explain, in the first place, with as much precision as possible, what is meant in this paper by the word Idea;—

a word which is used, both in conversation and in writing, with more vagueness, and with a greater variety of meanings, than almost any other.*

Every individual of the human species, is, at various times, conscious of various feelings, arising apparently from different causes. These feelings may be distributed into four principal classes, and designated as follows: 1. Simple and original sensations; 2. Remembered sensations, or Ideas; 3. Sensations of emotion; and 4. The Feeling of volition.

Simple and original sensations are those feelings which are occasioned,—by the application of external agents to the organs of sense, as light to the eye, aerial vibrations to the ear, odorous particles to the inner membrane of the nose, sapid bodies to the tongue and palate, heat, cold, and all sorts of resisting sur-

* The great, and, in most respects, justly celebrated Locke, throughout the whole of his essay, employs the word Idea in the most obscure and ambiguous manner. The consequence is, that many rise from the study of his work, which is really the production of a surprising genius, without finding that they have understood it. I have heard of one who, though he studied it for some years, complained that he never could ascertain what Locke meant by the word Idea. The fact is, that Locke made that term synonymous with the terms opinion, notion, principle, knowledge, theory, hypothesis, desire, instinct, habit.

faces to the skin;—by the usual and natural action of parts carried to excess, as when the muscles are fatigued;—by the want of certain matters necessary to the well-being of the system, as when we are hungry, or thirsty;—or, lastly, they either arise spontaneously, or depend upon causes which we cannot develope, as when we have the feeling of itching in the skin, aching in the bones, pain in various other parts of the body.

Such feelings as those which I have just described, and which I include under the term Simple and original sensations, have, each of them, their peculiar seat in peculiar organs or textures of the body.

Thus, the eye is so constructed as to refract all the rays of light proceeding from all the points of a luminous surface placed immediately before it, (provided they fall upon the cornea, or transparent coat,) and thereby to produce, from every point of the object, an impression or sensation of light upon a corresponding point at the bottom of the eye. No other part of the body is capable of this sensation. In like manner the sensation of sound is peculiar to the ear, of smell to the nose, of taste to the mouth. Thirst is felt in the fauces, hunger in the stomach, and the sensation of nausea is seated in the same organ. The feeling of fatigue is peculiar to the muscles. In the bones we have the sensation of aching; the sensation of griping is confined to the bowels. Heat and cold and the sensations produced by resisting surfaces, as hardness, softness, roughness, smoothness, figure, are almost peculiar to the skin, the internal parts being much less susceptible of them. Titillation and itching occur in the skin; lancinating pain is peculiar to serous membranes, as the pleura, and peritoneum. Tendons and ligaments are nearly insensible of cutting, burning, or pinching, but when twisted, or stretched, they evince an acute sensibility.

Now, with regard to the feelings which I have just enumerated, as occurring, on certain occasions, in various organs or textures of the human system, it is almost unnecessary to state, that, some of them are capable of returning when the agents which originally excited them are removed, whilst others are incapable of returning without a fresh application of the original cause.

When the sensations of any organ or texture are experienced whilst the objects or agents from which they first arose are absent, those feelings so experienced, are called Ideas, or Remembered sensations.

On such occasions, we know that the objects themselves are absent, partly by the comparative faintness of the feelings, and partly by the circumstance, that no corresponding feelings are excited in the other organs of sense. Thus, if I have the idea of a rose, I know that it is not an original sensation excited by the actual presence of a rose, because, if it were, in addition to the sensation of colour and figure by the eye, I should have the sensation of odour by the nose, and, if I chose, the sensations of smoothness, figure, magnitude, &c. by the skin.

But the comparative faintness is the principal thing that distinguishes an idea;—for sometimes ideas are so strong as to be mistaken for original sensations; and when, in any person, this mistake is frequent or permanent, we say that such person is insane.

In all cases then, except those of insanity, ideas or remembered sensations are much more feeble and indistinct than the original sensations, and commonly possess the less strength and clearness, the greater the period of time that has intervened since the application of the agents from which they have arisen; and with regard to this, it is an obvious fact, that ideas of the objects of some senses, arise with much more facility, clearness, and strength, and are much more durable than those of others.

It may be asked then, in reference to this circumstance,—Of which class of simple sensations, have we the strongest, most distinct, and most lasting ideas?

The answer is, Of those of sight. After the lapse of years we still retain nearly perfect ideas of persons and places which we have but once seen;—so Ovid, in his epistles, beautifully says,

> " Illa meis oculis species abeuntis inhæret,
> " Cum premeret portus classis itura meos."

which may be thus rendered,

> Still do mine eyes the dismal sight retain,
> When thy departing vessels ploughed the main.

The ideas of sounds come next in respect of vigour and durability. We remember sufficiently well the voices of our friends and acquaintances. We can repeat in our minds a variety of tunes which we have once heard, and receive pleasure from them without the actual production of any sound.

Whether we ever have ideas of sensations received by the nose and mouth appears to me uncertain; if we have, they are certainly much weaker and more transient, than those just mentioned, so that in a short time they fade entirely away.

Whether we have ideas of sensations received by the skin, seems to me still more likely to be determined in the negative.

Of the sensations experienced in the other textures and organs, it is, I think, quite certain that we never have ideas.

On what circumstances it depends, that we have ideas of the sensations of some organs and textures, and not of those of others, is entirely unknown.

Ideas, I may be allowed to add, succeed one another, in our consciousness, agreeably to certain laws, which are called the laws of association, and which may be reduced to these two:—1. Whatever ideas have previously followed one another, will have a tendency to follow one another again in the same order; 2. Whatever ideas are in any degree similar will be more apt to succeed one another than those which have no similarity. Derangements of association, when remarkable, are called insanity.

Sensations of emotion, are such feelings as are excited, not by the usual and appropriate causes of simple sensations, nor yet by simple sensations themselves, but by ideas, or trains of ideas, and they differ from one another, according to the peculiar train from which each has arisen. They consist, sometimes in a peculiar thrilling sensation, pervading the whole system, or some part of the system, and sometimes in a feeling of pain, anxiety, or oppression about the heart, stomach, or liver. They are, I believe, never succeeded by ideas having any resemblance to them, as is the case with simple sensations.

The Feeling of volition is that uneasiness by which we are excited to action at the prospect, either of any good which we wish to obtain, or any evil which we wish to avoid. This feeling of uneasiness is always irresistible, except by a feeling of greater uneasiness, produced by some prospect of greater good or greater evil. When the volition is remarkably violent, or unusually frequent, on any subject, it is called passion; when it is quite uncontrolable, and regardless of consequences, it is called mania, or raging madness; and when it is too weak to lead to the performance of the necessary actions of life, it is called melancholy.

Such are the feelings of which every human individual is, on the appropriate occasions, conscious: and I trust that I have given such explanations of each, as completely to distinguish them one from another.

But man, living in society, has frequently a desire, and even a necessity, to impart to his fellows some knowledge of the feelings of which he is, on any particular occasion, conscious; and the feelings, of which he has most commonly to give an account, are ideas. It is obvious that, to accomplish this end, the means might have been various. "Whatever," says Lord Bacon, "is capable of sufficient differences, and those perceptible by the sense, is, in nature, competent to express cogitations. And therefore we see in the commerce of barbarous people, that understand not one another's language, and in the practice of divers that are dumb and deaf, that men's minds are expressed in gestures, though not exactly, yet to serve the turn."*

What I have to say on the subject of gestures, as the external signs of ideas, I reserve, for particular reasons, till afterwards.

The various means which might have been used by men to express their ideas, are, in general, superseded by the employment of vocal sounds, which are, in a number of ways, so varied, modified, and combined, as to form words.

* Of the Advancement of Learning, Book 2.

Written words, it may, for the sake of precision, be observed, are merely the representatives of spoken words; and therefore, except in some certain respects, arising from the necessary imperfections of written language, no distinction is required between them.

It is by no means my intention, I may be allowed to remark, to enter, at present, into the general theory of language. That is the province of the grammarian. My only object, on this occasion, is, to show in what way words may be considered as competent to express ideas, and in what respects they are deficient.

It is a common opinion, and a very frequent remark, that the use of words is to communicate ideas. Aristotle called them παθηματων συμϐολα, that is, the symbols or images of feelings, or, as the more usual expression is, the signs of ideas. That they are intended to be such, cannot be doubted; and they may, on some occasions perhaps, sufficiently answer the intention; but, there can be little doubt, that, in a great number of instances, they fall far short of accomplishing the end. From not sufficiently considering this important fact, much error, and much fraud, pass every day unnoticed, and unsuspected, mankind are deceived by words, and bow the knee to sounds more empty and unmeaning than the whistling of the idle wind.

"Mankind in general, " says one of the acutest men whom modern times have produced, " are not sufficiently aware, that words *without meaning*, or of equivocal meaning, are the everlasting engines of fraud and injustice."* Nothing then can be more useful, than to accustom ourselves to examine narrowly for what ideas a word stands, or may stand; and, if we find it to stand for none at all, or to be employed, on various occasions, to express ideas of the most discordant kind, let us be careful of suffering ourselves to be deceived by it, or by those who use it. All words, however, are not to be rejected because they do not represent ideas. In the sequel, I shall endeavour to shew that there are some words of this kind, the use of which is proper, and necessary.

The only sources of our knowledge, it is certain, are our own feelings. Language is, or professes to be, employed in giving names to these, or to use the more common mode of speaking, in expressing our ideas. The real and only intelligible meaning of this phrase, must be, that it is employed to excite, in the

* Επεα πτεροεντα. Vol. 1. page 75.

minds of the hearers, the same feelings as those of which we ourselves are conscious. But, as was remarked by the wisest of mankind, two hundred years ago, " It is not a " thing so easy as is conceived, to convey " the conceit of one man's mind into the " mind of another, without loss or mistak- " ing."* There is, I apprehend, only one case in which it can be accomplished; in all others, we must in a greater or smaller degree fail in our object.

When we are employed, for instance, in mentioning the names of individual objects, which are equally known to our hearers as to ourselves, and which are of such a nature as to be liable to little or no change, we may, in most instances, be certain, (provided we take care always to apply the same name to the same individual,) that all who hear us, have excited in them exactly the same set of ideas as we ourselves have. Thus, in speaking by name of any particular horse, or house,

* Bacon: Of the Interpretation of Nature, chapter 18. He adds, " especially in notions new, and differing from those that are received." In his aphorisms he says; " Neque etiam tradendi aut explicandi ea, quæ adducimus, facilis est ratio; quia, quæ in se nova sunt, intelligentur tamen ex analogia veterum." Aphor. xxxiv.

or person, all who are acquainted with that person, or that house, or that horse, (allowing that no change has taken place in those objects,) will have similar ideas raised by that name.

When speaking, however, of objects of a different kind, such, for instance, as are aggregates for ever varying in their mutual relations, number, form, and appearance, such as are liable to rapid changes, or such as have never been witnessed by both the hearer and speaker, the chances are as infinity to one that no two persons are contemplating the same set of ideas. This may be observed to be in a remarkable degree the case in our conception of poetical descriptions, and thus, we see, why, imitation apart, any character of a drama must be differently conceived, and of course differently represented, by every new actor who undertakes it.

Moreover, such is the necessary mechanism of language, that it is impossible for us to have a name proper for each individual idea, or set of ideas. The business of life could not, with such an arrangement, have gone on with the requisite rapidity. On this account, names have been generalized; and when the appellations tree, horse, man, had been given to one individual of each of these kinds, it naturally came to signify any individual of that kind. Hence the origin of general terms, which, by a strange perversion, have been stated to stand for general ideas; whereas a general idea cannot possibly have any existence: all ideas are, and must be, particular.

Let us then briefly examine in what respects such general terms are to be considered as the signs of ideas. Imagine a speaker addressing a numerous assembly, and using the word triangle, with the intention of elucidating some of its properties. I suppose him to use no diagram, but to be proceeding altogether mentally. Let him be demonstrating the twentieth proposition of Euclid, that in every triangle any two sides, however taken, are together greater than the remaining side. Now, I assert that the probability is almost infinite, that no two of his hearers have exactly the same feeling or image raised by the teacher, when using the word triangle. One will see, in his mind's eye, an equilateral triangle, another an isosceles, a third a scalenum; one may conceive a right-angled, another an obtuse-angled, and another an acute-angled triangle. With regard to size, there will probably be a still greater variation in the images. Neither is it necessary that the hearers should present to themselves exactly the same image or idea of a triangle, because, if the proposition be true, it must be equally true of all possible triangles. The same remarks are applicable to every instance in which general terms are used, and the probabilities against uniformity are much increased in cases where the terms are not only general, but exceedingly complex; as in the words ship, army, city, &c.

There is a great number of words in common use, which are to be considered as merely abbreviated expressions for various trains of ideas, although they are generally considered as standing, each for a specific and appropriate idea. Let us examine a few of them in order.

Take for instances, the words power, time, gratitude, which may serve as examples of a great many others of the same nature. When we hear the word power mentioned, what are the ideas excited? Every different hearer will probably have a different set passing through his mind, when he thinks upon this word, with a view to determine for what it stands. Each will endeavour to recollect instances which he has observed of the exertion of power. One will think of a steam-engine, and will say that the working of that machine is power; another will perhaps tell us that the effect of the wind in impelling a ship through the sea is power; another will repeat the trite, but not less correct, and beautiful saying, that "Knowledge is power." In short, such words as the word power, are abbreviated terms, which may stand for almost any set of ideas which the hearer chooses.

When I mention the word time, one hearer will think of the progress of the sun from east to west; another of the increase and waning of the moon; a third of the motion of the shadow on the sun-dial; and every different hearer will have a different set of ideas. So that of the word time we have no single idea; it evidently stands for any one out of numerous trains of ideas, representing various phenomena, which we have, on different occasions, witnessed.

When the word gratitude is mentioned, I endeavour to recollect instances which I have observed of what is commonly called gratitude. I see, in my mind's eye, one person conferring a kindness, and the other soon after returning it. So that the word gratitude serves merely to excite ideas of certain transactions to which we apply that term.

The words memory, imagination, judgment, if rightly understood, will be found to be merely abbreviated terms, expressive of certain operations which occasionally take place in the human mind.

Those kinds of words, usually called conjunctions and prepositions, were, till very lately, considered as not of themselves expressive of ideas. Locke says, "they are not truly, by themselves, the names of any ideas;" and Harris says they are words devoid of signification. But that most acute of men, the late John Horne Tooke, has shewn, in the clearest manner, that words of this kind are really contractions and representatives of other words. Those other words are, in general, abbreviated complex terms expressive of a number of ideas. Thus, the conjunction *and* signifies to add, and to add is an abbreviated term to express the placing of one thing with another, either in fact or imagination. In the same manner, the preposition *with* signifies to join, which is a like abbreviated term.

It may be here observed, in passing, that what are, in the technical language of grammar, called verbs, are to be considered in general as complex terms, signifying, along with their nominative, ideas of peculiar modes of existing, acting, or suffering of some person or thing. Of this kind are the verbs to stand, to sit, to walk, to fly, to sleep, to strike, to be struck, to hold, to be held.

There is a numerous class of words which have generally been considered as abstract terms, and which in fact stood for any idea, or no idea at all, at the option of the speaker or hearer. Such are the words right, wrong, truth, just. Words of this kind were formerly said to stand for abstract ideas. What an abstract idea was, we were never informed; of course the words might stand for any thing or nothing without danger of detection.

Mr. Tooke,* however, has most clearly shewn that these, and other words of the same kind, are merely, to use the common phraseology of grammar, past participles of certain verbs; which verbs are truly abbreviated, complex terms, expressing certain actions. Thus, right is from rectum, the past participle of rego, to rule; therefore every thing ruled or laid down by the proper authority is right. Wrong is from wring, to twist, that is, to turn from the right. Truth is that which is trowed or believed. Just, is that which is commanded, (from jubeo, jussi, jussum;) but then it must always be understood that the authority which commands, is of the

* Επεα πτεροεντα. Vol. 2.

highest kind. The highest authority, of course, is the law of God, and next to that, the law of the community in which we live.

Let us now proceed to the consideration of words which in nowise represent ideas. These form a very extensive class. They are of three kinds: first, words which from the nature of things are altogether devoid of archetypes; secondly, words standing not for ideas, but, either for such simple and original sensations, as are never *ideally* renewed, or for sensations of emotion, which are never called up except by the original cause, and cannot therefore in any case be called ideas; thirdly, words standing for causes whose effects alone we witness, and thence judge the existence of the cause, without being able to form any conception of it; and fourthly, words which at present have no ideas attached to them, although the contrary might have been the case when they were originally brought into use.

Of the first kind are the terms point and line, as used by mathematicians. When we hear the definition of a mathematical point, is any idea raised in the mind? Certainly not; or at least none that corresponds with the definition. It is necessarily the same with the mathematical definition of a line. The use of these words is, however, both proper and necessary, for without them the mathematician could not proceed a single step, and in the way in which they are used they do not occasion any error.

With regard to the second kind, it will be recollected, that in an early part of this paper, I mentioned that some of our simple and original sensations, as those of the eye, and the ear, most strongly, and those of the nose, and mouth, more weakly and transiently, were capable of recurring when the objects, from which they originally arose, are absent; whilst the sensations of other organs and textures, and also those sensations called emotions, are incapable of returning, without a fresh application of the original cause.

It is evident, then, that when we are reasoning concerning objects, (or, what is precisely the same thing, sensations,) of the two former classes, those of sight and hearing, the objects themselves being absent, we are, or may be, actually conscious of the ideas to which they have given rise. But, with regard to the simple sensations of other organs and textures, and also with regard to sensations of emotion, we generally consider, not the ideas, (for these we either never have at all, or at least, as in the case of the nose and mouth, we never have them distinctly or permanently,) but merely the words which are the names of the original sensations. And from these words we reason quite as correctly as from real ideas, because, having formerly observed and compared the effects of these sensations upon ourselves and others, we are enabled to speak concerning them, without error; for *they* in truth, and not the sensations themselves, form the general subject of such reasonings.

Even of visible and audible objects, the ideas are not always necessary to correct reasoning. The signs of these ideas are, for many purposes, quite sufficient, as was fully shewn in the instance of Blacklock, a poet of no despicable pretensions, who became totally blind before he was six months old.

> " Nature," as he himself says, " when scarce fair light
> he knew,"
> " Snatch'd heaven, earth, beauty, from his view,
> " And darkness round him reign'd."

He has, however, described with much correctness, and apparent feeling, the various appearances of objects. His poems contain innumerable instances of the appropriate use of words, intended to signify ideas to which he must have been an entire stranger.

It is perfectly true then, as asserted by Mr.

Locke, that we frequently think by words, and not by ideas. Yet even in the cases where we say that we think by words, we must recollect, that it is only by the ideas of words;—that is, either of written words, as received by the sense of vision, or of spoken words, as received by the sense of hearing. So that, in every case, thought consists in the consciousness of ideas.

Of the third kind of words, not representing ideas, are such as are the names of things having, as we suppose, (and, no doubt in many cases, correctly suppose,) a real existence, though we certainly can form no conception of them. Of this kind are the term cold, when used for a real material agent, and not for the name of a sensation, the word heat, used in the same manner, the electric fluid, the galvanic fluid, the principium vitæ or principle of life, of some medical philosophers, and the nervous fluid, of others. With respect to the sacred name of the Supreme Being, much might be said, were it not a subject too solemn for us lightly to dwell upon it in the illustrating of our comparatively trifling speculations. It may be remarked, however, that though all mankind, probably without exception, are convinced of the existence of such a Being, no one, we may safely say, has ever been able to form any idea of him. "For as all works," to use the "language of Lord Bacon, "do shew forth "the power and skill of the workman, and "not his image, so it is of the works of God, "which do shew the omnipotency and wis- "dom of the maker, but not his image."* There are other words of the same kind, such as mind, spirit, &c. which, although they are to be considered as the names of things, that in all probability have a real existence, yet of those things we cannot form any idea. And it may be asked, why can we form no idea of them? Because they are such as have made no impressions on our organs of sense.†

The fourth division includes not only those words, which, as generally employed, do ne-

* Of the Advancement of Learning, Book 2. "Sicut enim opificis potentiam et peritiam ostendunt opera ejus, imaginem autem minimè; sic opera Dei, conditoris omnipotentiam et sapientiam ostendunt, imaginem ejus haudquaquam depingunt." De Augmentis Scientiarum. Lib. iii. Cap. ii.

† Our knowledge, on such subjects as those above mentioned, is evidently obtained, not by impressions on the senses, but by various processes of reasoning;—it seems possible, however, that there may exist created beings, gifted with other senses, by which they are able to acquire ideas of those existences that are known to us only by inference.

ver represent any set of ideas; but also those which are, on certain occasions only, entirely destitute of meaning. A prominent instance of the latter kind is the expression used in the ceremony of drinking healths; which though formerly it might be a proper pledge of mutual protection from the danger of assassination, has long ceased to have any possible meaning. Various similar instances are also to be found in the terms used by medical and other theorists; and it was to such probably that Lord Bacon alluded, in the following passage; "And it is common with men, that if they have gotten a pretty expression by a word of art, that expression goeth current; though it be empty of matter."*

Oaths and curses, in common discourse, are to be ranked among the words devoid of ideas. Steele in his "Prosodia rationalis" gives a reason for their employment, which is probably unique. He supposes that we have an instinctive sense of rhythmus derived from the regular beat of the pulse and other natural causes. He says that "the beating of our "pulse, which we feel whenever we are si- "lent and inactive, prones us to rhythmical "divisions even in the series of our thoughts;

* Sylva Sylvarum, Cent. 2. Exp. 124.

" —as soon as we begin to move, our steps
" succeed in the government of rhythmical
" pulsation. The swing of the arm, and other
" such motions, made by public speakers, are
" derived from the instinctive sense of rhythmus, and are, in effect, beating time to
" their orations. Also cursing, swearing,
" and many other unmeaning words, so frequently interwoven in common discourse,
" are merely expletives to fill the measure,
" and to round each rhythmical period."*

Of words, which, as usually employed, are not the representatives of any specific set of ideas, instances are to be sought principally among those derived and compounded from the foreign and learned languages. Of this kind is the word blasphemy, which, not being the sign of any peculiar or appropriate idea or set of ideas in the mind of most speakers, will stand for any thing which they choose to call by that term. Superstition is an instance of a word, in the application of which hardly any two persons will agree. Heresy is another. In fact, to endeavour to point out every word of this kind, would be an endless task; for, as the English language is formed from so many others, and has received so great a number of its words

* Prosodia rationalis. London, MDCCLXXIX, page 118.

in a compound state, how are such words to excite ideas in the minds of any, except those who understand the primitives? Hence one advantage of the study of languages: for, most assuredly, if we would not be deceived by sounds, we shall have frequent occasion to recur to the touch-stone of etymology.

Mr. Dugald Stewart, however, has asserted that, "he has hardly met with any individual habitually addicted to etymological studies, who wrote his own language with ease and elegance." Yet, (whatever we may think of this opinion,) we may venture to affirm, that, without much etymological knowledge, no one can write in a modern language with correctness. And who is there, gifted with common sense, that would not prefer a precise, unequivocal, and, if I may use the expression, transparent style, before a string of harmonious and smoothly flowing words, which, being the types of no precise ideas in the mind of the writer, are not likely to excite any in the mind of the reader. This species of unmeaning eloquence, has been but too much and too long in vogue, and those who feel an interest in defending it, would do well to consider whether the ear will not be still more agreeably affected by music, properly so called, and the mind almost as much instructed. All for which a wise man will be anxious in regard to style, is correctness and intelligibility; every thing else is comparatively insignificant. The difficulties of the art of writing, appear to me to have been always much exaggerated. Two simple rules may supersede all the treatises of rhetoric. The first is, to understand the subject; the second, to be content to write with plainness and perspicuity.*

A very few words respecting gestures, will conclude this subject. Gestures are almost infinite, in number and meaning; one

* With regard to the study of words, and of language, we ought to draw a line of distinction. Nothing can be more useful, than that study of words which is bent upon discovering their meanings, and proper uses, so as to prevent ourselves from being deceived by them, and to enable us to avoid deceiving others. At the same time, hardly any thing is more contemptible, than "the hunting after "words more than matter," as Lord Bacon expresses it; the "seeking more after the choiceness of the phrase, and "the round and clean composition of the sentence, and "the sweet falling of the clauses, and the varying and il-"lustration of their works with tropes and figures, than "after the weight of matter, worth of subject, soundness "of argument, life of invention, or depth of judgment." (Of the advancement of learning, Book 1.) This was, in a great measure, the employment of the learned, about the 16th century, when hardly any thing was studied, except the works of orators and rhetoricians. At that time, a

gesture repels, another invites; one applauds, another reproves; one is intended as a demonstration of respect and veneration, another of gratitude.

Gestures may, in general, be considered as abbreviated modes of representing certain sets or trains of ideas. For instance, when a scolding woman clenches her fist, and shakes it at her opponent, it is evident that she means to threaten chastisement for her real, or supposed injuries. When a supplicant presents his joined hands placed before him, he intimates his willingness to give himself up to be bound. When the Oriental muffles up his face, and falls prostrate to the earth, he intends to signify reverence and humility;—reverence, in not daring to look on the face of his superior, and humility in his choice of the lowest situation in which he can possibly place himself. The shake of the hand is evidently intended as a token of union, though of the most distant kind; it is probably merely a refinement of the embrace.

man would spend his life in the study of one author, whence the expression, "homo unius libri!" Then Erasmus, with his usual keenness of wit, made the scoffing echo, Decem annos consumpsi in legendo Cicerone: and the echo answered in Greek, saying, Ὀνὲ.

There are some gestures, which seem to be mere arbitrary signs, and not, like those mentioned above, natural expressions of ideas, but which are of so long a standing, that it is almost impossible to trace their origin. Of this kind, is the uncovering of the head, which, although among Europeans, the most usual mark of respect, is probably the remains of some ancient custom. At present, the practice is without the shadow of a meaning, except as an arbitrary sign of respect. Of the same kind, is the custom of carrying the hand to the mouth by way of salutation; the origin and meaning of which seem involved in obscurity. Mr. Gibbon* says, that it is the root of the latin word adoro, adorare, and that from the first books of Herodotus, it appears to be of Persian origin. But, upon looking into Herodotus, at that part where he describes the Persian modes of salutation, I find he mentions nothing of the custom in question.

The various expressions of the countenance, arising from the action of the muscles of the face, may be considered as so many gestures, which are the signs of certain ideas, or trains

* Decline and Fall of the Roman Empire, Vol. 10. Page 124. Note.

of ideas, or in some cases, of certain sensations, at the time actually experienced, as is to be observed in the case of pain. These seem to be, in general, natural expressions, inasmuch as they appear to be similar in nations, that have had no intercourse with each other. The Frenchman's shrug of contempt seems to be peculiar to him, but the expressions of contempt, approbation, pleasure, displeasure, on the countenance, are confined to no nation; they seem intelligible to children and to brutes, and to approach to the nature of instinctive actions, for which we can assign no reason, except that we are irresistibly impelled to the performance of them.

ACCOUNT OF

SOME REMARKABLE FACTS

OBSERVED IN THE

DEOXIDATION OF METALS,

Particularly Silver and Copper.

In a Letter to Mr. Dalton.

BY SAMUEL LUCAS, ESQ.

(Read March 6th, 1818.)

DEAR SIR, *Sheffield, May* 31*st*, 1815.

WHEN I had the pleasure of seeing you in Manchester, I mentioned having observed, that pure silver, when melted and while in a fluid state, had the property of uniting with a small proportion of oxygen, not only from the atmosphere, but also from other bodies which gave it out at a suitable degree of heat, as some of the nitrates for instance; and that the oxygen thus absorbed remains united with the silver only so long as it continues in a fluid state, or while fluid, until some substance be applied, having a more powerful attraction for the oxygen. In proof of this I now send, for your inspection, a few specimens of silver that has been in the different states, and which carry the external marks; and also a bottle of the gas collected from silver, which had been exposed to the influence of the atmosphere by cupellation.

If silver in large quantities, after having been exposed in a melted state to a current of oxygen gas or atmospheric air, be allowed gradually to cool, the surface first becomes fixed or solid; this soon bursts, ebullition ensues, and an elastic vapour in considerable quantity escapes, driving before it a portion of the internal fluid metal, which becoming solid as it is brought to the surface, produces the protuberances as shown by the accompanying specimen, No. 1. This ebullition continues from $\frac{1}{4}$ to $\frac{1}{2}$ an hour or more, according to the quantity of silver, and the rapidity with which it is cooled.

If, instead of cooling gradually, it be made to assume the solid state suddenly by pouring it into water, still the same phenomena occur; an ebullition takes place, and oxygen gas is evolved; but as the silver is so much divided and passes so suddenly from the fluid to a solid state, the protuberances are proportionably minute, and are spread more equally over the whole surface, as will be seen in specimen No. 2.

No. 3, shews the arrangement of crystallization, which the silver assumes when the gas is separated from it, during the time of its becoming solid.

I have before observed, that substances having a powerful affinity for oxygen, will take it from the silver even while in a fluid state. Thus, if charcoal be spread, for a few moments only, on the surface of silver that has absorbed oxygen, the whole of the oxygen will immediately be taken from it; no ebullition or escape of gas occurs, whether it be cooled gradually, as in specimen No. 4, or when poured into water, as in No. 5. By comparing these two specimens with No. 1 and 2, a very great difference will be observed, which is occasioned wholly by the escape of gas from the latter, while no such circumstance attended the former.

The bottle of gas which you will receive herewith, was collected in the following manner. Some silver, after cupellation, till in a state of perfect purity, was poured by a few pounds at a time into a vessel containing about 30 gallons of water, and an inverted bottle previously filled with the water, and with a funnel in its mouth being instantly placed over the silver, as it was each time poured into the water, the gas as it was given out and arose from the silver, was thus collected in the bottle until it was filled.

Care is necessary, that the neck of the bottle be kept below the surface of the water, to prevent the access of atmospheric air, and I am not very certain that there is not a little admixture.*

In addition to the above, I have inclosed two samples of copper, in two different states, both, however, equally pure, except that the one is believed to be combined with oxygen, and the other not.

No. 1. is a sample taken from a furnaceful of about 5 cwt. when in a melted state, and which had been exposed uncovered to a current of atmospheric air for about 2 hours before and during the time it was melting. This, when poured into water, exploded most violently, as will be seen by the small, which was attempted to be granulated.

The specimen No. 2, is a sample from the same copper, after the surface had been covered with charcoal, for about half an hour. This you will perceive, is in a very different state from the other, and when poured into water, granulated without any explosion, as the small bits will show.

I remain, dear sir,

Your obedient servant,

SAMUEL LUCAS.

* I found this gas to contain 86 or 87 per cent. of oxygen. J. D.

OBSERVATIONS

UPON

THE CALLOUS TUMOUR.

BY

MR. KINDER WOOD.

(Read Dec. 12th, 1817.)

I HAVE been induced to make some dissections, and to enquire into the subject of the present paper, in consequence of the various accounts given by authors of the origin of that effusion succeeding to fracture of a bone, termed callous. How far the subject will receive illustration, I will not presume to determine; the enquiry was originally made for the purpose of clearing up my private opinions, and having no particular theory to support, I have only been solicitous to draw plain inferences from the facts.

Of the appearances found immediately after the fracture of a bone.

EXP. 1. I took a young rabbit and fractured the bone of the thigh; in six hours afterwards the animal was killed, and the injured parts examined. The bone overwrapped considerably, the faces of the fracture were sharp and irregular, the surrounding muscles much lacerated, and a considerable quantity of blood effused into the cellular membrane.

This observation describes a state of the injury previous to the commencement of any action of reparation, and stands as the point from whence to commence our observations. The swelling found at this period, arises from extravasation of blood, and is essentially different from a succeeding tumefaction, the consequence of increased action of the blood-vessels, and previous to which this bloody extravasation is considerably diminished by the action of the absorbent vessels of the part. The asperities of the fractured faces are to be carefully noted, since they are the chief cause of the laceration of the muscles, constantly present in a greater or less degree, and possessing a considerable influence over the termination of the case. In all the succeeding experiments, overwrapping of the bone was constantly found, and in the fracture of long bones it is rarely absent unless prevented by some mechanical cause. Betwixt the infliction of an injury, and the commencement of a curative process, a certain portion of time always elapses, the powers of the constitution seem to suffer an immediate depression in proportion to the violence and extent of the local injury, and to those exertions which will be required to perfect a process of reparation. In the case of simple fracture, these actions of reunion commence so early as within the twelfth hour.

Of effusion from the internal membrane.

Exp. 2. Twelve hours after fracture.—The muscles surrounding the injury had suffered severely; no perceptible changes had taken place in the external membrane; the fractured faces were covered and made beautifully smooth by an effusion of coagulable lymph firmly adherent to the internal periosteum, and evidently proceeding from that membrane.

This capping or smoothing of the face of the fracture is one of great moment, since by it the asperities of the injured bone are removed, and further violence to the surrounding soft parts prevented. When a fracture is going on well, this effusion may always be early found, by examining the parts after destroying the animal; it adheres firmly to the face of the fracture; it is also very constant, even when the fracture goes on ill, at which time it lies looser upon the face of the fracture, is scantier, and when seized with the forceps, comes away, bringing along with it a portion of the internal membrane, and leaving the cavity of the bone partially denuded, thus demonstrating the source from whence its origin is derived. This is the first effusion, and although the situation of the internal membrane is such, as to prevent our demonstrating the increased action of its vessels, except by delicate and fortunate injections, yet the appearance of this effusion is proof that such a state of them exists. Increased action therefore of the vessels of the internal membrane of the bone, and effusion of coagulable lymph from them, capping and smoothing the faces of the fracture, may be considered the first process towards the reparation of the injury.

Of effusion from the external membrane.

Exp. 3. Twenty-four hours after fracture.—The extravasated blood was of a dark colour; there was considerable laceration of the surrounding muscles, which were vascular and tumefied; the external membrane was thickened, and the asperities of the fractured faces were removed by an effusion from the inner membrane.

Exp. 4. Thirty-six hours after fracture.—There was slight appearance of extravasation; the injured muscles were tumid and very vascular; the external membrane was thickened and vascular; a considerable effusion of coagulable lymph, evidently proceeding from the thickened external membrane, and also from the injured muscles, surrounded and enveloped the overwrapping portions of bone; the fractured faces of which were smoothed and capped by effusion from the internal membrane, of a more compact and dense organization than the secondary effusion just noticed.

From what has been stated, we have to notice the greater aptitude which exists in the internal membrane than in the external to the effusion of coagulable lymph; hence the one may be termed the primary, the other the secondary effusion. In the secondary effusion the capped surfaces of the fracture lie embedded; the effusions do not run into each other, but present a distinct and observable difference; this distinction evidently depends upon the difference in point of time, at which the effusions are thrown out, in consequence of which, the one is in a more advanced state of organization than the other, a circumstance which perpetuates, till near the completion of the process, this separate appearance of the two effusions, the obliteration of which is one of the latest circumstances occurring.

It is this tumour, in the first instance so diffused and yielding as to be distinguished through the integuments with difficulty, but becoming progressively more defined as it becomes more organized, that has been termed the callous tumour, and of whose origin so many opinions have been formed.

When the process goes on favourably, no changes are found, except that of an increased secondary effusion, till the fifth or sixth day, when the tumour becomes more firm and solid, cutting rather like the intervertebrary substance than like cartilage, and is found more dense as the knife approaches the external membrane of the bone, to which it adheres, with an extreme firmness. It is not necessary to detail singly these examinations, they would extend this paper too far; it is only necessary to detail leading appearances.

When the process of reunion goes on unfavourably, whether the causes are constitutional or local, their effect is such as to diminish or destroy effusions so necessary to recovery; it will therefore be easy to shew the necessity of them from the consequences arising out of their scanty formation, or destruction when formed.

Of the failure of effusion, from both the external and the internal membrane.

Exp. 5. Sixty hours after fracture.—The animal was languid and refused food; the limb felt flabby, and no tumour encompassed the fracture. Upon examining the limb, both portions of the fracture were found lying immediately under the common integuments, and denuded of external membrane; above the denudation the membrane was thickened, and its blood vessels numerous, enlarged and turgid; the internal membrane of each portion was diseased and easily separated from the bone, leaving the canal bare. There was no effusion from the external membrane surrounding the injury, nor any from the internal, smoothing the face and asperities of the fracture. The adjacent muscles were highly diseased, and a bloody sanious fluid was extensively diffused in the cellular membrane.

Exp. 6. One hundred and thirty-two hours after fracture.—The fractured ends protruded through the skin, denuded of external covering. The internal membrane protruded slightly from the canal of the bone, loose, unattached, and coming away readily upon being seized with the forceps. The soft parts were one extensive mass of disease; there was no effusion of coagulable lymph in the neighbourhood of the injury, but a copious secretion of thin offensive and bloody water.

These two dissections shew an entire failure of actions conducive to a reunion. Disease of the membranes to the extent here found, always effects the death of a portion of the bone; we find no healthy effusions, the fluid surrounding the fracture being always such as is the consequence of an ulcerated or gangrenous state of the soft parts, viz. pus, or a more sanious and watery secretion. The denudation and death of a portion of the fractured bone acts as a source of increasing irritation producing absorption of the soft parts, and thus facilitating a second process of nature for perfecting a cure by the protrusion of the dead bone through the skin, and its separation from the sound and healthy parts with which it is connected.

After the amputation of a limb, affections similar to the above occasionally occur, when, instead of healing favourably, the surgeon is disturbed by a flabbiness of the stump, and a strong tendency to contraction, rendering stitches or adhesive bandages of no avail and succeeded by a protrusion of denuded bone, either through the wound or some part of the common integuments; this flabbiness and tendency to contraction have commonly been attributed to disease of the soft parts, but are undoubtedly symptoms of disease in the membranes of the bone, and especially of the periosteum internum, in consequence of which, the cut face of the bone lies loose and unconnected in the wound, uncapped by effusion, and presenting no fit medium for the reciprocal extension and communication of blood vessels, hence Dr. M'Donald, as quoted by Mr. Gibson in the 6th Vol. of the Transactions of this Society, found death of the bone to ensue, when the medulla was extracted, and the cavity of the bone filled with dry lint.

Of deficient or scanty effusion from the external membrane.

Exp. 7. Eighty hours after fracture.—The muscles were considerably lacerated; there was a copious effusion of blood and of bloody water in the neighbourhood of the fracture.

The lower portion of bone lay embedded in the flexor muscles, which were highly diseased, its external membrane was thickened in irregular patches, the fractured face was covered with its proper effusion. The upper portion of the fracture had its external membrane inflamed and uniformly thickened, its fractured face was completely capped and smoothed, a slight effusion of coagulable lymph was thrown out by the external membrane of this portion of bone, but none from the lower portion.

Exp. 8. Ninety hours after fracture.—This bone was much splintered; the muscles were much injured, and a bloody water was thrown out into the cellular membrane. A small portion of muscle was interposed betwixt the overwrapping bone. No effusion covered the face of the fracture of the lower portion of bone; the external membrane was diseased, and the bone denuded in irregular patches, it had made its way through the soft parts, and lay immediately under the skin; the face of the superior fractured part was covered with its proper effusion, which adhered strongly to the internal membrane; the external membrane was vascular and thickened, it had thrown out only a very slight effusion; the soft parts near this part of bone were less diseased than those in the neighbourhood of the lower portion.

Exp. 9. One hundred and four hours after fracture.—The animal refused food, and became dull and languid after the injury. The soft parts in general were diseased; there was little extravasation of blood or effusion of bloody serum; the fracture was very oblique, and a portion of muscle interposed betwixt the overwrapping bone. The face of the superior fractured part was only partially covered with effusion; its external membrane was inflamed and thickened, and had thrown out a slight effusion of coagulable lymph in its immediate neighbourhood. The lower portion lay immediately under the skin, was only partially covered by effusion from the inner membrane; the external membrane was diseased, and easily separable from the bone, and had thrown out no healthy effusion.

Exp. 10. One hundred and eighty hours after fracture.—There was no feeling of tumour upon examining externally. The soft parts surrounding the injury were highly diseased; and a considerable watery effusion was found in the cellular membrane; there was no effusion of coagulable lymph from the external membrane, or in other words, there was no callous thrown out by the injured parts; the fracture lay loose and unconnected in a mass of disease; the lower portion of bone was denuded of its external periosteum near the point of fracture; a slight effusion from the inner membrane lay loose upon each fractured face, upon pulling which, the membrane came away, leaving the canal of the bone denuded.

Exp. 11. One hundred and sixty hours after fracture.—The superior part of the bone protruded through the skin, denuded of its outer covering; the soft parts in its neighbourhood were much diseased. The inferior portion of bone was in a more healthy state; it was capped with an effusion from the inner membrane; the outer membrane was inflamed and thickened, and had thrown a little effusion into its immediate neighbourhood, the limb was œdematose.

Exp. 12. One hundred and eighty-four hours after fracture.—Both portions of bone were denuded of outer covering near the point of fracture; above the denudation, the membrane was inflamed and thickened, but no effusion of coagulable lymph was thrown out around the injury. A slight effusion from the inner membrane lay loose over each fractured face. The soft parts were a confused mass of disease.

The experiments from 7 to 12, shew disease of the external membrane with its consequences, deficient effusion, denudation of the bone, ulceration of the soft parts, &c. &c. In these dissections we may observe the universality of the attempt to cover the face of the fracture; even when disease has produced destruction of the external membrane, and death of a portion of the outer surface of the bone, this capping and smoothing of the asperities of the face of the injury is wholly or partially effected, a circumstance which proves it to have its origin in the membranous lining of the canal, for had it been a production of the outer covering, the effusion must necessarily have partook of the actions, and shared the fate of its parent. In such a condition of the external membrane, the internal does not wholly escape uninjured, sound parts are constantly affected by the contiguity of disease in parts of a similar organization, and hence its effusion is scanty, lies loose upon the face of the fracture, and if seized brings away its parent membrane, leaving the canal of the bone denuded.

When the external membrane is diseased, we constantly find a scanty, or a total failure in the secondary effusion, which in a state of health ought to envelope the injury; upon examining externally, we therefore find no tumour, and the bone grates and is moveable upon the slightest pressure. This effusion, when natural, is copious, and when the surrounding soft parts are not too much injured, it is increased, and they become a powerful, though not necessary auxilliary to its formation; it serves early as a kind of natural ligature, supporting the fracture and restraining its motions. As the secondary effusion is always slight or totally absent in a diseased state of the external membrane, this shews that it owes its origin chiefly to that membrane; and the existence of the primary effusion capping the face of the fracture, even in a total failure of the secondary one, shews the distinct origin of the two; and that we can only attribute the primary effusion to the internal periosteum or membrane lining the canal of the bone. And these opinions will be confirmed by examining the appearances of a fracture, when the internal membrane does not go through its assigned function at a time when the external performs its part regularly in the process of reparation.

Of deficient effusion from the internal membrane.

Exp. 13. One hundred and twenty hours after fracture.—The injured part was surrounded and enveloped in a firm tumour, resembling the intervertebrary substance, but rather darker; the tumour adhered to the surrounding soft parts, which were very vascular; and so intimate was its connection with the external membrane, that they were not distinguishable from each other. Upon cutting into the tumour, a small cavity was exposed, containing a slippery glairy fluid; into this cavity the point of the superior portion of bone slightly projected, having a small part of the fractured face uncapped and bare. The inferior portion of the fracture was in a natural state, capped and embedded in the tumour, and not communicating with the cavity above mentioned.

Exp. 14. One hundred and forty-four hours after fracture.—The adhesion of the integuments to the parts beneath, was unusually firm, when these were removed there appeared an increased vascularity of the soft parts, with a firm tumour encircling the injury, not perfectly defined, but extending irregularly into the cellular membrane and amongst the lacerated fibres of the muscles. Upon cutting into the tumour, a little bloodv slippery fluid escaped from a cavity, the interior of which was ragged and crossed in various directions by numerous slender fibres ; the fracture was very oblique, and the extreme point of the lower portion projected uncovered into this cavity. The superior portion presented a natural appearance, was embedded in the tumour, and not communicating with the cavity. The tumour encircling the injury, was more dense internally than externally, and so closely and intimately connected with the external membrane, that when forcibly pulled, it detached that part from the bone, leaving it studded with innumerable bloody points.

Exp. 15. One hundred and ninety-four hours after fracture.—There was a considerable firm tumour encircling the fracture ; in the centre of the tumour was found a small cavity, containing a slippery glairy fluid, into which projected, uncovered, a portion of the face of the superior part of the bone. The other appearances were similar to those in the two last cases, except that a greater organization of the tumour was shewn by the presence of spots, of a redder colour, interspersed through its substance.

In these dissections we find perfect actions of the external membrane, the fracture is therefore enveloped in a tumour clearly a production of that part; but the actions of the internal membrane are deficient, and hence a portion of the face of the fracture is uncovered; the denuded part is found projecting into a cavity formed in the tumour, which is filled with a smooth slippery fluid. In cases of ununited fracture, tumour will be found in those cases where the want of union depends simply upon deficient actions of the internal membrane; it will be absent where the want of union is caused by disease of the external membrane. By considering the above dissections, we become able to explain the formation of that very curious circumstance, the artificial joint, (a rare case, but of which several are upon record,) and to ascribe it to deficient action of the internal membrane, whilst the outer membrane performs its function duly. In the artificial joint the secondary effusion in its progress to organization, beomes converted into a capsular ligament; the fractured faces, partially uncovered by effusion from the inner membrane, project into a cavity filled with a smooth fluid as above; as the process goes on, the uncovered asperities of the fracture are taken up by the absorbents, and moderately adapted to each other; they become partially covered with cartilage, and the glairy fluid serves all the purposes of synovia, lubricating and rendering motion of the new joint as easy as possible. In the first volume of the Transactions of a Society for the improvement of Medical and Chirurgical Knowledge, is a case by Mr. Home, with an engraving so very much to the point, that I hope the quotation will be excused, since it shews the completion of the process here spoken of, and of which Exp. 13. 14 and 15, may be considered only an earlier stage. " A man aged sixty-eight, died in " St. George's Hospital, whose right os hu- " meri had been broken three years and nine " months before, but the bones had continued " disunited, and admitted of motion more " freely at that time than immediately after " the accident.—The arm was carefully dis- " sected, to examine the state of the frac- " tured part, between which there was no " callous, but a large bag filled with a glairy " fluid resembling synovia. The internal " surface of this bag was smooth, resembling " a capsular ligament, and its attachment to

" the bone was of the same kind, it adhered
" firmly to the surrounding parts, which were
" thickened and consolidated, rendering it
" very strong. The two ends of the bone
" were adapted to each other, all their irregu-
" larities having been absorbed, and their
" surfaces were of considerable extent from
" the fracture being oblique. The surfaces
" of the bones were fitted for motion, were
" not completely covered with cartilage, but
" studded over with it, and the bone was ex-
" posed in the interstices."

When the necessary effusions are well got over, another process, whose circumstances may be sufficiently gathered from what has been said takes place, viz. the organization of the tumour. The thickening of the external membrane, its increased vascularity, and the manner it assimilates itself to its effusion; the increased vascularity of the surrounding soft parts, and their adhesion to the tumour, shew from whence it is pervaded by blood vessels. The manner the bone is studded with bloody points upon tearing away its external covering shews that the bone itself is more vascular, and by inference, that the inner membrane is so also. A vascularity sensible to the eye first takes place where the bones overwrap, and two portions of external membrane touch, and here earth is first deposited; the interposition of the least portion of muscle is sufficient to diminish or destroy the activity of the vessels here, and to retard or even destroy the whole process. When the tumour is completely pervaded by blood vessels, we then observe it diminish, which shews that absorbent vessels have also made their way; the callous tumour at this period is found detached from the adjacent soft parts, and becomes more and more defined, and not till this period do we find it covered with any proper investing membrane. This part of the subject is so entirely regulated by those laws which govern the same process when taking place in soft parts, and have been so minutely described by former observers, that I have only thought necessary to allude to it in this cursory manner, in order to connect the subject and bring me more naturally to consider the origin of that membranous covering which is destined to form the future external periosteum of the fractured part.

Of the membrane investing the callous tumour.

The further progress of reunion produces destruction of that part of the external membrane which is enveloped in its own effusion, these parts becoming the bed of an earthy deposition, hence arises a necessity for the formation of a covering to the callous tumour which shall ultimately become the true external periosteum. This membranous covering first appears about the thirteenth day; most observable where the extreme bounds of the tumour terminate upon the sound periosteum; hence we naturally conclude it to be a production or prolongation of that part of the membrane, and not a condensation of the cellular membrane, as is the case with the covering of many unnatural tumours. On the fifteenth day the tumour is wholly covered by an extension of the periosteum, and is perfectly defined. On the nineteenth day several large blood vessels are found ramifying upon this membrane; the tumour is very vascular and the pervasion and union of all parts by blood vessels is complete.

From this period the tumour gradually diminishes in size, and becomes more and more consolidated as it approaches the structure of solid bone. Into this process of earthy deposition as into the pervasion of the tumour by blood vessels, it forms no part of my subject to enquire; nothing more is necessary to a completion of reunion than a continuance of actions now in full operation, viz. the absorption of parts which have served their destined purpose, and the deposition of earthy matter so as to assimilate the new formed parts to the original bone; this requires a period far beyond that when the limb becomes useful, for the tumour at the point of fracture does not arrive at its greatest diminution for a great length of time; the absorbent vessels of the part would seem to retain for ever afterwards an aptitude to be easily excited into action, since under circumstances of long continued fatigue, privation, and disease, at any succeeding period of life, they will take up the parts thus formed and leave an ununited bone, as was the case in some instances of the crew of the Centurion under Lord Anson.

These observations were made by fracturing the thigh bone of young rabbits; the fracture constantly overwrapped, no attempt being made at reduction, hence I was enabled to attend to what was going on upon the face of the fracture much better than if the bone had been replaced.

It has been thought that the callous tumour was a coagulable juice flowing from the face of the fracture, and gradually hardening into bone. It has been asserted that the effused blood itself underwent changes from which this tumour resulted. One observer attributes its formation entirely to the internal membrane, another entirely to the external membrane; another to a thickening of the periosteum alone, and others to the bone itself. In this conflict of opinion the subject still remains very much undecided, and upon the origin and formation of the callous tumour, all authors I have consulted are obscure. I hope sufficient has been said to shew that this tumour consists of two successive effusions; the primary one from the internal membrane, smoothing the face of the fracture by the removal of its asperities, thereby preventing further injury to the soft parts, and offering a fit medium for the reciprocal communication of the vessels of the injured parts: the secondary effusion from the external membrane aided by the injured soft parts, and forming the great mass of the tumour, a deficiency of either of which is attended by its peculiar inconveniences and mischiefs, and incompatible with an early and perfect recovery.

ON THE POSSIBILITY OF RECONCILING THE SCRIPTURAL AND PROFANE ACCOUNTS OF THE ASSYRIAN MONARCHY.

BY THE
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(Read Dec. 26th, 1817.)

" Audace si, ma cautamente audace."—Tasso.

To reconcile the scriptural accounts of the Assyrian affairs, with the details which are given us by profane historians, has long been the *crux historicorum.* The first thing necessary towards enabling us to ascertain whether they admit of conciliation, is to state the difference which actually exists.

The 10th chapter of the book of Genesis contains the earliest mention of the capital of the Assyrian empire, and it is there said, that " Assur builded it," but without any mention of the time of its foundation, or its actual size and importance. That the monarchies which existed in this part of Asia were originally very small, is evident from what is related in the 14th chapter of Genesis, where a king of Shinar (the Babylonian plâin) confederated with a king of Elam, and two others, comes and attacks some petty sovereigns of Palestine ; and Abraham, arming his own household, is able to defeat the invaders. From this time we scarcely meet with any mention of Assyria, till the book of Numbers, where, in the prophecy of Balak, it is foretold, that *Assur* should carry the Kenites into captivity. Among the oppressors of Israel, mentioned in the book of Judges, is *Chushan Rishathaim*, king of Mesopotamia, whose dominions *may* have included Nineveh, but who hardly could have exercised an independent sovereignty, and made conquests, if that city had been itself the metropolis of a mighty state. A circumstance mentioned in the history of David ([1]) leads to the same conclusion respecting his own time. He had a war with the king of Tobah, (Nisibis) which was at so short a distance from Nineveh, that it is scarcely possible that, had that city been the seat of a great monarchy, either the king of Tobah could have carried on war as an independent prince, or David have fought against him without coming into collision with Nineveh. The first mention of conquering monarchs at Nineveh, in the Jewish annals, is in the reign of Pul, 771 B. C., by whom and his successors Syria and Palestine were invaded, and the two branches of the Jewish people reduced to dependence and captivity. ([2])

Such is the antiquity and uninterrupted series of the Jewish annals, and the position of this nation relatively to a power on the banks of the Tigris, extending its dominions westward, that we may safely say, that had any such power existed previously to the 8th century before Christ, it must have come into collision with the Jewish nation, and that collision must have been recorded in Jewish history. We are next to see how far profane history agrees with sacred.

From Herodotus, whose Assyrian history is lost, we learn little more in his general history, ([3]) than that the Assyrians had been masters of Upper Asia (Asia beyond the Halys) 520 years, when the Medes shook off the yoke; which, reckoned backwards from 710 B. C., brings us to 1230 B. C. As however he does not describe the extent of their power, his account is not irreconcileable with scripture, since they might be masters of *Upper Asia*, without holding the sea-coast, or interfering with the Jews. It is from Diodorus Siculus chiefly, that what is commonly called the history of the first Assyrian monarchy is derived, Ctesias being his authority. According to him, Ninus, a prince of boundless ambition, engaging the Arabians under his standard, subdued Babylonia, Armenia and Media, and proceeded thence to remoter countries of Asia, which he reduced, with the exception of Bactria and India. Thence, turning his arms towards the west, he overran all the countries from the Euphrates to the Mediterranean, and from the banks of the Tanais to the cataracts of the Nile. ([4]) After these great exploits, he builds Nineveh, which he names from himself, and then returns to his former unsuccessful attempt upon Bactria, on which occasion his wife Semiramis is introduced to us. We pass over for the present, the miracle of her birth, to which we shall have occasion to refer; she was at this time the wife of Menon, the chief officer of Ninus' army, from whom she was taken away by the sovereign, to become his own wife. A son called Ninyas was born to them, and upon the death of Ninus, Semiramis succeeded to the throne. In rivalry of her husband she founded Babylon, and spent the rest of her days in warlike expeditions and public works; her attempt on India however proved unsuccessful, and she returned thence with the loss of her army. Her death was not less marvellous than her birth, and her crown descended to her son Ninyas, who began that course of effeminate sloth and luxury, which his successors followed for 30 or 36 generations, all of which past away, without affording a single fact for history, till the time of Sardanapallus, whose vices roused the indignation of Belesys the Babylonian and Arbaces the Mede, who rebelled, besieged him in Nineveh and drove him to the desperate expedient of burning himself, his haram and his treasures, to avoid falling into their hands; and with him ended the Assyrian monarchy according to Ctesias.

It is difficult to know where to begin in exposing the absurdities of this history. The alleged conquests of Ninus are at variance not only with the authentic accounts of those nations over which he is said to have made them, but with all probability. He comes forward at once from the darkness of antiquity, the king of Assyria, which was in so low a condition that neither Nineveh nor Babylon was yet founded, and applies himself forthwith to the conquest of the world. It is not thus that empires start at once from an obscurity in which they are unknown to history, to the dominion of the world, and had the Romans told us, that the Euphrates and the Rhine bounded the dominions of Romulus, they would scarcely have made a larger demand upon our faith, than the history of the conquests of Ninus. It is true, that Eastern monarchies rise with a rapidity unknown to European history, and that tribes of shepherd-invaders have spread themselves over a vast space, in a very short time. But the Assyrians were not such a people, they made war with the equipments of a civilized army, and consolidated their conquests by a regular system of legislation and dominion. It required three reigns of vigour and enterprise before the Persian monarchy could extend itself from the mountains of Persia to the shores of the Mediterranean; yet Cyrus, when he conquered Media and Assyria, entered into possession of countries long since flourishing and regularly administered, found wealth and arms and disciplined troops in readiness to be employed in the prosecution of his further enterprises; roads and communications established, and a multitude of facilities, which must have been wanting to Ninus, who comes forward a few centuries after the deluge, and in the first infancy of Asiatic civilization when he would not only find no roads to facilitate the marches of his troops, nor cultivated countries from which to feed them, but when not even the multitudes were in existence, who are said to have swollen his armies.

The history of all the countries which Ninus is said to have conquered, and his successors to have held, is inconsistent with the Ctesian account. I have already stated, that no Assyrian conqueror appears in Jewish history till the eighth century before Christ; and the same negative proof may be derived from the annals of all the other countries over which his empire is said to have extended. ([5]) Later writers indeed make Priam to have been tributary to a king of Assyria, but the whole of the Iliad is evidence against the truth of this account.

But the exploits of Ninus and Semiramis are scarcely more wonderful than the long line of their inactive successors. Thirty-six kings, from Ninyas to Sardanapallus, sat upon the throne of this mighty monarchy; and history, which has preserved the names of them all, has not a single fact to relate concerning them. The impossibility of this has been so strongly argued by the learned authors of the Anc. Univ. Hist. IV. 299, note

M. that I shall content myself with referring to that work. ([6]) Dr. Gillies, indeed, in his history of the world I.—62, endeavours to account for this silence by saying, that " the arrangements of Ninus were adopted by a line of seventeen princes, whose mild and pacific reigns leaving no trace of blood behind them, have escaped the notice of history." But if the authority of Ctesias and Eusebius is good, for seventeen successors to Ninus, it is good for six-and-thirty ; and to drop all the revolting and improbable circumstances in a narrative, of whose truth we have no evidence but those very historians with whom we take this liberty, is not to extract history from fable, but merely to change an extravagant fable into a moderate one. Facts are not therefore true, because they are credible. But the shortened account of Dr. Gillies, is not even credible; if we could believe that seventeen kings in succession, sat on the throne of an Asiatic monarchy, in those rude early times, in a country remarkable for the rapidity of its political changes, within the limits of ascertained history ; and that their reigns furnished not a single fact interesting enough to be recorded, we might believe it of twice seventeen. If the centuries which thus rolled away were centuries of peaceful improvement, the human mind must have made rapid advances, literature and science must have been cultivated, men of talents, curiosity and leisure, must have risen up, and history, which records the minutest circumstances of the earliest reigns of the Assyrian monarchs, could never have become, at once, profoundly silent respecting their successors. Could we therefore admit the very improbable supposition of seventeen peaceful reigns, we have still to explain why we know nothing more of them than that they were seventeen.

That the Ctesian history is in its present form incredible, has been universally felt by modern critics, but they differ widely in their opinions of the portion of truth which it contains. Some, like the author I have just mentioned, endeavour to pare down its extravagant chronology, and romantic events, to something like history; and to connect the profane and scriptural accounts together; but they refuse all union or assimilation. Diodorus knows as little of Pul and his successors, as the scripture does of Ninus, Semiramis and their dynasty. Others, like Sir Isaac Newton, (7) reject the Ctesian history as a pure fiction, with the exception of the single circumstance of the capture of Nineveh by the Medes and Babylonians. Yet this summary rejection of a history, long and minute, however incredible, is not satisfactory to the mind of an inquirer, because it leaves unexplained the cause which should have prompted to such an extensive fiction. A branch that has been stopped in the stream may have gradually formed an island with the matter which it has arrested; a particle of sand may have been the nucleus of a large incrustation; but something there must have been from the first, or the process could never have begun. The analogy will be found to hold with respect to the fictions of ancient history; they are not purely arbitrary; something has determined the mind to invention in one direction, rather than another; the error appears to me to have consisted in supposing that the basis must always have been some *historical fact.* It will be my object in the remainder of this paper, to show that it is mythology, not history, which has supplied the *punctum saliens* to which this mass of fiction owes its existence, and that all the persons who figure in the Ctesian history are the gods of the Assyrians, converted into human beings, by an error which we shall find repeated over and over again, in the history of almost every ancient nation.

I shall collect in a note (8) the passages of ancient authors which prove that Belus, Ninus, Ninyas, Semiramis and Sardanapallus were really worshipped as gods among the Assyrians; the only one of them with regard to whom any doubt can exist is Sardanapallus, who is certainly not commonly said to have been worshipped; yet if we consider that his statue was found in the splendid temple of Hierapolis, particularly devoted to the worship of Semiramis, we shall conclude that he must have been considered by those who placed it there in some very different light from the effeminate and voluptuous prince, the disgrace of his nation and of humanity. The fact indeed, that all these Assyrian princes were also gods, has not escaped those who have written upon this history, (9) but none of them, as far as I have seen, have drawn the proper inference from the fact, with the exception of M. Court de Gebelin, in his *Monde Primitif*. 4. 495. who has, however, endeavoured to support his opinion by false etymologies.

Three ways only can be conceived in which this coincidence of the divinity and the sovereign can be accounted for. 1. The sovereigns may have been deified for their exploits and other merits. 2. The sovereigns may have born the names of their national divinities, and thus the histories of the two may have been confounded; or 3. What we call their history is only a corruption of mythological legends respecting the gods, and instead of the sovereigns having been exalted to the heavens, the gods have been brought down to the earth.

It would lead me too far from my present purpose to discuss the general question, whether the gods of the Gentiles were deified men; I shall content myself with observing, that ancient and deeply rooted as this opinion is, all the latest and most philosophical investigators of heathen mythology have found themselves compelled to abandon it. In the particular case under our consideration, if we believe Ninus, Semiramis, &c. to have been deified for their exploits, we must admit the Ctesian account of those exploits, with all the intrinsic absurdities, and inconsistencies with other histories, which we have already pointed out. Semiramis was worshipped throughout many countries, and Arabians, Phœnicians, Assyrians, Capadocians and Cilicians brought their offerings to the temple of Hierapolis, which was devoted to her honour. Now if she were the head of a monarchy which extended itself over all these regions, it is *possible* that they might all agree in deifying her; but if they never were her subjects, their doing so is unaccountable. Again, Semiramis and Ninus may have been deified for their great deeds, but Ninyas, who began the system of effeminacy, and Sardanapallus, who, by indulging it, ruined the monarchy, for what merits were they placed among the gods? The difficulties to which we expose ourselves, by maintaining that the gentile deities were historical personages, is strikingly shown in what the authors of the Ancient Universal History say of the series of deities now under our consideration, IV. 366. "All the kings of Assyria," say they, "were deified, and the Jupiter and other gods of mortal origin, came from the banks of the Euphrates, and the Tigris; and instead of being so ancient as mythologists and historians make them, they began to be worshipped not above 900 years before the birth of Christ." If this be true, the gods of Homer's theology were his own contemporaries: for he lived about 900 B. C.

The 2d supposition, that of a coincidence between the name of the sovereign and the deity, in consequence of which their actions have been confounded, need not detain us long. Were there only an instance or two of this, we might say that it was possible; that it should happen so many times over, as it must have done to account even for Assyrian history, is highly improbable; that it should have occurred so many hundred times as is necessary to explain all the coincidences of the actions of supposed kings with the mythology of their country, which are found in early ancient history, is next to impossible. There remains then only the 3d supposition, viz. that the legends of mythology have been converted into history, and gods and goddesses into terrestrial kings and queens; and that the deeds of Ninus and Semiramis have no better claim to be received as history, than the conquest of India by Bacchus, or the expedition of Hercules into Spain. I wish however to premise, that I am far from supposing that I can assign a mythological original for *every* circumstance in the history in question: nor is it, I conceive, necessary that I should do so, in order to establish the probability of my hypothesis; there must have been some reason why a Belus, a Ninus, a Semiramis, &c. rather than any other names, were chosen as the heroes and heroine of the Ctesian fable; and this reason I find in the Assyrian mythology; but, the general idea once being given, the details of their conquests are only the natural developement of it, in which it would be in vain to seek for any peculiar reference to mythology. On the other hand, there are many circumstances in the narrative which, in an historical view, are improbable, or at least so peculiar, that the mind would not be naturally led to them, in expanding the general idea of conquering sovereigns into all its details; for these we must seek some specific cause; and this again is supplied by mythology. Our further progress in this inquiry, will be greatly facilitated, by our casting a glance upon the peculiar system of mythology which appears to have prevailed in this part of Asia, and which indeed is very closely connected in its principles with the Grecian, the Egyptian and the Hindû religions.

When man first began to reflect on the causes of the changes which he beheld in nature, as we can only reason to what we know not, from what we know, nothing was more natural, than that he should assimilate the operations by which all the productions of nature are brought into being, to those by which the species of animals are continued; that he should personify the energies of nature, under the image of male and female, a great father and great mother, to whose mysterious union the fertility of all things was ascribed. Accordingly we find this idea running through all the mythologies which I enumerated above; every where the different sex of the divinities, and their sexual unions, form the basis of that system of symbolical language in which the operations of nature are described. But the deities thus created by the rude philosophy and sensual imaginations of men, would speedily be identified with those elements of nature which bear the most conspicuous part in the production of things. Thus we find the earth, (10) the great supply of the materials for the growth of animal and vegetable bodies, most extensively personified and worshipped as the great mother; the sun, whose vivifying rays call forth her productive energies, as the universal father; (11) the refined fiery æther as the father; the inferior atmosphere, damp but fertilizing, as the mother; (12) fire as the active, water as the passive principle in the production of all things. (13) Again, we not only find two different elements represented as of opposite sexes, but a male and a female assigned to the same element. (14) It was in pursuance of the same idea, that the emblems of the active and passive principle, the *linga* and *yoni* of the Hindoos, the φαλλος and κτεὶς of the Greeks, first received those honours, which, in their primary acceptation symbolical, became afterwards the cause of the grossest licentiousness.

When philosophy first began to assume an existence separate from theology, which it did in Asiatic Greece, we find it occupying itself with theories respecting the element from which all things had been formed, and water and fire respectively proposed for this office. The same ideas had no doubt been current, in a different form, many centuries before, but in the garb of mythology instead of philosophy. One of the most ancient of these theories appears to have been, that which considered a watery chaos as the primeval condition of the universe; we find it in Phœnician cosmogony, (15) and it has even the sanction of the venerable authority of Scripture. (16) As a mythological legend, it assumed several forms; one, the most immediately connected with our present subject, was the representation of the great goddess, the general mother, in a shape half female half fish, bearing the name Atargatis, (Addir-dag, the great fish,) as a symbolical representation that water was the parent of all things. (17) Softened down into the name Derceto, she is the reputed mother of Semiramis, the heroine of the Ctesian romance. The various accounts which are given by authors of the attributes of Derceto (18) are easily reconciled, if we advert to what was before mentioned, that a number of the Heathen divinities coincided, in as far as they all represented the great parent, male or female, though differing in respect to the element which they typified. Thus Derceto coincided, with Rhea and Cybele, who were the earth, (19) with Venus, as a symbol of the watery origin of all things, that goddess being said to rise from the ocean; with Astarte, or the moon, (20) that luminary being frequently represented as the mother, while the sun is the father of all things. We need not therefore wonder that we find some authors asserting the identity of Derceto with these goddesses, while others distinguish them; the identity lay in the general character, the diversity in the circumstances.

Another peculiar tenet of ancient, and especially of Asiatic mythology, was the coexistence of both sexes in the same divinity, so that each was at once both male and female. (21) It is evident, that when we speak of powers, active and passive, as existing in nature, we make an abstraction: that nature is really one whole, comprehending both these powers within itself, and if it is to be represented as a being, this must be denoted by the body of the divinity being of both sexes. Hence many gods were ἀῤῥενοθηλεις or hermaphrodite; (22) hence we find the same divinity at times engaged in all manly and warlike occupations, at another clothed in female attire, delicate and effeminate in person and employments, nay even changing the most essential attributes of the sex (muliebria patientem). (23) On the other hand, but from the same principle, deities ordinarily represented as female, are exhibited with male attributes, armed and bearded, and the priests to express this double and variable sex, served the divinity in female garments, (24) or even deprived themselves of manhood, in order to produce an imitation of the deity, who was at once of both sexes and of neither. All these particulars will be found established by quotations in the notes; we shall now proceed to show how they have influenced the Ctesian history.

The name of Belus, with whom the line of kings, according to some accounts, begins, shows him to have been the great solar divinity, to whom the nations of the East unanimously gave the titles of royalty, calling him Baal and Moloch and Adonis, (Lord and King) the former of which names the Greeks made into Belus. Sometimes they tell us that Belus is the sun, at others Jupiter, meaning by the latter explanation, that he was the supreme god of Assyria and Phœnicia, as Jupiter was of Greece. We may observe, that it frequently happened, that the same part or power of nature was worshipped under different names by the same people, and that where the name did not expressly describe the object represented, its primary meaning became in some degree lost to the worshippers themselves. The Φοιβος Απολλων, the *bright-shining Apollo*, of the Greeks, was certainly the sun; but both names had become *proper* names, and the same divinity was worshipped by the title of Ἥλιος. So we find in scripture, 2 Kings xxiii. 5. Baal, and the sun spoken of as different, though we have the amplest testimony to their original identity. As usual, Belus has been made a king both of Assyria and Pœnicia.

> Hic regina gravem gemmis auroque poposcit
> Implevitque mero pateram: quam Belus & omnes
> A Belo soliti. Æn. 1.—728.

Yet as Assyria and Phœnicia never had the same sovereign till many centuries after the supposed time of Belus, it is not easy to see how a king of either should become a god in both. Among the Assyrians, he was the god of war, and therefore identified by the Greeks with Mars, as well as Jupiter. Histiæus, quoted by Josephus (see note 8.) calls him Ζευς Ἐνυαλιος, Jupiter-Mars; and the Alexandrine Chronicle explains Βααλ θεος by Ἀρης πολεμικος. Perhaps then we may venture to consider it as proved, that the first person mentioned in Assyrian history was not a king deified, but a god converted into a king. Were there no other circumstance respecting Ninus and Ninyas, which made their historical existence dubious, than that their names coincide with the city which was their metropolis, and which the elder of them was said to have founded; this would excite a reasonable suspicion, that they had been invented to furnish an etymology for Nineveh. No one, who has not made it the subject of particular investigation, can have any idea of the extent to which the ancients have corrupted history, by deducing the names of countries, rivers, mountains and towns from personages of supposed historical existence, but really having no other being than in the legend framed to account for the name. ([25]) The names Ninus and Ninyas correspond with the difference between the Greek and the oriental way of spelling Nineveh, the former calling it Νινος the latter Νινοη. The same traces of mythological origin which we observed in Belus appear also in Ninus. According to Joannes Malela, not certainly a very pure authority, but by whom many curious fragments of history have been preserved, Saturn reigned over Syria, (Assyria) having for wife Semiramis, whom the Assyrians call Rhea, and had two sons, one named Ninus, the other Jupiter, i. e. Belus, as we have already seen. If Belus be, as we have shown, no historical personage, Ninus, who is his brother, must have also had a mythological origin. In fact, Ninus and Belus appear to be one and the same god; the former name being given from the name of the city. But the strongest proof that Ninus is a mythological personage, arises from the very evident traces of such an origin in his wife Semiramis. She was in fact, under another name, the very same goddess as Derceto or Atargatis whom the legend makes to be her mother; nothing being more common in ancient mythology, than for two names of the same deity converted into persons, to be placed in the relation of parent and offspring to each other. ([26]) The various traditions respecting the temple of Hierapolis, viz. that it was consecrated to the worship of Atargatis, Derceto, Semiramis, Rhea and Juno, are at bottom the same, one and the same great female divinity being worshipped by different nations under these various names. The dove was sacred to Semiramis, because, as was pretended, she had been nourished by doves, and at her death disappeared under the form of a dove; but really because that bird was an emblem of productiveness, and therefore sacred to Venus, with whom Semiramis was the same, as with the other goddesses already enumerated. The story of Venus changing herself into a fish on the banks of the Euphrates, identifies her with Atargatis the fishformed goddess, who was Derceto and Semiramis. (27) Hence the priests of Hierapolis religiously abstained from eating fish. The extraordinary mixture of opposite qualities and inconsistent actions in the life of Semiramis, receive an easy solution from a reference to her mythological character. Her masculine manners and dress of a male (28) are quite in harmony with what we observed before of the mixed sex and habit of this great female divinity; her unbounded lust with the character of Venus or Mylitta, the representative of the productive powers of nature, whose rites, at first symbolical, degenerated into occasions of the grossest licentiousness.

That she should at the same time have introduced the custom of making eunuchs, and being attended by them, (29) is quite inconsistent if told of any historical personage, but is easily explained from her being, as we have often observed, the goddess Rhea or Cybele, in whose honour her priests castrated themselves. Her incest with her son Ninyas, is another fact of which we must seek the origin in mythology, where such unions among deities are not uncommon. (30) Her ascendency over Ninus is, historically considered, inconsistent with the character of that conqueror; but the same mixture of sexes which made Semiramis a warrior, makes Ninus effeminate; he is a Hercules with Omphale, and is described in a manner which identifies him with Sardanapallus. (31) This mixture of sexes, and consequent inconsistency of the actions with historical truth, is no where more striking in this narrative than in what regards Ninyas, who is said to have been the feeble prince, who set the example of that effeminate luxury which his successors indulged, and at the same time was deified as the god *Mars* (see note 8.) But we have already seen that Belus (his grandfather according to history,) was the god Mars; consequently

Belus and Ninyas are no other than one and the same divinity, as we have already shown Ninus and Belus to be. Descending to Sardanapallus himself, we find the same unaccountable mixture of the dissolute, effeminate voluptuary and military hero which we have already twice remarked. It is in vain that recourse is had to the supposition of two and three Sardanapalli, though this solution is countenanced by an ancient writer who had felt the difficulty; because these inconsistent traits are expressly related of *one* Sardanapallus. The difficulty is easily removed, by adverting once more to the mixture of the sexes in the Asiatic divinities. I should here observe, that I by no means intend to deny that some circumstances of historical truth respecting the capture of Nineveh, derived from the siege by Nabopolassar and Cyaxares, may have been mingled with the story of the death of Sardanapallus; the time was near enough for tradition, while I cannot believe that any historical truth can have descended through that dark interval of seventeen or seven-and-thirty kings, which has not furnished a single fact concerning itself. There are several points however, respecting the history of Sardanapallus which require a more minute examination than can be bestowed upon them here, and for which therefore I refer to the notes.

Perhaps some may be inclined to think the historical reality of the existence of Ninus, Semiramis, &c. proved by the tombs and monuments belonging to them. With respect to the first it may be observed, that nothing was easier than to attach the name of Ninus or Sardanapallus to a mound of earth and call it his tomb; that as mounds in different places had this honour attributed to them, some of them *must* and therefore all of them *may* have been falsely fixed upon; that the Egyptians gave the name of the tomb of Osiris to mounds, though it is certain that Osiris was no historical personage. As to the public works attributed to Semiramis, nothing can be more uncertain than the traditions which fix the authors of monuments of such high antiquity. If we were as credulous respecting the remains of our own country as respecting those which are mentioned in ancient history, the obscurity which hangs over the origin of Stonehenge might easily be dissipated. As criticism has grown more enlightened, popular traditions on this subject have been discarded, and we are content to acknowledge ourselves ignorant by what hands it was raised. If corresponding tales respecting the origin of Assyrian monuments have obtained longer and more extensive belief, has this been owing to their having a firmer basis, or their being less rigorously scrutinized? (34) The argument, if admitted, would prove too much; it would follow not only that Semiramis had lived and reigned, but reigned over countries in which we have already shown it to be inconsistent with history and probability, that Assyrian dominion should have been established till about eight centuries before Christ.

To some it may appear improbable that such a confusion of mythology and history should take place as I have supposed; but let it be considered, that, in the earliest times, priests were the sole depositaries of literature, and that mythology appears to have been in all countries the earliest subject upon which literature was employed. Is it wonderful, then, that the divinities having been represented under human forms, human passions and adventures having been ascribed to them, and names given to them which, in the lapse of time and the changes of language, were no longer understood; it should have been forgotten that the human form was merely the dress by which an abstract idea had been rendered tangible, that the human passions and adventures were symbolical and not real, that the names were epithets of divinities, not names of men, and that thus the Pantheon should have been converted into a gallery of history? Is it not agreeable to the progress of fiction in other cases, that when once the idea of the gods having been sovereigns of the countries where they were worshipped was adopted, those who successively wrought up the original materials should drop more and more of the circumstances which could not be made to assume an historical air, bend those which they retained more and more to the shape of history, and add more of those details which were necessary to give fulness and connection to the story? We need not, however, rest upon these presumptions that history would undergo such corruptions; we have positive proof that it has been subject to them. In Herodotus, who wrote when the Egyptians had not materially corrupted their own history, we find Osiris and Isis represented not as human beings, but as divinities answering to the Bacchus and Ceres of the Greeks: but in Diodorus Siculus, who lived in the Augustan age, they are converted into historical personages, a king and queen of Egypt. Osiris invents agriculture and civilizes his people; builds Thebes; collects a large army with which he invades Æthiopia, founds towns there, establishes collectors of revenue, and builds dams, by which the course of the Nile was bounded and its inundations restrained. Hence he crosses over to Arabia and subdues the whole country as far as India, founding cities without number, returns to Europe, subdues Thrace and Macedonia, to which he gives the name of his general Macedo, and finishing his peregrinations in Egypt, is deified for his virtues and public services. Now let us suppose, that instead of being able to expose the romantic falsehood of all this from other authors, we had the history of Egypt *only* from Diodorus, as we have that of Assyria from Ctesias, what would have been the consequence? Osiris and Isis would have taken their place among Egyptian sovereigns, as unsuspectedly as Ninus and Semiramis have done among the Assyrian; the ancients would have invented a list of their successors, and the moderns would have maintained that though indeed the conquests of Osiris could not have been so extensive as was represented, it was unreasonable to deny his existence and his victories. All this would have been completely false in respect to Egyptian history;

and why may not the same thing have happened in Assyria?

The explanation which has now been applied to a single part of ancient history, is capable, if I mistake not, of being extended to very large portions of it. The earliest annals of every ancient nation, except the Jewish, have been corrupted in the same way as the Assyrian, and gods and their actions converted into personages of history. So widely spread are the fictions which have originated in this source, that whole centuries must be struck off from the authentic history of many countries. The origin and affinities of nations may have been preserved by tradition, and are besides written in characters which time cannot obliterate, nor national vanity falsify, in the similarity of language, feature and religious institutions; but beyond this I am disposed to doubt if there is any thing in ancient history that can be received with certainty, till about eight centuries before the Christian æra. Sir Isaac Newton long ago observed, that there is no dependence to be placed on any thing said to be done above 100 years before the use of writing: we have no proof that writing was commonly used in Greece till about the period which I have specified, and all early profane history is known to us only from its connexion with the Grecian.

Before I take leave of this subject, I would observe, that the existence of any such power in Assyria as the Ctesian account represents, at the period which it assigns for its existence, would have been incompatible with the designs of Providence, as far as we can judge of them by actual events. The nations inhabiting the Asiatic shores of the Mediterranean, have been made the instrument of communicating to the rest of the world three of the greatest blessings, civilization, literature and pure religion. Had Phœnicia been enclosed within the dominion of an Assyrian despot, its inhabitants never could have gone forth, with the spirit of enterprize which freedom inspired by rendering its pursuits secure, to carry the knowledge of arts and letters to the barbarous nations of Europe. Had Ionian Greece fallen under the dominion of Ninus and Semiramis, instead of being reserved for the conquest of Greece, poetry and philosophy would have been stifled in their cradle, and Athens could never have become the instructress of all succeeding ages in literature and art. Had Judea been conquered by Assyrian monarchs, when the people of Israel had recently settled in it, before time had enabled their peculiar institutions to form in them a distinct and permanent national character, they would have been dispersed and absorbed among the idolaters who surrounded them : Judaism would have terminated before the world knew of its existence; and Christianity, the daughter of Judaism, could never have come into being, for the illumination and conversion of the world.

NOTES AND ILLUSTRATIONS

TO THE PRECEDING PAPER.

(1) 2 Samuel viii. 3. "David also smote Hadadezer, the son of Rehob, king of Zobah, as he went to recover his border at the river Euphrates." The 5th verse mentions that the Syrians of Damascus came to succour Hadadezer, but there is no mention whatever of the Assyrians.

(2) 2 Kings xv. 19.

(3) Her. I. 95. Ασσυριων αρχοντων της ανω Ασιης επ' ετεα εικοσι και πεντακοσια, πρωτοι απ' αυτων Μηδοι ηρξαντο απιϛασθαι.

(4) Diod. Sic. II. 1. & seq. Κατεϛρεψατο μεν γαρ της παραθαλαττιου και της σουεχους χωρας την τε Αιγυπτον και Φοινικην, ετι και κοιλην Συριαν, και Κιλικιαν, και Παμφυλιαν. και Λυκιαν, Προς δε ταυταις την τε Καριαν κ. τ. λ. μεχρι Ταναιδος.

(5) A passage in Herodotus may seem to favour the opinion that Ninus extended his conquests to the shores of

Asia Minor. Speaking of the Lydian kings he says, I.—7. "Candaules, whom the Greeks name Myrsilus, the king of Sardes, was a descendent of Alcæus, the son of Hercules. For Agron, the son of Ninus, the son of Belus, the son of Alcæus, was the first of the Heraclidæ who reigned over Sardes." Now if a son of Ninus reigned over Sardes, it may be said Ninus must have conquered Sardes. But this whole chain of genealogies wants an historical basis, and is an example of that propensity of ancient historians to turn mythology into history, of which we shall have numerous examples before the close of this inquiry. Belus was a divinity of the Assyrians, and must therefore have been a king deified after his death, or a god converted by historians into a king. If the latter be the case, the genealogy which gives him a father and grandmother is evidently false; if the former, how should a king of Assyria be the grandson of a petty chieftain of Greece; and if three steps of the genealogy be evidently false, what security have we for the other two? The fact is, that Ninus, Belus and Hercules were all the same divinity, and that the Assyrian and Lydian mythologies are here entangled. Plato (de Legg. iii.) makes Priam a tributary of the kings of Assyria; and Ctesias says, that his name was Teutemus, and that he sent Memnon to Troy: but the history of Memnon is palpably mythological.

([6]) "This vast chasm of inaction in the Assyrian monarchs, from Ninyas to Sardanapallus, a vacancy as we may call it of at least 1200 years, is as strong a proof that the profane accounts are fabulous, as any of the many which we have offered to prove them so. Is it probable that in so long a succession of princes, there should have been one only that did any thing worth the recording? or is it possible, that in so very long a succession of years, there should have arisen no man at all who had ambition and courage enough to take advantage of the sloth and supineness of these kings at Nineveh? Where were the other kings of the world all this time, and especially such as were vassals to this throne? Did they glory in their chains and the burden of their tribute?—So many pacific ages, such a series of the calmest peace, both from within and without, may shock the easiest credulity."

(7) Chronology of Ancient Kingdoms, &c. 267.

(8) Βηλος ην Βασιλευς Βαβυλωνος, η και ὁ Ζευς αυτος. Eust. Dionys. Perieg.

Διος ὃν καλουσιν οἱ Βαβυλωνιοι Βηλον. Diod. Sic. II. c. 8. Βηλος, ουρανος και Ζευς Hesychius. "Belus primus rex Assyriorum, quos constat Saturnum, quem eundem Solem dicunt & Junonem, coluisse—Apud Assyrios autem Bel dicitur, quâdam sacrorum ratione & Saturnus & sol." Servius Æn. 1.—733. The goddess Baalith was converted into a queen, as Bel into a king—προαγγελλω συμφοραν; την ουτε Βηλος εμος προγονος ουτε βασιλεια Βηλτις αποτρεψαι σθενουσι Abyd. ap. Euseb. Chron. Gr. p. 49. Ed. Scal. Yet this Baalith derived her name not from being a queen of Assyria, or any other country, but the female regent of the heaven and the world. Βηλθης, Ἡρη και Αφροδιτη. We proceed to Ninus, with regard to whom we may observe, that he does not appear to have been originally, like Belus, a deity of the Assyrians, but to have been invented to account for the name Nineveh, and in process of time, confounded with the real deity Belus, whom many made his father. According to Malela (p. 19) he was the son of Saturn and brother of Jupiter and Juno; it is hardly necessary to observe that the brother of two mythological personages cannot be other than mythological himself. We may infer from the images of Belus and of Ninus being borne together by the army of Darius, going forth to battle, that they were both held of equal sanctity. Curt. III. 3. 16. "Ninus—Chaldæis Hercules, Assyriis Jupiter dictus."

Lloyd Dict. Poet. on which we may observe, that Mars and Hercules were identified by the Chaldeans, and that Ninus being worshipped as Hercules, was the same with Mars. " Chaldæi quoque stellam Herculis vocant quam reliqui omnes Martis appellant." Macrobius. Sat. III. 12. Of Ninyas, we are told that he was deified and worshipped as the god Mars. " Assyriis regnavit tertius Zameis, qui & Ninyas, filius Nini & Semiramidis." Euseb. Chron. p.10. Μετα δε Νινον εβασιλευσεν Ασσυριων Θȣρρας ονοματι όντινα μετωνομασεν ὁ τȣτȣ πατηρ Ζαμης, εκ της Ρεας αδελφος, Αρεα, εις ονομα τȣ πλανητος αςερος—The author goes on to say, that to this Mars the Assyrians first raised a column and worshipped him as a god, calling him βααλ θεος. The name Θȣρρας is Greek, and an epithet of Mars. Θȣρος Αρης Eur. Suppl. 579. According to Eusebius, the next king to Ninyas was Arius i. e. Αρειος, which is again only an epithet of his supposed predecessor, changed into a king. It is then one and the same divinity who re-appears to us in the persons of Belus, Ninus and Ninyas, the great god of the Assyrians, the sun, worshipped more especially as the god of war, the two latter names being in all probability invented to account for the name of the city. Josephus (Ant. 1.—4) from Hestiæus represents the great divinity of the Assyrians as Ζευς Ενυαλιος i. e. Jupiter-Mars. Semiramis was Rhea, την δε Σεμιραμιν και Ρεαν εκαλȣν. Suid. in voc. Of Sardanapallus I shall speak hereafter.

(9) See Anc. Univ. Hist. IV. 353, 361.

(10) The earth was the Cybele of the Phrygians, the Rhea of the Greeks, the Latins worshipped her by the name Tellus, the Hertha of the Germans is obviously the same; the Egyptian Isis among her various characters was also the earth, and the same personification is found in Hindû theology.

(11) " Le grand ouvrage de la génération s'opère par l'action du soleil premièrement & secondairement par celle de

la lune, de manière que la source primitive de cette ènergie soit dans le soleil, comme père & comme chef des dieux mâles qui forment son cortège." Proclus in Timæ. l. 15. as quoted by Dupuis, origine de tous les Cultes, 1.—131.

(12) "vides sublime fusum, immoderatum æthera,
qui tenero terram circumjectû amplectitur?
hunc summum habeto divum: hunc perhibeto
Jovem."

"Aer autem, ut Stoici disputant, interjectus inter mare & cælum, Junonis nomine consecratur, quæ est soror & conjux Jovis, quod ei similitudo est ætheris & cum eo summa conjunctio." Cic N.D. II.—25, 26.

(13) "Quippe ubi temperiem sumsere humorque calorque
Concipiunt & ab his oriuntur cuncta duobus
Cumque sit ignis aquæ pugnax vapor humidus omnes
Res creat & discors concordia fœtibus apta est."

"Alterum enim quasi masculinum elementum est, alterum quasi fæmininum: alterum activum, alterum patibile." Lactantius de or. Err. II.—10.

(14) Senec. Q. Nat. III. 14. Aerem marem judicant (Ægyptii) quâ ventus est; feminam quâ nebulosus & iners. Aquam virilem vocant mare: muliebrem omnem aliam. Ignem vocant masculum quâ ardet flamma; feminam quâ lucet innoxius tactû.

(15) Ωκεανον τε θεων γενεσιν και μητερα Τηθυν.
Hom. Il. Ξ. 201. Euseb. Præp. Evang. I. c. 10.

(16) Gen. 1.—2.

(17) Ibid. Auctor de deâ Syriâ ap. Lucian. c. 14. p. 460. Vol. 3, ed. Hemst. The author himself seems disposed to deny the identity of the goddess worshipped at Hierapolis

with the fish-formed deity of Ascalon, because in his time she was no longer represented in this way. Strabo, however, XVI. 1132. says, that Atergatis and Derceto were the same.

([18]) Lucian ubi sup. c. 15.

([19]) Lucian ubi. s. c. 32.

([20]) Ατταγαθη (leg. Αταργγαθη) Αθαρα; a corruption of Astarte. Hes. in voc. Luc. c. 32.

([21]) Thus, according to the Orphic doctrine, taught in the initiations of Bacchus, the first organized matter which was contained in the primeval egg was of both sexes ανδρογυνος. Recog. Clem. ap. Cotel. P. P. Apost. 1.—589. So Φυσις is invoked in the Orphic hymns

Παντων μεν συ πατηρ, μητηρ, τροφος ηδε τιθηνος:

Ἁγνην τ' ευιερον τε Μισην, αῤῥητον ανασσαν,
Αρσενα και θηλυν, διφυην λυσειον Ιακχον. Orph. H. 42.

Εννυχιη, δαδουχε, κορη ευαστερε Μηνη
Αυξομενη και λειπομενη, θηλυς τε και αρσην. Orph. H. 9.

Ὁδε Διονυσος—εστι θηλυμορφος, μηνυων την περι την γενεσιν των ακροδρυων αῤῥενοθηλυν δυναμιν. Euseb. Præp. Ev. III. 11. Suid. Ανδρογυνος. Bacchus was represented by artists in the character of an Hermaphrodite. Visconti Monim. Antichi An. 1795 Septembre.

Ζευς αρσην γενετο, Ζευς αμβροτος επλετο νυμφη.
Fr. Orph. p. 457. ed. Herm.

"Venerem igitur almum adorans, sive femina sive mas est." Macrob. Sat. III. 8. The moon was worshipped as Lunus, as well as Luna, Id. ibid. Æl. Spart. in Carac. Hesych. Αδαγοος.

([23]) Διονυσος—τα ανδρος ποιων και τα γυναικων πασχων. Suidas Ανδρογυνος. Bacchus, who generally appears as a beautiful effeminate youth, was a great warrior and conqueror. The effeminacy of Hercules is well known:

qui pacato statuisset in orbe columnas
Tam durâ traheret mollia pensa manû.

Prop. III. 9. 17.

(24) The priests of Hercules wore women's clothes in his mysteries. Joh. Lydus de Mensibus p. 92. referring to Nicomachus, who had written on Egyptian festivals. The worshippers of Bacchus at times did the same, Hes. Ἰθυφαλλοι. As a counterpart to this, the Venus Urania, the Assyrian and Phœnician Venus, was armed. Herod. I.—105. Paus. 267. Ed. Kuhn; though her name Μυλιττα, from the root ילד *to bring forth,* shows that she was also the goddess of generation. "Signum etiam veneris est Cypri barbatum corpore, sed veste muliebri, cum sceptro ac staturâ virili." Macr. Sat. III. 8. Strabo p. 588, 9. ed. Alm. speaking of the temple of Comana in Pontus, describes the worship there in a manner which leaves no doubt that she had the attributes of Venus, and says that she had also a temple at Comana in Cataonia; yet in speaking of this (p. 535) he calls it a temple of Ενυω, Bellona, from the union of martial attributes with those of the goddess of love. Now these goddesses were the same as Atergatis, Derceto, Semiramis. The priests of the goddess of Comana, like those of Cybele, Rhea, Atergatis and Derceto, emasculated themselves. Hence the "exsectos Comanos" of Valerius Flaccus.

(25) Although this proneness of the Greeks to invent persons and adventures to account for names be obvious on the slightest inspection, it may not be uninteresting to produce here an instance or two which have not been generally observed. When we are told that Attica was called Κραναη from Cranaus, though the name *(rocky)* is exactly descriptive of its soil, or Αιγιαλεια a maritime region from Ægialus, instead of αιγιαλος the sea-shore, the falsehood of the etymology which supposes the name to have originated

from a person is obvious: but where the real root is less obvious, personages equally fictitious are received without scruple into history. The name *Larissa* is one which occurs very frequently in ancient geography; there was one in Argos, another in Thesaly, another in the Troad, and a fourth in Syria. The very circumstance of a name being repeated in so many and distant countries, shows that it must be significant of some circumstance in which they all agreed, and Strabo indirectly informs us what this was, when he tells us (621 Alm.) of three of them, απαντες ποταμοχωστον την χωραν εσχον, that they had all alluvial deposits in the adjacent country, a circumstance which marks the vicinity of a considerable river. Of the ThessalianLarissa he says, that it was the same in site and quality with the Caystrian (440) και γαρ ευυδρος. And this is the exact meaning of the name, derived from λα an intensive prefix and ρεω. λαδρεοντι παρα το λα και το ρεω. λαρεοντι, μεγαλως ρεοντι, πλεονασμῳ του δ. Et. Mag. The same remark holds good of the Syrian Larissa which was on the Orontes, and the Assyrian on the Tigris. According to Pausanias however, (II. 23) a daughter of Pelasgus, called Larissa, gave her name to these places. Dionysius of Halicarnassus, a little varying the story, makes Larissa to bear three sons, Pelasgus, Achaius and Phthius. Δαυλος according to Strabo (648) signifies a place grown up with wood, and so etymology proves, δα an intensive, ὑλη wood. Here too Pausanias (307 ed. Kühn) tells us of a heroine Daulis. The country of Δωρις, which was remarkably mountainous, derived its name from δα and ορος; yet all the Greek historians represent a Dorus as giving his name to it. The high central district of Peloponnesus was called, from the bears and wolves which harboured in its mountains, Αρκαδια and Λυκαονια, an Arcas and a Lycaon have been invented to account for these names. The promontory of Sarpedon in Lycia, was said to have derived its name from a king Sarpedon, who has in fact derived his existence from the place, the name of which is descriptive of a promontory. *Σαρπηδων ονομα κυριον, παρα το ἁρπη, ὁ σημαινει την δρεπανην* Etym. Mag. The interchange of σ with the spiritus asper is very frequent in Greek, and the name δρεπανη or δρεπανον was given to a great number of promontories. See Index to D'Anville Anc. Geogr. What further proves the truth of this etymology, is, that the name Sarpedonia was given to a promontory of Thrace, with which the supposed king of Lycia could have no connexion. This instance is very remarkable, as proving that, even before the time of Homer, personages had been introduced into history, on no better ground than a false etymology. The object of producing these examples was to show the high probability that Ninus and Ninyas have been invented to account for the name of Nineveh. The most closely analogous example, perhaps, is to be found in the common story of the foundation of Rome. The twin brothers Ρωμυλος and Ρωμος who gave their name to it, were said to have been the children of Mars; and Romulus to have been deified by the name Quirinus. But Quirinus was a title of Mars (Plutarch. Rom.) Whence it is evident, that the real founders of the city being unknown, this title of its tutelary deity had been given to its supposed founder, for whom a name was invented from that of the city. I believe it would not be too much to assert, that wherever we find the founder of a city, &c. said to have been deified, the fact has been that the principal deity has been changed into a sovereign. Dido or Elissa was worshipped at Carthage; I apprehend the fact to have been, that the feminine form of אל was given to the great female divinity whom the Carthaginians worshipped; whence Elissa; Δειδω may have been the name which the Greeks gave to her, from her terrific martial attributes; for she was the armed Juno. "Juno Tyria a Carthaginiensibus colebatur quam deam cælestem vocabant, aliis que variis nominibus cultam legimus. Apuleius Miles. VI. Sive celsæ Carthaginis, quæ te virginem, vecturâ leonis cœlo commeantem, percolit." Rasche Lex.Num.II. 2. 1164. But she was also Astarte and Venus. " Dea in numo expressa est Carthaginis Astarte quæ Plutarcho in Crasso Juno aut Venus Dicitur *a Didone in eam translatâ ;* ab aliis Venus Cælestis vocatur." Rasche II. 2.757. We have already seen that the Venus Cælestis was armed. Dido was said to have a sister Anna; but we learn from Eustathius that Αννα was a name of Dido herself. Dionys. Perieg. 195.

(26) Thus Ceres was the earth; and Proserpine, who represented the same part of nature, was made her daughter. Υπεριων an epithet of the Sun (ὑπεριονος ἠελιοιο Hom. Od.I.8.) is made the father of Ηλιος Apollod. I. 2. Φαεθων, an epithet of the same divinity, is also made his son. Derceto herself is made the daughter of Venus. Schol. Arab. Phæn. p. 32. with the correction of Larcher, Essai sur le culte de Venus p. 19.

(27) Manilius Astron. IV. p. 98. ed. Scal.

Scilicet in piscem sese Cytherea novavit
Quum Babyloniacas summersa profugit in undas
Anguipedem alatis humeris Typhona furentem.

Many other passages which show the Syrian goddess to have been Venus, may be seen in the memoir of Larcher ubi supra. Tzetzes Chil. IX. 275. attributes this transformation into a fish to Semiramis, a fresh proof of her identity with Derceto and Atergatis.

(28) Diod. Lib. II. c. 6. Justin. I. c. 2.

(29) Quid nobile gessit
Eunuchus ?—prima Semiramis astû
Assyriis mentita virum, ne vocis acutæ
Mollities, lævesque genæ se prodere possent
Hos sibi conjunxit similes. Claud. Eutrop. I. 336.

Semiramis, regina illa vetus, quæ teneros mares castravit omnium prima.

Ammian. Marcell. 14. 6.

(30) Thus Jupiter is said to have committed incest with Proserpine. Such representations would naturally arise, since the male and female divinities with different names, were most of them radically the same. In the present instance Belus, Ninus and Ninyas, are only three different forms, as we have seen, of the great male deity. Semiramis alone represents the female, and consequently is given to them all as consort. According to Conon apud Phot. she was the daughter of Ninus and married her own son, who must therefore have been a successor of Ninyas; according to others, she was incestuously the wife of Ninyas; the general account makes her the wife of Ninus; Malela the wife of Belus. Now can it be explained on any historical principles, how it should be uncertain respecting a personage of such celebrity, whether she were the wife of Belus or his great-grandson? yet this is a small part of the contradictions which exist among historians respecting this queen. I shall quote here a note of Mr. Bryant on this subject. "It may be worth while," says he, "to observe the different opinions of authors respecting the time when Semiramis is supposed to have lived.

According to Syncellus before Christ.........	2177
Petavius..	2060
Helvicus............	2248
Eusebius..............	1984
Abp. Usher.......................................	1215
Philo-Byblius from Sanchoniathon (apud Euseb. Præm. Evang. l. I. p. 31.) about	1200
Herodotus about................................	713

What credit can be given to the history of a person, the time of whose life cannot be ascertained within 1535 years?"

Anc. Myth. II. 382. When this learned author proceeds to account for the histories of Ninus and Semiramis from the conquests of a people called Semarim, he departs from probability quite as widely as the common narrative. There is not the smallest proof that the Assyrians were ever called Semarim. The origin of the name Semiramis is not clear: the ancients say it means a dove; but the Semitic languages furnish no natural etymology which could give this meaning. Some have had recourse to the Sami-Rama of the Hindoos, but in the present state of our knowledge of their mythology, it is building on a quicksand to argue from such resemblances. As Semiramis was evidently originally a goddess of the watery element, perhaps her name may have been derived from the words שר המים. Before I quit this part of the subject, I shall observe that the whole of the curious story of Combabus, connected with the temple of Hierapolis, is a fiction to account for a name, the meaning of which had been lost. Κομβη (see Hesych) was a title of Cybele and Κομβαβος the name of her priests: this accounts at once for the story connected with the supposed lover of Stratonice. The other story which the Pseudo-Lucian relates of Combabus being beloved by Juno, affords a glimpse of the mythological origin of the whole.

(31) See Athenæus. Lib. 12. c. 40.

Εγω Νινος παλαι ποτ' εγενομην πνευμα—
εχω δ' ὁκοσσον εδαισα χ' ὡκοσσ' ηεισα
χ' ὡκοσσ' ερασθην. Compara Cic. Tus. Q. v. 35.

"Sardanapallus—incidi jussit in busto

Hæc habeo quæ edi, quæque exsaturata libido
Hausit; at illa jacent multa ac præclara relicta.

It may further be remarked here, that some make Semiramis several generations later, and to have been called Atossa (Euseb. Can. Lat.) her father Balochus, however, is only Belus with a slight alteration, and her reigning jointly with him, another term given to the same masculine character, by which she usurps the power of Ninus.

(32) See in Diod. Sic. II. 25. the valour with which Sardanapallus defends himself. Wesseling observes, "Mira profecto metamorphosis, qui omnem inter concubinas & effeminatorum catamitorum greges vitam exegerat, repente omnium fortissimus imperator est. Sæpe quidem *victis redit in prœcordia virtus,* raro feminæ, nedum quâvis meretriculâ corruptiori. Verius Hellanicus in Schol. ad Comici Aves 1022 & Callisthenes apud Suidam duo fuisse perhibent Sardanapallos quorum alter δραςηριος & γενναιος· alter μαλακος fuerit; qui quidem a Ctesiâ & sequacibus confusi has turbas dedere." It does not appear, however, that these writers had any other reason for maintaining that there were two, than the difficulty of reconciling the contrarities which have been mentioned, and if this be a good reason for supposing two Sardanapalli, all the other personages must be doubled; for the martial character of Ninyas and Ninus are equally inconsistent with their other traits. To suppose two personages of the same name, is a very cheap expedient for clearing up difficulties; yet even this will not suffice here; for the learned Freret has shown, that *three* are necessary to account for all that historians have recorded of Sardanapallus. "Will the line stretch out to the crack of doom?" Instead of raising these shadowy sovereigns to extricate us from our difficulties, would it not be more reasonable to question the historical truth of accounts which are palpably inapplicable to any one individual, when all the rest of the series of Assyrian potentates have shown such strong marks of a mythological origin?

(33) One of the most remarkable circumstances respecting Sardanapallus, is the honour in which he appears to have been held at Tarsus, where we are told, especially by the historians of Alexander, that there was a tomb of him and an inscription which is variously represented, but

which shows him to have been reputed a voluptuary. See St. Croix Hist. d'Alex. 2d. ed. 247.854. If Sardanapallus were really the last sovereign of Nineveh, and such a sovereign as is represented, it is difficult to conceive how he should have founded Tarsus, or have had a tomb there. Tarsus was a city of very high antiquity, and could not have been founded by Sardanapallus 800 years before Christ. Besides, till his last struggle for his crown he spent his days in spinning in his seraglio. Shall we say, that when driven out from Nineveh he fled into Cilicia, and founded Tarsus? Besides the objection to the date, surely this is not the natural employment of fugitive princes; to build Tarsus and Anchiale in a day was a feat, which required the resources of the Assyrian monarchy in its splendour, rather than of the exiled Sardanapallus. I believe that we must seek in the mythological character of Sardanapallus, for a solution of that which presents insurmountable historical difficulties. Though the king Sardanapallus could not be buried both at Tarsus and Nineveh, be an effeminate voluptuary and found two large cities in a day, burn himself at Nineveh, and fly into Cilicia, the divinity Sardanapallus might be worshipped at both, and be converted, by a similar change of mythological into historical characters, into the founder of Tarsus and the last monarch of Nineveh. We have already seen how common it was for tutelary deities to be made founders of cities and leaders of colonies. The Greeks claimed the honour of the foundation of Tarsus for their own Perseus. The manner in which Ammianus Marcellinus (14. 8) speaks of it is very remarkable. Hanc condidisse memoratur Perseus, Jovis filius & Danaes, *vel certe ex Ethiopiâ profectus Sandan quidam nomine* vir opulentus & nobilis. Now as Perseus and Sardanapallus were respectively alleged to be the founders of this city, it is evident that the Sardan of the common story (*phale* is said to mean *noble* in Chaldee) is the Sandan of Ammianus Marcellinus. Æthiopia is here to be taken in the sense which it bears in mythology, i. e. the East and S. E. not as the region above Egypt, its *geographical* sense. Memnon is called an Assyrian and an Ethiopian. In a passage quoted by Valesius, ubi s. from Basilius Saleucencis, on the martyrdom of Thecla, Tarsus is called the city Σανδα του Ἡρακλεους του Αμφιτρυωνος; which leads us to the real fact that Sandan was a title of Hercules. "Agathias ex Beroso, Athenocle, & Symmacho testatur, Hercules Persis Σανδης dicebatur." The Persians proper cannot be meant here, whose worship was of quite a different kind, but the Assyrians, who are often called so by later writers. Thus *Baal* is said to be a *Persian* name for Mars by Joannes Malela. We see then that Sardanapallus, the founder of Tarsus, identifies himself with the deity in Assyrian mythology who answered to the Grecian Hercules—but Hercules and Mars were in this mythology the same; (vid. supra) Sardanapallus is therefore identified with Ninyas, Ninus, Belus whom we have already seen to be the solar god, worshipped under this character. We need then no more suppose two Sardanapalli, from the seemingly inconsistent traits related of him, than that it was one Hercules who played the glutton and the drunkard in the house of Admetus, and spun in female clothes among the maidens of Omphale, and another who established his pillars at the Western extremity of the world, and built cities on the frontiers of India. Antiquaries have doubted whether some of the figures on the coins of Tarsus represent Hercules or Sardanapallus, "Duos Tarsenses numos Begerus exhibet Th. Br. I. 507, quorum figuram stantem Sardanapalli esse contendit, urbis Tarsi conditoris, cui coronam & poculum, luxûs indicia, et suppositum animal quod lupum cervarium arbitratur, voracitatis symbolum, apprimè convenire adfirmat. Quidsi Hercules sit?" Rasche Lex. Num. IV. 2. 21. If the opinion which I have advanced be well founded, the figure in question may represent both these divinities. To the same divinity whom we have seen described under the name of Sandes, I am inclined to refer the Σανδακος of Apollodorus (III. 14) whose whole history shows him to have belonged to the solar divinities; he reckons Phaethon, Tithonus, and Aurora, all whose names are connected with the sun; he marries Φαρνακη; but this was a title by which the moon was worshipped (Strabo 12. 835 Alm.) he has by her Cinyras, whose son is Adonis. Now as all mythologists admit that Adonis is the Sun, it is evident that his progenitor can be no historical personage, but must be a deity of the same class as himself. We find elsewhere *Apollo* and Pharnace for *Sandacus* and Pharnace, evidently with the same reference to the solar worship.

A singular account is given by Stephanus Byzantinus of the foundation of Αδανα or Antiochia on the Sarus as it was sometimes called, a city adjacent to Tarsus. It was founded he says by Adanus and Sarus, who made war with the people of Tarsus and were defeated. Adanus he says was the son of Ουρανος and Γη and was the same as Οστασος and Ανδης (Sandes) and Cronus and Rhea and Japetus and Olymbrus. Obscure as some parts of this account are, it should seem that these Sarus and Adanus have been formed out of the two first parts of the name *Sar-dan*-apallus, and as Adanus is both Cronus and Rhea, it is evident that he united both sexes in one body, as we have seen that the Assyrian deities did, and as was expressly asserted of Adonis (Orph. H. 55. Hist. Poet. Script. Gale. p. 306) whose name is radically the same אדן one of the numerous titles of royalty bestowed on the Sun. When Stephanus adds that the Σαρος was originally called κοιρανος, he appears to confound the name of the river with its meaning. שר, whence Σαρος, really does mean κοιρανος.

A statue lately in the museum at Paris, but now restored to its former situation in the collection of the Pope (Mus. Pio-Clem. Tom II.pl. 31.) exhibits a majestic figure, with a diadem on the forehead, a long beard, a tunic reaching to the feet, and an ample robe; and on the borders of the garment is inscribed CΑΡΔΑΝΑΠΑΛΛΟC: Winckelmann, maintaining that this could not possibly be the effeminate prince who did not allow his beard to grow, in order that he might appear like a woman, gave it the name of the Indian Bacchus, which has since been generally acquiesced in by antiquaries, and the inscription has passed for a later addition. Yet in proportion to the incongruity between the attributes of this statue, and the common accounts of Sardanapallus, is the improbability that any one should affix his name to it. We have already seen that Sardanapallus was not exclusively the frivolous voluptuary, whom history represents him to have been, and as he is identified with Hercules, he has many points in common with Bacchus, who was only the same great luminary the sun, personified with somewhat different attributes, and having a different series of adventures ascribed to him. Perhaps therefore the inscription may be genuine, and Bacchus and Sardanapallus be identified in this curious statue, as we have seen Hercules and Sardanapallus on the coins and in the traditions of Tarsus.

(34) What reliance is to be placed upon ancient traditions, respecting the places of sepulture of kings and heroes, may be seen in the very closely analogous case of Memnon. This celebrated chieftain is said to have been killed at Troy, and several places claimed the honour of being able to exhibit his tomb. The most common account, certainly, represents him as being buried on the banks of the Æsepus, and not far from the place at which he fell. Q. Calaber 2. 584. Strabo 13. p. 404. Pausan. x. 875. Simonides, however, places his tomb in Syria, near

the river Bada, the situation of which is not well ascertained, Comp. Dictys Cret. vi. 10. Others again referred it to the neighbourhood of Ptolemais; Joseph. Bell. Jud. ii. 10. 2. Others said that he had been buried at Susa; Ælian Anim. v. 1.; others in Æthiopia; Diodor. 8. i. p. 136. Wess.; while others alledged that he was never buried at all, but changed into the celebrated vocal statue, which still appears among the ruins of Ægyptian Thebes. Philostr. Icon. i. p. 742. See Jablonski de Memnone.

Is the inference to be drawn from this, that there were six Memnons, of whom these various traditions were respectively true, or that the variety of them shows that there was no decisive evidence for any. There is just as good reason for multiplying Memnons as Sardanapalli, and Freret, to have been consistent, should have extended his argument to them. The fact is, that Memnon, as Mr. Bryant and Jablonski have shown, was one of that multitudinous tribe of heroes, who owe their supposed historical existence to titles of the Sun; never having lived in any country, he was the more easily referred to several, and in each, the ingenuity of the inhabitants readily assigned him a sepulchre.

A DESCRIPTIVE ACCOUNT
OF THE
SEVERAL PROCESSES
WHICH ARE
USUALLY PURSUED IN THE MANUFACTURE OF THE ARTICLE KNOWN IN COMMERCE
BY THE NAME OF
TIN-PLATE.

BY
SAMUEL PARKES, F.L.S. *&c.*

IN A LETTER TO BENJAMIN NAYLOR, ESQ.

(Read Feb. 20th, 1818.)

AS the processes in this manufacture are more numerous and complicated than is generally imagined, it may be advisable to preface the account with an enumeration of some of those properties of tin which will be most likely to explain the rationale of the principal operations.

Tin has a great affinity for several of the other metals—particularly for zinc, mercury, copper, antimony, lead and iron—and owing to these affinities, its employment in the arts is very considerable.

Tin, with zinc forms a metal of close grain, very useful for many purposes, especially for the formation of *pewter*. The zinc is found to impart great hardness to the tin, without lessening its ductility.

The combination of mercury and tin, in which the tin is dissolved by the mercury into a very soft amalgam, is largely employed, as is well known, in silvering the backs of mirrors, and for other purposes in the arts. An amalgam of tin of greater consistence was formerly in use in the museums of Paris for closing the mouths of glass-bottles containing sundry curious and valuable preparations.

Copper is also alloyed with tin for various purposes of manufacture. This metallic mixture is employed in making what are called bronze statues; for casting bells, and pieces of artillery, and also for the fabrication of medals and medallions. In some of these cases the tin is mixed with copper, on account of its property of rendering the copper more fusible; and this was probably the reason why the ancient Romans used that metal in the greater part of their brass coinage. It is owing to the affinity of tin for copper, that vessels of capacity, made with the latter metal, for culinary and other purposes, are so readily covered with a coating of tin, to preserve them from the action of substances which would not fail to erode copper, if unprotected by some such covering. The affinity of tin for copper is farther exemplified by the process of whitening *pins*, which is effected by boiling the pins with granulated tin, in a lie made with alum and tartar.*

A useful alloy is likewise formed by the mixture of tin and antimony. This metallic compound is very white—extremely hard—and will bear a very fine polish. On these accounts it is employed in making specula for telescopes, and also for the manufacture of rolled plates to engrave music upon.

The next metal which I have mentioned as uniting readily with tin—is lead. This metal will combine with tin in any proportion—and in most proportions the lead acquires a greater degree of fusibility by its union with the tin. It is this alloy which forms plumbers' solder—but that compound is prepared with different proportions of tin, according to the purpose for which it is intended. The article called tin-foil, used for lining tea caddies, for coating electrical jars, and for other purposes, is also made from a mixture of these two metals.

* An interesting Memoir on the tinning of common Pins, (by M. Gadolin) will be found in the Journal de Physique for the year 1789.

But what is more relevant to the subject of this paper, is the chemical affinity which subsists between *tin* and *iron*. One of the strongest proofs of this affinity is derived from the circumstance that even *cast*-iron may be tinned in the same manner as wrought-iron. Of late years, cast-iron saucepans, and pots of a large size, are permanently tinned on their inner surfaces, to prevent the liquors which are boiled in them from acquiring any stain by a partial dissolution of the iron. Many other articles, such as bridle-bits, common stirrups, small nails, &c. are now made much cheaper than formerly, by first fabricating them in cast-iron, and then covering them with a thin coat of tin, by the immersion of them in a hot mass of that fluid metal.

That these effects are owing to chemical affinity, cannot be doubted, when it is considered, that in all these cases the pores of the iron are in some degree actually penetrated by the tin.

In the manufacture of *tin-plate*, which I am now about to describe, a similar effect is produced, and also by the same means. Plates of iron properly prepared, are immersed in a large mass of melted tin, which is kept hot by a fire constantly burning underneath it;—the consequence of which is, that the tin in some measure penetrates the iron, and this attaches other tin to it, so that the whole surface of the iron acquires, by this means, a complete covering of that metal.

As no accurate account has ever yet been given of the various processes by which this is effected, the following outline may probably be acceptable to the members of your very respectable Society.

English bar iron of the finest quality, called tin-iron, and which is generally prepared with *charcoal* instead of mineral coak, and made with the greatest care, for this particular purpose, is first cut to the necessary length, and then rolled at the mill, by a process which is peculiar to this manufacture,* into plates of the requisite thinness, and of such form as is suitable for the business. These plates are then cut by hand-shears to

* It would extend this paper to too great a length, or I should like to have given a description of this process, as it is extremely curious. Should the members of the society wish it, I will furnish it.

the sizes suitable to the different markets.* And as the shearer shears the plates, he piles them in heaps, occasionally putting one plate the cross way, to keep each box separate. Two hundred and twenty-five plates are called a box, but they are not put into boxes of wood in this stage of the operation. The iron plates now go into the hands of the *scaler*, who takes them from the shear-house, and bends each of them singly across the middle, into this form Λ, preparatory to their being cleaned for tinning, and for the conveniency of putting them into the scaling furnace, as will be more fully explained hereafter.

This furnace, or oven, is heated by *flame* thrown into it from a fire place of a peculiar construction, and it is this flame that scales the plates, which are put into the oven in rows, and arranged three in each row, until the oven is full. It will be obvious that if they lay flat on the floor of the oven, the

* These plates are generally cut *by hand,* but an ingenious Whitesmith in Glamorganshire, a few years ago invented a method of shearing them by *a machine.* This machine is worked by a water-wheel, and will shear 100 boxes per day—whereas a hand-shearer cannot complete more than 25 boxes in the same period of time.

flame could play only on one side of each plate, whereas, by being bent in the form already described, the flame can operate equally on both sides. It may here be remarked that the form of all tin-plates, one sort excepted, is that of a parallelogram, and that if a piece of stiff paper, or paste-board, 13¾ inches long, and 10 inches wide, be bent in the centre at an angle of about sixty degrees, and then put to stand on the two ends, we shall have the form of a plate No. 1. properly bent for the scaling oven.

The operation of *cleansing*, as it is called, and which is preparatory to the process of scaling, is commenced by steeping the plates for the space of four or five minutes, in a mixture of muriatic acid and water, in the proportion of four pounds of acid to three gallons of water. This quantity of the diluted acid will generally be sufficient for eighteen hundred plates, or eight boxes of 225 plates each.

When the plates have been steeped for the time prescribed, they are taken out of the liquor, and placed upon the floor, three in a row, and then by means of an iron rod put under them, they are conveyed to a furnace heated red-hot, where they remain until the heat takes off the scale, the removal of which was the object in submitting them to that high temperature.

When this is effected, the plates are taken to a floor, where they are suffered to cool—they are then straightened, and beaten smooth upon a cast-iron block. The workman knows by the appearance of the plates during this operation, whether they have been well scaled—for if they have, that is, if the rust or oxide which was attached to the iron, has been properly removed, they will appear mottled with blue and white, something like marbled paper. The operation we have been describing is called *scaling*.

As it is impossible the plates can go through this process without being in some measure warped, or otherwise disfigured, they are now rolled a *second* time, between a pair of cast-iron rollers, properly hardened and finely polished. This operation makes both sides of the plates perfectly smooth, and imparts a sort of polish to their surfaces. These rollers are each about 17 inches long, and 12 or 13 inches in diameter—but I am inclined to think that if the diameter was greater,* they would set the plates flatter, and do the work better in every respect.

* Since the above was written, I have submitted the manuscript to a gentleman who is very largely engaged in

The technical name of this apparatus is *rolls*, not *rollers*. All the rolls which are employed in rolling plates, either hot or cold, in this manufactory, are *hard* rolls—and there is as much difference between a pair of *hard* cast iron rolls, and a pair of *soft rolls*, although they may both be run out of the same pot of metal, as there is between iron and steel. The workmen inform me that the difference is entirely occasioned by the manner of casting them—the soft rolls being cast in sand, whereas the hard rolls are formed by pouring the metal into a thick cast iron box—and that the metal, by coming in contact with the cold box is sufficiently chilled to render the whole face of the roll entirely hard. The difference in the temper of these two kinds of rolls is so great, that when they are put into the lathe to be turned perfectly true, the turnings from the one will be $\frac{1}{8}$th of an inch in thickness, whilst the turnings which come from the other are not larger than very fine needles. The temper of cast iron thus varying according to the nature of the mould into which it is poured, is a cir-

the manufacture of tin-plates, and he tells me that the cold rolls which are employed in his work, are 30 inches in diameter.

cumstance that appears to me to be deserving of attention in the manufacture of a variety of other utensils employed in the arts.*

These rollers are used without heat, but they are screwed down very close one upon the other, only leaving bare room for the plates to pass, that the utmost attainable degree of pressure may be given to them. This last operation is called COLD ROLLING.

When the plates have undergone this process, they are put one by one into troughs filled with a liquid preparation called *the lies.*

This is merely water, in which bran has been steeped for nine or ten days, until it has acquired a sufficient acidity for the purpose. The design of putting the plates into the troughs *singly,* is, that there may be more certainty of the liquor getting between them,

* The art of making good hard rolls appears to be very imperfectly understood, because the difficulty of producing them is very great. Not one in three can be called thoroughly good. For if they are not sufficiently hardened on the surface, they will not wear, whereas if they are made hard throughout, or *struck hard to the centre,* as the workmen call it, they will generally crack across the middle and become useless. This fault is quite independent of air bubbles or flaws, which are always discoverable in turning by the lathe.

and both the sides of every plate being soaked alike in the lies. In this liquor they remain for ten or twelve hours standing on the edges, but they are turned, or inverted, once during that time. This operation is called *working* in the lies.

The next operation is that of steeping in a mixture of sulphuric acid and water, in proportions which vary according to the judgment of the workmen.

The trough in which this operation is conducted, is made with thick lead, and the interior is divided by partitions of lead. Each of these divisions is by the workmen called *a hole*, and each of them will contain about one box of plates. In the diluted sulphuric acid which is in the different compartments of this vessel, the plates are agitated for about an hour, or until they have become perfectly bright, and entirely free from the black spots which are always upon them when they are first immersed in it.

Some nicety however is required in this operation, for if they remain too long in the acid, they will become stained, or blistered by it, as the workmen term it; but practice enables a careful operator to judge of the time when they ought to be removed. This, however, is one of the most difficult parts of the business, as few men like to work in it; though I understand that a good Pickler is highly valued by his employers, and obtains great wages. It is necessary to notify that this, and the former process with the acidulated water, are both hastened by giving to those menstrua an increase of temperature—and this is effected by means of heated flues which run under each trough. Little additional heat is necessary in summer, however, as 90° or 100° of Fahrenheit is a temperature sufficiently high for either of these operations.

When the plates come out of the pickle, they are put into pure water, and scoured in it with hemp and sand, to remove any remaining oxide, or rust of iron that may be still attached to them, for wherever there is a particle of rust, or even *dust* upon them, there the tin will not fix; and they are then put into fresh water to be there preserved for the process of tinning. The design of putting the plates into pure water, after they come out of the sours, is to prevent their becoming again oxidated—for it has been found that after these operations, they will acquire no rest, although they should be kept twelve months immersed in water.

It will be perceived that all these processes are nothing more than preparatory measures for the operation which is to succeed, viz. that of TINNING.

For this purpose an iron pot is nearly filled with a mixture of *block* and *grain* tin, in a melted state; and a quantity of tallow or grease, sufficient, when melted, to cover the fluid metal to the thickness of four inches, is put to it. However, as some gentlemen may not be acquainted with the difference between *block* and *grain* tin, it may be remarked, before we proceed, that the metal known in commerce by the name of block tin, is prepared either from the mineral called *tin-stone*, or the one known in Cornwall by the name of *tin-pyrites*, whilst the article called *grain tin* is smelted from an ore which is found in grains called *stream tin* ore, under beds of alluvial soil, in low situations, whither in the course of ages, it has been washed from the hills by a succession of torrents of rain. The former, which is produced in the greatest abundance, is always contaminated with a portion of iron, sulphur, and other injurious substances, and is therefore only employed for common purposes—while the grain tin, which is nearly free from every impurity, and usually from twenty to thirty shillings per hundred weight dearer, is used in the processes of dyeing, and in all other cases where *pure* tin is required. I am also desirous of remarking, that in my opinion, it would be more profitable to the proprietor of a tin-plate work, if he were to use *grain* tin alone, or grain tin mixed with that kind which is known by the name of "Refined Tin," because these kinds not only contain less dross, but they melt, as I know by my own experience, into a more fluid metal; and consequently, more would run off the plates in the operation of tinning, and less tin would be consumed. At present these manufacturers use the block and grain tin, in equal proportions.

When the tin-pot has been charged in the way above mentioned, the metal is heated from a fire-place underneath it, and by flues which go round the pot, until it is as hot as it can be made without actually inflaming the grease which swims upon its surface. The use of the grease is to preserve the tin from the action of the atmosphere, and consequently to prevent it from oxidating. By melting a little tin or lead in an iron ladle, and, when the dross is skimmed off, putting a morsel of tallow upon the metallic fluid, the effect of the tallow in cleansing the face of the metal will be evident. The workmen also say, that increases the affinity of the iron for the tin, or, as they express it, that it makes the iron plates take the tin better.

It is curious that *burnt grease*, or any kind of empyreumatic fat, effects this purpose better than pure fresh tallow.

Another pot, which is fixed by the side of the tin-pot, is filled with grease only; and in this the prepared plates are immersed, one by one, before they are treated with the tin; and when the pot is filled with them, they are suffered to remain in it so long as the superintendant thinks necessary. If they remain in the grease an hour, they are found to tin better than when a shorter time is allowed them.

From this pot they are removed, with the grease still adhering to them, into the pot just before spoken of, which contains the body of melted tin; and in this they are placed in a vertical position. Three hundred and thirty-eight, or three hundred and forty plates are usually put into this pot at once;* and

* It is immaterial how many plates are put into the tin-pot at once—but this number is usually fixed upon, because it amounts to about one box and a half of plates; though, as they are put in edgewise, it is necessary the pot

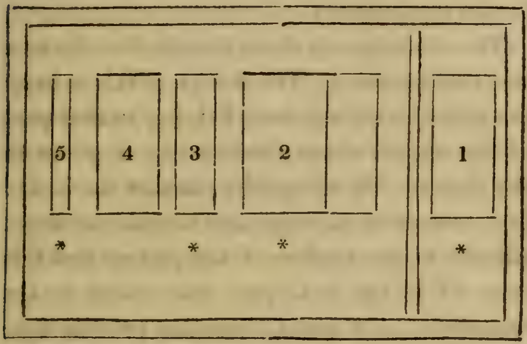
for the sake of their being thoroughly tinned, they usually remain in it one hour and a half; but occasionally more time is required to complete this operation.

When the plates have lain a sufficient time immersed in the melted tin, they are taken out and placed upon an iron grating, that the superfluous metal may drain from them; but, notwithstanding this precaution, when they become cold there is always more metal found adhering to them than is necessary; and this is taken off by a subsequent process, called *washing*. As this process is rather complicated, it will be necessary to describe it with some minuteness.

In the first place, the wash-man prepares an iron pot which he nearly fills with the best grain tin in a melted state—another pot of clean melted tallow, or lard free from salt—a third pot with nothing within it but a grating to receive the plates—and a fourth, called the *listing-pot*, with a little melted tin in it, about enough to cover the bottom to the depth of a quarter of an inch. The whole will however be better understood by

should be nearly filled, to prevent their falling down—in which case they could not be got out, through so heavy a body of metal, without much difficulty.

referring to the following drawing, which exhibits the several vessels in the order in which they stand in the manufactory, all supported by substantial brick work.



The building in which the pots are fixed is called the *Stow*. The plates are worked from the right to the left of the stow, as will be evident by attending to the uses of the separate pots.

No. 1. represents the tin-pot.
2. The wash-pot with the parting within it.
3. The grease-pot.
4. The pan, containing a grating at the bottom.*
5. The list-pot.

* This pan is designed for the reception of the plates as the boy takes them out of the grease-pot. It has no fire underneath it.

The drawing represents the *surface* of the pots. The asterisks shew the places where the workmen stand, and also mark those pots which have heated flues under them. No. 4. has no fire under it.

The parting in the wash-pot No. 2, is a late improvement. The design of it is to keep the dross of the tin from lodging in that part of the vessel where the last dip is given to the plates. By using the *common* tin in the first process of tinning, much oxide, or dross, adheres to the surface of the plates, and this runs off in the wash-pot, and comes to the face of the new metal—but this parting enables the operator to prevent it from spreading over the whole surface of the pot. Were it not for this parting, the wash-man must skim the oxide off the fluid metal every time he puts plates into it.

The pots of which I have given a sketch being all in a state of fitness, the wash-man commences his part of what remains of the business, by putting the plates which have undergone the various operations hitherto described into the vessel of grain-tin, called the wash-pot.* The heat of this large body

* None but *grain*-tin is ever put into this vessel, for the whole of the common tin which is consumed in such

of melted metal soon melts all the loose tin on the surface of these plates, and so deteriorates the quality of the whole mass, that it is usual, when sixty or seventy boxes have been washed in the grain tin, to take out the quantity of a block, say three hundred weight, and replenish the wash-pot with a fresh block of pure *grain*-tin. These vessels generally hold three blocks each, or about half a ton weight of metal. That which is taken out of the wash-pot when it is replenished with pure metal, is given to the tin-man to put into his pot.

When the plates are taken out of the wash-pot, they are carefully brushed on each side with a brush of hemp of a peculiar kind, and made expressly for the purpose. As this part of the business requires considerable adroitness and expedition, it may be worth while to explain it a little more in detail.

The wash-man first takes a few plates out of the wash-pot, and lays them together before him on the stow,—he then takes one plate up with a pair of tongs, which he holds in his left hand, and with a brush held in his right hand brushes one side of the plate—he

manufactories, is used in the *first* process, viz., that which is called *tinning*.

then turns it, and brushes the other side, and immediately dips it once more into the hot fluid metal in the wash-pot, and without letting it out of the tongs, instantly withdraws it again, and plunges it into the grease-pot (marked No. 3.) adjoining to the wash pot from whence he had just taken it.

A person who has not seen the operation, can form but a very inadequate idea of the adroitness with which this is performed—practice, however, gives the workman so much expedition, that he is enabled to make good wages, although he obtains only three pence for the brushing and metallic-washing of 225 plates. I am informed that an expert wash-man, if he makes the best of his time, will wash 25 boxes, consisting of 5625 plates in twelve hours; notwithstanding every plate must be brushed on both sides, and dipped twice into the pot of melted tin.

Why the plates should be dipped *twice* during this part of their manufacture, may perhaps require some explanation. It must be recollected that they are brushed quite hot, and before the tin is set, therefore, if they had not the last dip, the marks of the brush would be visible. Moreover, the brush takes the greatest part of the tin off them, so that if they were removed to the grease-pot without being re-dipped, the hot grease would take off what remained.

The only use of the grease-pot is to take off any superfluous metal that may be upon the plates—but this is an operation that requires great attention, because, as the plate is immersed in the grease while the tin is in a melting, or at least in a soft, state upon it, a part *must run* off, and the remainder become less and less while the plate continues in it; therefore, if these plates should ever be left in the melted tallow longer than is absolutely necessary, they will doubtless require to be dipped a third time in the tin. On the other hand, if the plates were to be finished without passing through the grease, they would retain too much of the tin, which would be a loss to the manufacturer; and besides, the whole of the tin would appear to be *in waves* upon the iron.

It is also equally necessary to attend to the temperature of the melted tallow, which must be colder or hotter in proportion as the plates are thicker or thinner; for if, when the tallow is of a proper temperature for a thin plate, a thick one was to be put into it, it would come out, not of the colour of tin as it ought to be, but as yellow as gold. The reason of this is evident. The thick plate contains more heat than a thin one, and consequently requires the tallow to be at a lower temperature. On the contrary, if a parcel of *thin* plates were to be worked in a pot of tallow which had been prepared for *thick* plates, such a pot would not be hot enough to effect the intended purpose.

It is a common observation that in most of our manufactures, and in all chemical speculations, theory and practice are generally at variance—but there are few manufactures perhaps, where there are so many minutiæ which would escape the notice of a casual observer, and yet that require to be carefully attended to, in order to produce a good result, as in that which we have now been describing—and should the perusal of this paper occasion but one individual to pause, who was about to enter into a new concern with which he was only partially acquainted, I shall have written to a good purpose.

But to return to the process. When the plates are sufficiently brushed, they are again immersed one by one in the pot of melted tin, as has already been remarked, and immediately from this they are put into the pot of tallow above mentioned. This pot has pins fixed within it, in such a manner as to prevent the plates from touching each other; and this part of the process is conducted in the following manner.

When the wash-man has passed *five* of the plates through the melted tin, and from thence into the pot of tallow above mentioned, a boy takes out one of them and puts it into the empty pot to cool, and the wash-man puts in the *sixth* plate. The boy then takes out a *second* plate, and lays it to cool likewise; when the man puts in his *seventh*, and so they go on, in this regular manner, until the whole of the parcel is finished.

In consequence of the plates being immersed in the melted tin in a vertical position, there is always, when they have become cold, a wire of tin on the lower edge of every plate which is necessary to be removed, and this is done in the following manner.

A boy, called the list-boy, takes the plates when they are cool enough to handle, and puts the lower edge of each, one by one, into the list-pot, which is the vessel that was before described, as containing a very small quantity of melted tin, and the same as that which I have marked No. 5. When the wire of tin is melted by this last immersion, the boy takes out the plate, and gives it a smart blow with a thin stick, which disengages the wire of superfluous metal, and this falling off, leaves only a faint stripe in the place where it was attached. This mark may be discovered on every tin plate which is exposed for sale—the workmen, in the manufactory of them, call it the *list*.

Nothing now remains but to cleanse the plates from the tallow. This is done by means of bran, and as they are cleansed they are put into strong wooden boxes, or boxes of *sheet-iron*, made exactly to fit them; and this completes the whole business. Each box contains a determinate number of plates, and the following table will shew the different sizes of tin plate which are made in Great Britain, and the marks by which each kind is known in Commerce.

Names	Sizes	No. in a Box	Weight of each Box			Marks put on the Boxes
	Inch					
Common......No. 1.	13¾ by 10	225	1	0	0	CI
Do.............. 2.	13¼ by 9¾			3	21	CII
Do.............. 3.	12¾ by 9½			3	16	CIII
Cross ....No. 1.	13¾ by 10		1	1	0	XI
Two Cross........ 1.			1	1	21	XXI
Three Cross...... 1.			1	2	14	XXXI
Four Cross........ 1.			1	3	7	XXXXI
Common Doubles..	16¾ by 12½	100		3	21	CD
Cross Doubles			1	0	14	XD
Two Cross do.........			1	1	7	XXD
Three Cross do......			1	2	0	XXXD
Four Cross do.......			1	2	21	XXXXD
Com. small Doubles	15 by 11	200	1	2	0	CSD
Cross do. ...do.....			1	2	21	XSD
Two Cross ...do.....			1	3	14	XXSD
Three do. .....do.....			2	0	7	XXXSD
Four do. .....do.....			2	1	0	XXXXSD
Wasters Com. No. 1.	13¾ by 10	225	1	0	0	WCI
Do. Cross.... 1.	13¾ by 10		1	1	0	WXI

A List of the Current Wholesale Prices of Tin-Plate in September 1817, in London.

Sorts	Weights			Prices per Box	No. in each Box
	lb.		lb.	s.	
C No. 1	112	to	115	41	225
C 2	103	...	106	39	
C 3	98	...	101	37	
X No. 1	140	...	142	49	
XX 1	160	...	163	55	
XXX 1	182	...	185	61	
CD	98	...	103	37	100
XD	126	...	129	45	
XXD	147	...	150	51	
XXXD	168	...	171	57	
CSD	167	...	170	63	200
XSD	188	...	191	69	
XXSD	209	...	212	75	
Wasters CI	112	...	115	35	225
Do. XI	140	...	143	43	

Having given so detailed an account of the manufacture of tin-plate, it may be expected that I should say a few words on the *origin* of the art.

Formerly none of the English workers in iron or tin had any knowledge whatever of the methods by which this useful article could be produced; our ancestors, from time immemorial, having supplied themselves with it from Bohemia and Saxony. The establishment of this manufacture in those districts, was doubtless owing to their vicinity to the tin mines in the circle of Ersgebirg, which, next to those of Cornwall, are the largest in Europe. The ore which is found there is not the *tin pyrites,* but the mineral called *tin-stone*; and it is curious that it should occur in abundance, both on the Bohemian and Saxon sides of the mountain group—accordingly, manufactories of tinned iron have been established in both those kingdoms. Alluvial deposits of grain-tin are also found in the same vicinity.

From the time of the invention of tin-plate to the end of the seventeenth century, not only England, but also the whole of Europe depended upon the manufactures of Bohemia and Saxony for their supply. However, about the year 1665 one Andrew Yarranton, encouraged by some persons of property undertook, to go over to Saxony to acquire a knowledge of the art—and on his return, several parcels of tin-plate were made of a superior quality to those which we had been accustomed to import from Saxony; but owing to some unfortunate and unforeseen circumstances, which are all detailed by Mr. Yarranton,* the manufactory was not at that

* "England's Improvement by Sea and Land," with many plates of Plans, Charts, &c. in two parts, by Andrew Yarranton, Gent. Part I. Quarto. London, 1677. Part II. London, 1681.

time established in any part of Great Britain.

As it is now difficult to procure a copy of the work from which I have obtained a knowledge of the manner in which this manufactory was brought into England, an abridgment of the author's own account of the transaction may perhaps be interesting to some of the members of the Society.

"Knowing," says Mr. Yarranton, "the usefulness of tin-plates, and the goodness of our metals for that purpose, I did (about sixteen years since*) endeavour to find out the way for making thereof; whereupon, I acquainted a person of much riches, and one that was very understanding in the iron manufacture; who was pleased to say, that he had often designed to get the trade into England, but never could find out the way. Upon which it was agreed, that a sum of monies should be advanced, by several persons, for the defraying my charge of travelling to the place where these plates were made; and from thence to bring away the art of making them. Upon which, an able fire-man, that

* This account is dated February 2nd, 1681; I therefore conclude that Mr. Yarranton's journey to Saxony must have been about the year 1665.

well understood the whole nature of iron, was made choice of to accompany me; and being fitted with an engenious interpreter, that well understood the language, and that had dealt much in that commodity, we marched first for Hamburg, then to *Lipswick ;* and from thence to *Draisden,* the duke of *Saxomes* Court, where we had notice of the place where the plates were made; which was in a large tract of mountainous land, running from a place called *Seger-Hutton,* unto a town called *Awe,* being in length about twenty miles; the tin-works being there fixed upon a great river running clear along the valley, and also upon some little rivulets that run out of the mountains of Bohemia and Saxony; and coming to the works, we were very civilly treated, and, contrary to our expectation, we had much liberty to view, and see the works go—with the way and manner of their working and extending the plates, as also the perfect view of such materials as they used in clearing the plates, to make them fit to take tin, with the way they use in tinning them over, when cleared from their rust and blackness. And having (as we judged) sufficiently obtained the whole art of making and tinning the plates, we then came for England, where the several persons concerned in the affair thought fit to make some trial in making some small quantities of plates and tinning them, which was done; and all workmen that wrought upon them agreeing that the plates were much better than those which were made in Germany; upon which, preparation was making to set this beneficial thing at work;—but, it being understood at London, A PATENT was trumpt up, and the patentee was countenanced by some persons of quality—and what, with the patent being in our way, and the richest of our partners being afraid to offend great men in power, who had their eye upon us, it caused the thing to cool, and the making thereof was neither proceeded in by us, nor possibly could be by him that had the patent; because neither he that hath the patent, nor those that have countenanced him, can make one plate fit for use."*

This enterprising individual, who spent the greater part of his life in promoting schemes for the good of his country, and who, in the opinion of Bishop Watson, ought to have had a statue erected to his memory,†

* England's Improvement, &c. page 149—152 Part II.

† The following particulars which I have collected respecting Mr. Yarranton, will tend to justify the good Bishop

proceeds to inform us that before they were stopped by the patent, they had made " many thousand plates from iron raised in the forest of Dean, and tinned them over with Cornish tin, and the plates proved far better than the German plates, by reason of the toughness and flexibleness of our forest iron. One Mr. Dison, says he, a tinman in Worcester, one

in this opinion. He was bound as an apprentice, early in life, to a linen draper, but after some years he left that situation in disgust. In the year 1652 he took some iron works, which he carried on for several years; and during this period, he made regular surveys of the three great rivers in England, and by means of associations which were formed by himself, he rendered three other rivers navigable—he studied agriculture with such effect, that many of the arable estates in the midland counties were rendered doubly productive by the new methods of husbandry which he either brought from abroad, or discovered;—he laid a plan for the junction of the Thames and Severn *at that spot* where of late years this very scheme has been effected;—he proposed the cutting of several navigable canals, half a century before any such project had been executed in this country. He made the necessary surveys and planned docks for the cities of London and Dublin;—besides his journey to Saxony already mentioned, he went to Holland, under the patronage of the ancestors of some of our present nobility, to examine the inland navigations of the Dutch, and to investige the nature of their linen manufactures;—and on his return promulgated the plan for a new manufacture of linens, which he calculated would employ all the poor of England.

Mr. Lydiate near Fleet-bridge, and one Mr. *Harrison* near the Kingsbench, have wrought many, and they know their goodness."*

In another place this interesting writer informs us that " when he was in Saxony, the different establishments for making tin-plates were very numerous, and that most of them belonged to the duke."† " The trade," says he, " is so great, that by computation, no less than 80,000 men depend upon it, and when the plates are finished, they are sent by land to *Lipsick*, from thence to the *Elbe* river, and so down to *Hamburg*, and from thence sent by sea as far as trade is known."‡ " There was," says he, " no tin any where in Europe, except in Cornwall, until a Cornish man found tin in the mountains of Saxony, near a town called *Awe*, where *his statue is yet to be seen.* The tin works are fixed upon

He published schemes for the improvement of our national fisheries; he made several tours through Ireland, for the express purpose of planting new manufactures and devising the increase of the staple trades of that country;—He made a regular survey and estimate of the expence of rendering the river *Slane* in Ireland navigable, for the purpose of bringing timber down to the coast for his Majesty's navy; and rendered many other signal services to his country.

* Page 173. † Page 155. ‡ Page 172.

a great river running down the valley—and the tin, iron and woods, grow in and upon the mountains adjoining to both sides the river; and those tin-works have proved so beneficial to the place, that there are several fine cities raised by the riches proceeding therefrom."* He adds, "the trade of making tin-plates was about sixty years since fixt in Bohemia, and had there long continued, but the woods decaying, and there being at that time a wise duke of Saxony, willing and ready to improve his own revenue, and his subjects, did accept of directions how this trade might be brought away and fixt in the duke of Saxony's territories†—A Romish Priest, converted to be a Lutheran, was the chief instrument in the whole affair, until it was perfected—and a Cornish miner, a Protestant, who had been banished out of England for his religion, found out the tin in Saxony—both which persons proved instruments of great wealth to that duke and country."‡

Notwithstanding Mr. Yarranton had so completely introduced the knowledge of

* Yarranton, part II. page 176. † Ibid. page 178.

‡ Saxony is only separated from Bohemia by a chain of mountains called the *Erzegeberg*; which in German signifies hills that contain mines.

making tin-plate into this country, I do not find that any manufacture of that article was established in these kingdoms until some time between the years 1720 and 1730, which must have been long after Mr. Yarranton's death. The first establishment of this kind was, I believe, fixed in Monmouthshire, where it continued to flourish many years.*

About the time that this manufactory was established, the amiable and intelligent M. Reaumur, to whom the French are indebted for a new mode of graduating the thermòmeter, and for many discoveries and improvements in the arts,†—undertook to discover the method of making tin plates for the

* Upon further enquiry, I find that this was at the town of Ponty-Pool; and it is remarkable, that after the lapse of nearly 100 years the manufacture has recently been re-established at the same town, on a very extensive scale.

† It was Mons. Reaumur who was the means of introducing into France the methods of making Porcelain; for when Francis D'Entrocolles, who had resided many years in China as a Christian Missionary, sent to France specimens of the materials used by the Chinese in their Porcelain, M. Reaumur immediately instituted a series of experiments to discover the method of imitating their productions, and in the years 1727 and 1729, communicated the result of his researches to the Academy of Sciences; and his two memoirs were published by the Society, in their Transactions.

French people. This eminent man, whose mind was cast in a mould very similar to that of Mr. Yarranton, but who possessed more science, never relinquished any thing which he undertook; and accordingly, notwithstanding the innumerable difficulties which he had to encounter, at length succeeded in acquiring such a knowledge of the principles of the manufacture, as enabled him to instruct several people in the vicinity of Paris, in an art which, until then, had never been practised in that country.

Soon after the time of which I am speaking, several similar manufactories were erected in Great Britain, and now the establishments of this nature are so numerous and extensive in many parts of these kingdoms, that the manufacture of tin-plate is become of great national importance, and more than one hundred thousand boxes of these plates are annually exported.

January 12th, 1818.

THE LAWS OF STATICAL EQUILIBRIUM ANALYTICALLY INVESTIGATED.

BY

MR. JOHN GOUGH.

COMMUNICATED BY DR. HOLME.

THE theorem, which is frequently called the parallelogram of forces, contains the fundamental principles of dynamics. On this account the young mathematician ought to use his best endeavours to acquire clear notions relative to the proposition in question; because it is of the first importance in the science of mechanics. Many writers on this subject, more especially those on the continent, object to the common demonstration of the theorem, because motion is made use of in demonstrating it, whereas the idea of Statical Equilibrium is in direct opposition to the idea of motion. In conformity with this objection the French mathematicians assume as an axiom, a proposition, which shall be pointed out and demonstrated in the course of the present essay, and then have recourse to an analytical process, in the perusal of which the reader loses all sight of the idea of force and has his mind totally engaged with algebraic symbols. I shall on the contrary endeavour, in the following train of reasoning, to keep the idea of force in view as much as possible; for which purpose, the geometrical analysis will be used, in preference to algebra; because the diagrams of the former method, constantly recall the attention of the reader to the elementary principles of his subject.

Article 1.—Def. The term statical equilibrium is used by certain modern writers on mechanics, to denote an opposition of forces, which does not produce motion.

Art. 2.—Axiom. If a number of causes act in conjunction, their joint effect differs from the effect which would be produced by any one of the number acting separately. Should this be objected to as an axiom, we may observe that the contrary proposition supposes causes to act without effects, consequently such are no causes at all.

Art. 3.—Axiom. Every force acts in a right line, which line is called its direction.

Art. 4.—Axiom. Equal forces acting in

opposite directions, in the same right line, produce an equilibrium, by counteracting each other's effects: but no two forces, whether equal or unequal, can maintain an equilibrium, the directions of which include an angle; for in the latter hypothesis the direct opposition is wanting, which alone gives the title of an axiom to the preceding assertion.

Art. 5. Suppose F and G to represent two forces in magnitude, which act conjointly upon a material point B, in the directions (or right lines) AB and CB, Fig. 1. including the angle ABC. These things being stated, it follows that if F or G were to act separately, B would be urged in AB or CB by Art. 3, but F and G act conjointly: therefore B is urged in neither of these directions by Art. 2, moreover AB and CB include an angle ABC; consequently F and G do not keep the point in equilibrio by Art. 4.; hence B is urged in a right line by Art. 3; let this be DB. Draw SC meeting DB at right angles at D; now since F and G urge B towards different parts, while this point remains in DB, it is evident from Art. 4, that B is retained in DB by two equal and contrary forces, acting at the angle B parallel to AC, in the opposite directions AD, CD; therefore DB is in the plane BAC; and it divides the angle

ABC. For if BD be not in the plane BAC, then BACD is a pyramid, and the solid angle D is contained by three triangles; of which ADC is one; therefore the sides AD, CD include an angle; consequently the point B cannot remain in BD; but it has been shewn to remain in BD.

Art. 6. Let K denote in magnitude the force, with which B is urged by the joint action of F and G in the right line DB; then F, G, are called the components, and K their equivalent. These things being stated, it will appear evident from Art. 4, that if a force equal to K be exerted at any point of DB produced or not, in opposition to the joint effect of F and G, action and reaction will take place, without the production of motion, which is a statical equilibrium, by Art. 1. It is easily proved in like manner, that if F and K be two forces acting in the right lines AB, DB, and a third force G be opposed to their joint effect in the line CB or CB produced, statical equilibrium will ensue; viz. G is the equivalent of F and K; hence any one of the three forces F, G and K, is the equivalent, and the remaining two are its components.

Art. 7. It appears from Art. 5, that the point B is retained in DB by two equal and contrary forces acting at B perpendicular to

DB; but F and G are the only forces which affect the state of B; consequently parts of these forces are destroyed in maintaining this equilibrium, and the remaining parts constitute a quantity K; hence the sum of the components is greater than their equivalent.

Art. 8. All that has been concluded respecting F, G and K, relates to abstract forces having no particular ratios or differences; consequently if three right lines be taken proportionate to these magnitudes, a triangle may be formed of them; because any two are greater than the third: conversely the three sides of any triangle may represent three forces in magnitude; any two of which are the components of the remaining one.

Art. 9. Let AB, CB, fig. 2, including the angle ABC, denote two forces, F and G, in magnitude and direction; also, suppose BM to be the direction of their equivalent: join AC meeting BM in M; through B draw PR perpendicular to BM; and make AP, CR perpendicular to PR. Then as F is to the force in PB, so is AB to PB; and as G is to the force in RB so is CB to RB; but as F is to G, so is AB to CB by hyp.; therefore as the force in PB is to force RB, so is PB to RB. Now these forces are equal and contrary by Art. 7, hence PB = RB; but BM is parallel to AP and CR, therefore as PB is to BR, so is AM to MC; hence AM = MC. (Eucl. VI. and 2.) complete the parallelogram ADCB, and BM produced will evidently pass through the angle D; therefore the diameter BD, gives the direction of the equivalent K. Again make AT perpendicular to BD, and as BTAP is a parallelogram BT = AP, but AB represents F, therefore AP or BT represents that part of F which acts in the direction DB by Art. 7 and 8; for the same reason CR represents the part of G which acts in DB; but the triangle CBR and DAT are similar and equal, consequently BT + TD or BD expresses K in magnitude, which it also represents in direction.

Art. 10. The angle ADB = angle CBD; therefore their sines are equal; and we have AB × sine ABD = BC × sine CBD; hence as AB : CB :: sine CBD : sine ABD; but as AB : CB :: F : G; hence as F : G :: sine CBD : sine ABD; consequently if F = G, DB bisects the angle ABC; which is assumed as an axiom by the French analysts.

Art. 11. Sine of angle BAD or of BCD = sine of ABC; therefore BD = $\frac{\text{BA} \times \text{sine ABC}}{\text{sine DBC}} = \frac{\text{CB} \times \text{sine ABC}}{\text{sine ABD}}$; hence

$K=\frac{F\times \text{sine } ABC}{\text{sine } DBC}=\frac{G\times \text{sine } ABC}{\text{sine } DBA}$. Moreover $AP=AB\times \cos. PAB$; and $CR=CB\times \cos. RCB$; hence $\cos. K=F\times PAB+G\times \cos. RCB$. Now as $\angle PAB+\angle RCB=\angle ABC$, it follows that if the directions of F and G converge to a point infinitely distant from A, C, the angle ABC will become evanescent, and the cosine of PAB become $=\cos. RCB=$ Radius $=$ Unity; hence in this case $K=F+G$; from which we easily infer that whatsoever supports a body acted upon by gravity, sustains the whole weight of it. This is also assumed as an axiom by some writers on mathematics. (vide Emerson.)

Art. 12. Let AB, CB be the directions of two forces F, G, fig. 1; and let DB denote the direction of their equivalent K; through any point L in the line BD, draw any right line ST meeting AB, CB in S, T; also draw LV any how meeting CB in V; then $LB\times$ sine $LBS=LS\times$ sine LSB; and $LB\times$ sine LBT $=LT\times$ sine $LTB=LV\times$ sine LVB; hence as sine LBT : sine LBS :: $LT\times$ sine LTB : $LS\times$ sine LSB; but as sine LBT : sine LBS :: G : F, Art. 11; therefore as $LS\times$ sine LSB : $LT\times$ sine LTB :: G : F; for the same reason as $LS\times$ sine LSB : $LV\times$ sine LVB :: G : F.

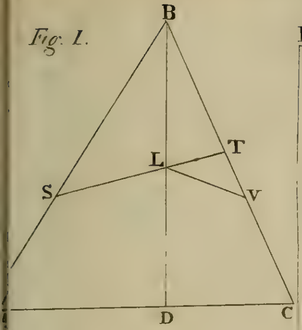
Art. 13. Suppose ST to be an inflexible right line, at the extremities of which S, T, the forces F and G act in the directions SB, TB; also, let ST be sustained at L by a force K acting in BD, and a statical equilibrium will be produced by Art. 6. Now let the lines of direction SB, TB converge to a point infinitely distant from ST; then will the angle SBT be evanescent; and we have seen the angles LSB, LTB, are supplemental to each other; as SL : LT :: G : F; which is a known property of the straight lever; and if SLV represent a crooked lever, the proportion stated above is universal, viz. as LS×sine LSB : LV×sine LVB :: G : F.

Art. 14. *Vide* fig. 3. Let AB, CB and DB be the direction of two forces F, G, and their equivalent K; from any point P, not in DB, draw PS, PT and PV perpendicular to AB, CB and DB respectively; then F×SP, G×TP and K×VP are called the momenta of F, G and K referred to the point or centre P. Now no force whatever applied to the point P, can keep the material point B in equilibrio, when acted upon as in the figure, by Art. 4; because the direction of such a force will either be parallel to DB the direction of K, or it will form an angle with it; therefore if PV be an inflexible line, capable of revolving about P, the force acting in the line DB perpendicular to PV, will cause it so to revolve.

Art. 15. Through PI draw P parallel to DB, meeting AB produced in I and make IK parallel to CB, meeting PT produced in R; also from D, draw DM, DN, perpendicular to AB, CB; then the triangles DMB, PSI are similar, as are also DNB, PRI; therefore as BD : IP :: DM : PS; and as BD : IP :: DN : PR; hence as DM : PS :: DN : PR; and multiplying by F and G we have, as $F\times DM : F\times PS :: G\times DN : G\times PR$; but $F\times DM=G\times DN$, by Art. 12; consequently $F\times PS=G\times PR$; therefore $F\times PS-G\times PT=G\times PR-G\times PT=G\times TR$; but by trigonometry $TR=IB\times$sine RIB or ABC; and $PV=IB\times$sine PIB or of DBA; hence as PV:RT :: sine∠ ABD : sine CBA; but as sine ABD : sine CBA :: G : K, by Art. 11; consequently as PV : RT :: G : K; and $PV\times K=RT\times G=PS\times F-PT\times G$; that is, the momentum of K=the difference of the momenta of F and G. Had P been situated out of the angle ABC, the momentum of K=the sum of the momenta of F and G.—If *a*B, *c*B, &c. be the direction of forces F′, G′, *d*B the direction of their equivalent K′, and *h*B the direction of L the equivalent of K and K′, draw P*s*, P*t*, P*v*, P*w*, perpendicular to Ba Bc, Bd and Bs; then by last Art. $K \times PV + K' \times Pv = L \times Pw$; but $K \times PV = F \times PS - G \times PT$; for the same reason $K' \times Pv = F' \times Ps + G' \times Pt$, therefore $L \times Pw = F \times PS - G \times PT + F' \times Ps + G' \times Pt$, &c.; that is, if any number of forces F, G, F′, G′ act in the directions BA, BC, Ba, Bc, &c. lying in the same plane, and Bh be the direction of their common equivalent L, the momentum of L referred to any point P=the sum of momenta of forces F′ and G′ without the angle of whose direction P is situated+the difference of momenta of F and G within the angle of whose directions the same point lies.

Plate. 2. page 390.

Fig. 1.



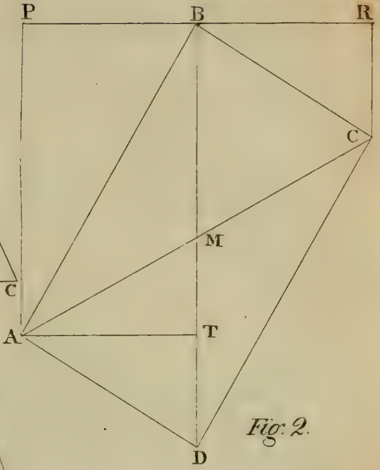
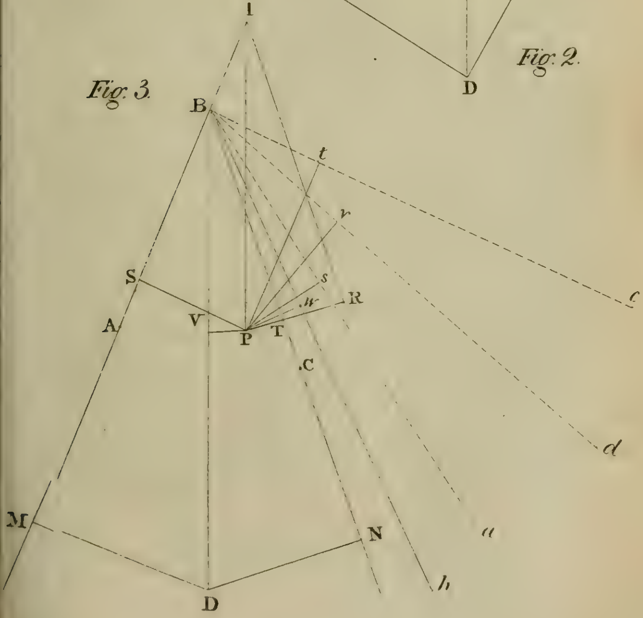


Fig. 2.

Fig. 3.



EXPERIMENTS

ON THE

GAS FROM COAL,

CHIEFLY

WITH A VIEW TO ITS PRACTICAL APPLICATION.

BY

WILLIAM HENRY, M.D. F. R.S. &c.

(Read March 19th, 1819.)

THE chemical properties and composition of the gas from coal formed a principal object of two different series of experiments, the results of which I laid before the public many years ago. The first of these communications, entitled "Experiments on the Gases obtained by the destructive Distillation of Wood, Peat, Pit-coal, Oil, Wax, &c. with a View to the Theory of their Combustion, when employed as Sources of artificial Light," appeared in Mr. Nicholson's Philosophical Journal for June 1805;* and the second memoir was published in the Transactions of the Royal Society, for 1808.

* 8vo. Series, Vol. xi. page 65.

By the first train of experiments, I endeavoured to derive, from a careful analysis of the compound combustible gases, a measure of their illuminating power, admitting of more exact appreciation, than the optical method of a comparison of shadows. The one, which I was led to propose as the most accurate, and which I still think entitled to preference, was the determination of the quantities of oxygen gas consumed, and of carbonic acid formed, by the combustion of equal measures of the different inflammable gases; that gas having the greatest illuminating power, which, in a given volume, condenses the largest quantity of oxygen. The average results of a great variety of experiments were comprized in the following table.

Kinds of gas.	Oxygen gas required to saturate 100 measures.	Carbonic acid produced.
Pure hydrogen	50	
Gas from moist charcoal	60	35
wood (oak)	54	33
dried peat	68	43
cannel coal	170	100
lamp-oil	190	124
wax	220	137
Olefiant gas	284	179

In the same essay, I maintained an opinion, which, on the most mature consideration, I see no reason to change; that the great variety of gases, evolved by the destructive distillation of inflammable substances, do not constitute so many distinct species, but are mixtures of a few, the nature and properties of which were before ascertained. It will contribute to render what follows more intelligible, if a brief account be given of those gases of known composition, the mixtures of which, in various proportions, compose, according to this view, all the observed varieties; and I shall make their comparison under a form best adapted to illustrate their practical application.

1. HYDROGEN GAS. This is the lightest of all known gases, its specific gravity, that of atmospheric air being taken at 1000, being about 73. As ordinarily procured, by the solution of iron or zinc in diluted sulphuric acid, it contains impurities which give it a disagreeable smell; but well purified hydrogen has little if any odour. It burns with a pale and feeble flame, not at all suited to artificial illumination.

		Product of its combustion.
	grains	grains
The cubic foot weighs about	40	
Consumes half a cubic foot of oxygen	300	
	340	Water...... 340

2. CARBURETTED HYDROGEN has been shewn to constitute the gas of marshes, and the fire-damp of coal mines. In these natural forms, it is contaminated with a small proportion of carbonic acid, and a larger one of azotic gas, but appears to be free from all other impurities. It is proved to be a definite compound of hydrogen and charcoal without any oxygen. It is lighter than common air, in the proportion of about 600 to 1000; it has very little odour; and burns with a flame greatly surpassing that of hydrogen, in density and illuminating power.

		Products.	
	oz. dr.*		oz. dr.
A cubic foot weighs......	0 . 12	1 cubic foot of carb. acid...	1 . 13
Consumes 2 cubic feet of oxygen.....	2 . 10	Water.........	1 . 9
	3 . 6		3 . 6

3. CARBONIC OXIDE is rather lighter than common air—It contains no hydrogen, and is purely a compound of charcoal and oxygen, the latter being in just half the proportion which is required to constitute carbonic acid. It burns with a feeble blue light.

* The avoirdupois ounce of 437½ grains, or 16 drams, is to be understood.

	oz. dr.	Product.	oz. dr.
A cubic foot weighs......	1 . 3		
Consumes ½ a cubic foot of oxygen......	0 . 11		
	1 . 14	Carbonic acid ...	1 . 14

4. Olefiant gas, or Bi-carburetted hydrogen. This has been demonstrated to be a compound of nearly 85 by weight charcoal, and 15 hydrogen, without any oxygen. It is a little lighter than common air, viz. in the proportion of about 974 to 1000. It surpasses all other gases in the brightness and density of its flame. Its name was originally derived from the property, which it possesses, of being speedily and entirely condensed, by rather more than an equal volume of chlorine gas, into a liquid resembling oil in appearance, but since shewn to approach more nearly to the nature of ether.

	oz. dr.	Products.	oz. dr.
A cubic foot weighs......	1 . 3	2 cubic feet carb. acid	3 . 10
Consumes 3 cub. feet of oxygen.....	4 . 0	Water	1 . 9
	5 . 3		5 . 3

Olefiant gas I found to be one of the products of the distillation of oil and of bees' wax, and was led, therefore, to suggest, that the wick of a lamp or candle, surrounded by flame, is to be considered as a bundle of ignited capillary tubes, into which the melted inflammable matter is drawn, and there resolved, not into a condensible vapour, but into olefiant and carburetted hydrogen gases. In the gas from coal, also, I detected the presence of olefiant gas, by the test of the action of chlorine.

In the second series of experiments,* I submitted to distillation, on a small scale, various kinds of coal, from different parts of the kingdom. The aëriform products, at different stages of the process, were kept apart, and were separately analyzed. From coal distilled in small iron tubes or retorts, which, when filled, were placed at once in a low red heat, small quantities of sulphuretted hydrogen and carbonic acid gases came over at first, in mixture with the other gases, but in a gradually diminishing proportion, till at length, in the last products, they were not discoverable at all. The production of olefiant gas observed the same order, and a

* Phil. Trans. 1808. page 282.

gradual diminution took place, as the process advanced, in the combustibility of the gas, as determined by its requiring less and less oxygen for saturation. A great variety was ascertained to exist in the quality of the gas from different kinds of coal; that from Wigan cannel, holding the highest rank in illuminating power, and that from the stone coal of South Wales, the lowest.

Since the period when the second of these papers was published, the use of artificial gases, as a source of light, has been rapidly increasing in this, and, I believe, in other countries, and promises to attain an extent and importance, sufficient to justify any labour that may tend, however remotely, to its improved application. It has frequently happened, of late years, that I have been requested by the proprietors of large manufactories lighted by gas in this neighbourhood, to give an opinion on practical points, respecting some of which I felt myself incompetent to decide, from the want of the necessary data. It is to supply these data, that I have once more returned to the investigation of the subject. The objects, which I have had it in view to determine by the following course of experiments, are, whether, on the large scale of manufacture, there is a decline in the value of the aëriform products of coal, from the beginning to the end of the distillation, similar to that which takes place on a small scale;—at what stages of the process, those gases, which may be considered as impurities, are chiefly evolved; and whether they are essential or accidental products;—whether the method of removing the sulphuretted hydrogen, and carbonic acid gases by quicklime, which I suggested in the second memoir, is adequate to the complete purification of coal gas;—whether this purification is attended with any loss of that portion of the gas which, on account of its superior illuminating power, it is desirable not to remove;—and, if such a loss should be found to ensue, whether it may not be avoided by some modification of the purifying process. In determining these points, I was indebted for the necessary supplies of gas to Mr. Lee, at whose extensive manufactory the principal facts were ascertained, that formed the basis of the first accurate calculations, respecting the economy of gas from coal.*

* See Mr. Murdoch's "Account of the Application of the Gas from Coal to economical Purposes." Phil. Trans. 1808, page 124.

On the Quality of the Gas, at different stages of the Distillation.

The gas, which I first submitted to experiment, was obtained from Wigan cannel coal, a substance preferred in this neighbourhood as affording aëriform products, which, both by their quantity and quality, more than compensate its higher price.* The retorts are charged while red hot with this substance, and indeed are never suffered, during the whole of the winter season, to fall below the temperature of ignition. The gas was collected in a bladder furnished with a stop-cock, which was fixed into an opening in the pipe between the retort and the tar-pit. It was taken at this place, in order to avoid contact with water, and admixture with any atmospherical air, that might accidentally remain in the gazometer. Wishing to examine the gas in a perfectly recent state, and finding it impossible to make the necessary experiments with sufficient accuracy in a shorter interval, I was obliged to be satisfied with procuring it every other hour. In this place,

* About a shilling per cwt. of 112lb. or 13½d. delivered in Manchester.

I shall only state the general results, and I shall describe, in a subsequent part of the paper, the methods of analysis, in order that other persons, who may choose to compare my experiments with their own, may conduct them under equal circumstances.

By the expression *impure gas,* is to be understood, the gas precisely in the state in which it was collected from the retort; and by *purified gas,* the same product after being freed from carbonic acid and sulphuretted hydrogen by solution of pure potash, applied in very small quantity, relatively to the volume of the gas, and with the least agitation adequate to the effect.

TABLE I.

Shewing the Quality of Gas from 1120*lb. of Cannel, at different periods of the Distillation.*

Hours from the commencement.	100 measures of impure gas contain of		100 m. purified gas consist of			100 m. of purified gas	
	Sul. hyd.	carb. ac.	Olef.	other infl. gases	az.	cons. oxyg.	give carb. acid.
½ an hour...	0½	5½	16	64	20	180	94
1 hour.......	3	3½	18	77¼	4¾	210	112
3 hours......	2½	2½	15	80	5	200	108
5 hours......	2½	2½	13	72	15	176	94
7 hours......	2	2½	9	76	15	170	83
9 hours......	0½	2½	8	77	15	150	73
10½ hours......	0	2	6	74	20	120	54
12 hours......	0	0½	4	76	20	82	36

Excluding from the calculation the azotic gas, with various proportions of which the products were contaminated, the following table shews the quantity of oxygen gas consumed, and of carbonic acid produced, by the really combustible part of the gas.

TABLE II.

Shewing the Quality of the really combustible part of the Gas, at different periods of Distillation.

	Take oxygen.	Give carb. acid.
100 measures of half hour's gas	225	118
1 hour's gas	220	117
3	210	114
5	206	108
7	200	98
9	176	85
10½	150	70
12	103	45

The next set of experiments was made on gas from common coal, got at Clifton, near Manchester, and of fair average quality.

TABLE III.

Shewing the Quality of the Gas from 1120*lb of Common Coal, at different periods of the Distillation.*

	100 m. of impure gas contain: sulp. hyd.	100 m. of impure gas contain: carb. acid.	100 measures of purified gas: olef.	100 measures of purified gas: other infl. gases	100 measures of purified gas: az.	100 measures purified: cons. oxy.	100 measures purified: give carb. acid.
1 hour's gas	3	3	10	90	0	164	91
3 hours do.	2	2	9	91	0	168	93
5 hours do.	3	2	6	94	0	132	70
7 hours do.	1	3	5	80	15	120	64
9 hours do.	1	$2\frac{1}{2}$	2	89	9	112	60
11 hours do.	1	1	0	85	15	90	43

Exclusive of the azote, with which the three last portions of gas were mingled, they consumed oxygen and gave carbonic acid as follows. The seven hours gas in this instance, as sometimes happens from irregularities of temperature, was more combustible than that collected two hours sooner.

	Consumed oxygen.	Gave carb. acid.
100 m. of 7 hours gas	140	75
9 hours	123	66
11 hours	106	50

A comparison of the results exhibited in the third table, with those of the distillation of cannel coal, is greatly in favour of the latter substance as a source of light. This will appear most distinctly, by setting against each other the proportions of oxygen, which are consumed by the gases evolved from the two substances at equal times from the commencement.

TABLE IV.

Comparative Table of the Qualities of the Gases from Wigan Cannel, and from common Coal, at equal times from the Commencement of the Distillation.

	Oxygen consumed by 100 m. cannel gas.	Oxygen consumed by 100 m. of Clifton coal gas.
1 hour's gas	220	164
3 hours	210	168
5 hours	206	132
7 hours	200	140
9 hours	176	123
11 hours	150	106

It appears from these experiments, that the gas from cannel has, in an equal volume, an illuminating power about one third greater than that from coal of medium quality. The quantity, also, from the former substance, exceeded by about one seventh that obtained from coal, distilled under precisely similar circumstances; 3500 cubic feet of gas having been collected from 1120 pounds of cannel, and only 3000 cubic feet from the same quantity of coal. The whole product of one distillation of cannel, mixed together in a gazometer, was of such quality, that 100 measures required for combustion 155 measures of oxygen gas, and gave 88 measures of carbonic acid. But as the gas was contaminated with 15 measures of azote in every hundred, the oxygen, required for saturating 100 measures of the really combustible part of it, may be stated at 195; and the carbonic acid produced at 110. It may be necessary to observe, that in comparing the value of gases produced from different kinds of coal, or from the same kind of coal differently treated, it is not enough to determine the *quantity* of aëriform products; and no satisfactory conclusion can be drawn respecting the relative fitness of any variety of coal for affording gas, or the advantages of different modes of distillation, unless the *degrees of combustibility* of the gases compared be determined, by finding experimentally the proportion of oxygen gas required for their saturation.

The results expressed in the first table, when contrasted with those which I formerly obtained by the destructive distillation of small quantities of coal, present several circumstances of disagreement, as to the quality of the products at different stages of the operation. In small experiments, the sulphuretted hydrogen and carbonic acid gases were evolved only at the early stages of the process; and sulphuretted hydrogen, especially, could not by the nicest tests be discovered in the last products of gas. On the large scale, both these gases continue to be evolved throughout the whole operation, though in greatly diminished proportion towards the latter end. Even in the advanced stages of large distillations, the presence of sulphuretted hydrogen in coal gas may be traced by the proper test, though not in a quantity that admits of being easily measured. The test, which I used for some time, was the white oxide of bismuth, for which I afterwards substituted white lead, ground with a little water to the proper consistence, and spread by a camel's hair pencil on a slip of card. This was secured by a small pair of forceps fixed in a cork, by means of which, the slip of card could be placed in a jar or bottle of the gas, and kept there for some time. By experiments on artificial mixtures, I found that a cubic inch of sulphuretted hydrogen, diffused through twenty thousand cubic inches of common air, distinctly affected the test, which it changed to a light yellowish or straw colour. By mixing sulphuretted hydrogen with various proportions of common air, I prepared coloured cards of a variety of shades, which served as standards of comparison for judging of proportions of sulphuretted hydrogen in coal gas, which were too minute to be accurately measured.

In the small experiments made several years ago, I never found, in the early products of gas from cannel coal, a proportion of olefiant gas at all approaching that which is noted in Table I, and its quantity in small distillations rapidly decreased, until in the latter products it could be no longer traced at all. The method of analysis, which I formerly employed, led me, however, as I have lately discovered, to under-rate the proportion of olefiant gas, and to over-estimate that of sulphuretted hydrogen. But making due allowance for this error, the superiority of the products of large operations, so far as respects olefiant gas, still exists, and is confirmed by comparative experiments on a small scale which I have lately made. Thus it appears from table I, that even after twelve hours continuance of the process, olefiant gas still constitutes 4 per cent. of the gases evolved from cannel. The other inflammable gases, also, when obtained in large quantity, are more uniform in quality, and possess, towards the close of the process, much greater combustibility and illuminating power, than when procured in small experiments. This superiority is obviously dependent on the greater facility of preserving an uniform temperature, in all chemical processes which are carried on upon a scale of magnitude.

The temperature to which the coal is subjected, must necessarily be a point of the greatest importance to the quantity and quality of the aëriform products; for while too low a heat distils over, in the form of a condensible fluid, the bituminous part of the coal which ought to afford gas, too high a temperature, on the contrary, occasions the production of a large relative proportion of the lighter and less combustible gases. It would be a great step in the improvement of the manufacture of coal gas, if the whole of the hydrogen could be obtained in combination with that proportion of charcoal which constitutes olefiant gas; and it is satisfactory to know, that no impediment to this arises out of the proportion of the hydrogen and charcoal present in coal. If this object be ever accomplished, it will probably be by the discovery of means of uniformly supporting such a temperature, as shall be adequate to the production of olefiant gas, and shall never rise above it; and some probability of success is perhaps derivable from the fact, that M. Berthollet, by the careful decomposition of oil, which in my experiments afforded a mixture of gases, succeeded in obtaining olefiant gas in a state of purity.*

With the view of ascertaining how low a degree of heat is adequate to the production of gas from coal, I placed a small iron retort, containing cannel, in melted solders of various composition, without obtaining more than the common air of the vessel. The retort, charged with fresh materials, was then immersed in melted lead, but after expelling the common air, no more than a few bubbles of gas came over, and that only when the lead, by being kept over the fire, had acquired a temperature about its fusing point. On restoring this temperature by adding fresh metal, the evolution of gas was always suspended. I placed also, one of Mr. Wedgewood's hydrometer pieces in contact with a retort which was at work at Mr. Lee's manufactory, and which shewed only a dull red or blood coloured heat; but, after remaining in that situation half an hour, a contraction of barely one degree of the scale had taken place. This temperature, however, I suspect is rather too low, and has a tendency to distil over too much tar, and consequently to

* Mémoires de la Soc. d'Arcueil, ii. 84.

produce less gas than might be obtained by a degree of heat somewhat higher. The best adapted temperature will probably be found to vary with different kinds of coal; and I have been prevented from ascertaining it with respect to cannel, by the inconveniences that would arise from disturbing the regular arrangements of a large manufactory. From some experiments of Mr. Brande, it appears that the sudden application of the requisite heat evolves from coal much more gas, than the gradual heating of a cool retort up to the point of ignition.*

In the experiments upon gas from Wigan cannel, the results of which are comprized in the first table, azotic gas was found in all the aëriform products, from the beginning to the end of the operation. But in experiments on the gas, obtained at other times from the same substance, no appreciable quantity of azotic gas could be discovered till after the sixth hour of the process, when it began to appear, and progressively rose to 20 parts in the hundred. Of this purity of the early products from azote, and appearance of it in the latter ones, Mr. Dalton was an eye witness on one occasion, when he was so

* Journal of Science Vol. 1. page 75.

good as to co-operate with me; and I had afterwards repeated opportunities of verifying the fact. With the view of ascertaining whether the azote found its way from the atmosphere into the distilling vessels, I subjected 100 grains of cannel coal to heat in a glass retort, the capacity of whose body and neck did not together exceed $1\frac{1}{4}$ cubic inch. Besides a portion of gas which was lost, 50 cubic inches were collected, which, on careful analysis, were found to contain 5 cubic inches of azotic gas. Of these only one cubic inch can be traced to the common air present in the retort at the outset; and the other 4 cubic inches must have been furnished by the coal itself.

It is reasonable indeed to expect, that a substance like coal, which affords ammonia under some circumstances, should, under others, yield the elements of that alkali in a detached state; and the reason, why azote is for the most part not to be found in the gas which is first evolved, is, that at a low temperature, that element unites with hydrogen, and composes ammonia. But when the contents of the retort, which, for some time, have been kept comparatively cool by the escape of condensible fluids, become more intensely heated, ammonia is either not formed, or, if formed, is decomposed again into azotic and hydrogen gases, both of which may be traced in the aëriform products of the advanced stages of distillation. As a matter of practice, it is certainly desirable that the azote existing in coal should enter into the composition of a condensible fluid, rather than that it should escape in a gaseous state; for it is an impurity which, when once mingled with the combustible gas, cannot be removed by any known method, and must materially impair its illuminating power. That such an effect must result from its presence, may be inferred from the experiments of Sir H. Davy, who found that an explosive mixture of carburetted hydrogen and common air was deprived of its combustibility by being mixed with one sixth of its bulk of azotic gas.*

On the Purification of Coal Gas.

The chief impurities mingled with the gas from coal, which it is desirable and practicable to remove before applying it to use, are carbonic acid and sulphuretted hydrogen gases. The former is of little importance; but the latter imparts to the coal gas, when

* On the Safety Lamp, page 30.

unburned a very offensive smell, resembling that of bilge water, or the washings of a gun-barrel, and the inconvenient property of tarnishing silver plate; and during combustion, gives rise to the same suffocating fumes (sulphurous acid) which are produced by the burning of a brimstone match. The most obvious method of absorbing both the carbonic acid and the sulphuretted hydrogen, is to bring the recent gas into contact with quicklime; and the cheapness of that substance, and facility of applying it, led me, several years ago, to propose it for the purpose.* It has since, I believe, been suggested that the sulphuretted hydrogen may be removed by chlorine; but a sufficient objection to this agent is, that it would also separate the most valuable part of the product, the olefiant gas. The transmission of the gas through ignited tubes has also been proposed; but it is a well known property of both the varieties of carburetted hydrogen, that they deposit charcoal when strongly heated; and M. Berthollet has shewn that the amount of this effect is proportionate to the increase of temperature†. Some persons, practically engaged in lighting

* Phil. Trans. 1808. page 303.

† Mem. de la Soc. d'Arcueil. iii. 154.

with gas have, to my knowledge, been led, by the increase of the quantity of gas which is obtained by passing it through red hot tubes, to imagine, that an advantage is thus gained; and they have not been aware, that the gas, when thus treated, sustains a much more than proportional loss of illuminating power.

The quantity of quicklime required for the absorption of a cubic foot of carbonic acid, or of the same volume of sulphuretted hydrogen gas, will be found on calculation not to exceed 1050 grains, or about $2\frac{1}{2}$ ounces avoird. A volume of coal gas containing a cubic foot of each of those impurities will require, therefore, at least 5 ounces of lime applied in the best possible manner. But it is never found in practice that the whole of any gas, when sparingly diffused through another, can be taken out entirely, without using much more of the appropriate agent than, from its known powers of saturation, might have been deemed equivalent to the effect. The proportion employed by Mr. Lee is five pounds of fresh burned lime to 200 cubic feet of gas. The lime, after the addition of the quantity of water necessary to reduce it into powder, is passed through a sieve, and then mixed with a cubic foot (about $7\frac{1}{2}$ wine gallons) of water. This is found to be enough to purify the gas sufficiently for ordinary purposes; but it still retains a minute proportion of sulphuretted hydrogen, which, from the shade of colour produced in the test, may be estimated at about one ten thousandth of its volume. For some purposes, the same gas is therefore washed a second time with a similar proportion of fresh lime, which, without being removed from the cistern, is again employed to give the first washing to another quantity of fresh gas. After the second purification, the gas produces no change whatever in the test, which preserves its perfect whiteness, thereby demonstrating the complete removal of the sulphuretted hydrogen. In this state of purity, its odour, also, is so much diminished, as scarcely to be at all offensive.

In order to ascertain whether any, and what portion of olefiant or carburetted hydrogen gas is lost by the action of the lime liquor, I compared, with the greatest care, the products of the combustion of the recently prepared gas, and of the same gas after one and two washings with lime and water.

	Consumed oxygen.	Gave carb. acid.
100 measures of the unwashed gas	190	108
Gas once washed	175	100
Twice washed	175	100

The frequent repetition of similar experiments fully satisfied me that the fresh prepared gas from coal does in fact sustain, by agitation with lime liquor, a loss of combustible matter, amounting to about 8 or 10 per cent.; but that the second washing is not attended with any farther appreciable loss. I found, also, that the recent gas, by being kept a fortnight in bottles completely filled with it, and well stopped so as to exclude all agency of the water in which they were inverted, was diminished in combustible matter about half the foregoing amount. On the other hand, gas which had been washed with lime liquor, suffered no change when kept under like circumstances, for an equal time. It is probable, therefore, that what is separated from the unwashed gas, whether by keeping or by the action of lime liquor, is chiefly condensible matter, partly perhaps an etherial oil and partly a substance which it is desirable to remove, rather than to allow it to be deposited in a solid form, in the small pipes, or in the burners.

The little effect of the lime liquor on the olefiant gas, which I had not anticipated, admits however of being satisfactorily explained on known principles. Water and similar fluids absorb, according to Dalton, about $\frac{1}{8}$th, according to Saussure about $\frac{1}{7}$th, of their volume of olefiant gas. The utmost quantity, therefore, which a cubic foot of lime liquor, acting upon pure olefiant gas, could absorb, would be $\frac{1}{7}$th of a cubic foot. But agreeably to a law discovered by Mr. Dalton, and explained and confirmed by my own experiments,* a cubic foot of lime liquor, when brought into contact with 36 cubic feet of olefiant gas mixed with 164 cubic feet of other gases, can absorb only about one fifth of one seventh, or $\frac{1}{35}$th, of a cubic foot of olefiant gas. This quantity, which does not exceed $\frac{1}{1260}$th part of the olefiant gas present in 200 cubic feet of the best coal gas, is too trifling a loss to be discoverable by experiment, or to be worthy of being regarded in practice, even when doubled by a second washing. It is therefore consistent with general reasoning, as well as with experiment, that the washing of coal gas with a due proportion of lime liquor should entirely

* Nicholson's Journal, 8vo. vii. 297. and Thomson's Annals, vii. 214.

remove the sulphuretted hydrogen gas and other offensive ingredients, without abstracting an appreciable quantity of either of the carburetted hydrogen gases. It is nevertheless important that the quantity of water, employed in washing the gas, should not be increased beyond what is necessary to give the mixture due fluidity, because, under equal circumstances, the power of water to absorb a gas is in direct proportion to the quantity employed.

Such are the principal circumstances, that occurred to me as requiring to be investigated, and to be at the same time capable of affording results, that may admit of general application, wherever coal gas is employed as a source of light. There are others of more limited utility, that may be left to be determined by those persons, who are interested respecting them; such as the preference due to different varieties of coal as sources of gas, and sometimes even to other inflammable substances, which, on account of local situation, may be entitled to preference over coal. The facts which have been stated supply, also, data for deciding other questions, which may be suggested by circumstances of partial interest;—for example, whether it may not be adviseable, in some cases, to collect only the first portions of gas; or, if all be collected, to reserve different portions apart from each other, and to apply them to appropriate uses. Thus, when coal gas is conveyed in portable gazometers to a distance, (as is now practised by Mr. Lee in supplying his house two miles from the manufactory*) it will be important to select that gas, which in a given volume has the highest illuminating power, and which therefore requires vessels of the smallest capacity for its conveyance. Having, I hope, furnished documents for solving questions of this sort, I shall proceed to describe in what manner the facts were ascertained.

Method of Analysis.

1. *Determination of the proportions of carbonic acid and sulphuretted hydrogen gases in coal gas.* In experiments formerly made

* A small carriage upon springs conveys two square close gazometers made of wrought iron plates, and each containing 50 cubic feet of perfectly purified gas, equivalent together to about 6lb. of tallow. Each gazometer weighs about 160 pounds; and has a valve at the bottom, which is opened by the upright main pipe, the moment the gazometer is immersed in the pit. The strength of one man is found to be sufficient for the labour of removing the gazometer from the carriage to its place.

on this subject, I employed the agency of chlorine to condense both these impurities, and estimated how much of the absorption was due to each, by a rule which I have stated.* Recent experience, however, has led me to distrust this method; and after comparing the effects of several other agents, by experiments on mixtures of known composition, I now prefer the white carbonate of lead, precipitated from acetate of lead by carbonate of ammonia without heat, and therefore fully saturated with carbonic acid. This precipitate it is better not to dry, but, after washing it sufficiently, to leave it under as much water as will give it, when wanted for use, a due degree of fluidity. This mixture may be applied by means of a tube of the capacity of a cubic inch, divided into 100 equal parts, and accurately ground into a short and wider piece of tube, which ought not to contain more than three or four tenths of that quantity. The wider tube being filled with the fluid carbonate of lead, and placed with its mouth upwards under water, the graduated measure full of gas is fitted to it; and the gas and liquid are brought into contact by alternately inverting the two tubes, all violent agitation

* Phil Trans. 1808. page 295.

being carefully avoided. The sulphuretted hydrogen is thus absorbed, and the carbonic acid, being left untouched, is afterwards taken out from the same portion of gas by a similar use of solution of pure potash.

2. *To ascertain the proportion of olefiant gas in the residue left by potash.* From 25 to 30 hundredths of a cubic inch of chlorine gas are passed into a tube of the diameter of about $\frac{3}{10}$th of an inch, accurately divided into hundredths of a cubic inch; and the volume of the chlorine is noted when actually in the tube, to avoid errors from its absorption in rising through the water. To this is admitted half a cubic inch (equivalent to 50 measures) of the gas under examination, and the mixture is left, excluded from the direct light of the sun and perfectly quiescent, for fifteen minutes. At the expiration of this time, the remainder is noted, and the diminution which has taken place being divided by 2, the quotient shews the quantity of olefiant gas in fifty measures of the mixture. This process, I am aware however, does not give results of perfect accuracy, for, in addition to other sources of fallacy, I find that chlorine begins to act on carburetted hydrogen much sooner than is generally

supposed,* though within the period mentioned, and in such narrow tubes, it does not occasion a sensible diminution of bulk. The method described may, therefore, be considered as affording a tolerably near approximation to the proportion of olefiant gas; and as all the varieties of coal gas were subjected to the test under precisely the same circumstances, the errors must have been of nearly the same amount in all cases, and cannot materially interfere with the fair comparison of the different specimens of coal gas, so far as respects their proportion of olefiant gas.

3. *To ascertain the quantity of combustible matter* in gas which had been deprived only of sulphuretted hydrogen and carbonic acid, a mixture of the gas with a due proportion of oxygen gas was fired by the electric spark over mercury. This method I preferred to slow combustion, carried on with the apparatus, which I have described in the Philosophical Transactions for 1808, solely because, when a great number of experiments are necessary, as in this enquiry, the method of

* While this sheet was passing through the press, I have noticed a passage in Mr. Brande's Manual of Chemistry (page 156 n.), from which it appears that the speedy action of chlorine on carburetted hydrogen had bee observed by Mr. Faraday.

detonation is attended with a great saving of time. But on all occasions where only few experiments are required on gases of great combustibility, I prefer slow combustion, both on account of greater safety to the apparatus, and, from the quantities that may be consumed, of greater accuracy also. When rapid combustion is practised, I believe that, on the whole, more accurate results are gained by firing the gas at one operation properly conducted, than at two. The latter method seems to have been preferred by M. Berthollet, but so far as my experience goes, it is more apt to precipitate charcoal from the gas.

To burn each measure of the early and more combustible products of gas, I employed from 3 to 4 measures or upwards, of oxygen gas, the degree of purity of which had been ascertained. The volume being noted after firing, and again after agitating the residue with liquid potash, the last diminution shewed the quantity of carbonic acid. The gas left by potash was next analyzed by combustion with a due proportion of pure hydrogen,* which shewed how much of the residue was oxygen, and how much azotic gas. If more

* The method of doing this is given in my Elements of Chemistry. Vol. I. chap. v. Sect. vi.

azote was found, than had been introduced as an impurity of the oxygen gas, it was considered as having formed a part of the combustible gas. A single experiment on any kind of gas was never relied upon; and to insure accurate results, the same gas was fired with different proportions of oxygen. Deducting the pure oxygen found in the residue, from its quantity at the outset, the volume of oxygen gas was learned, which had been spent in saturating a given measure of combustible gas.

In gases free from all admixture with carbonic oxide, it is easy to know how much of the oxygen consumed has been spent in saturating the charcoal; for as oxygen gas by conversion into carbonic acid suffers no change of volume, the quantity which has combined with the charcoal is exactly represented by the volume of carbonic acid produced by the combustion. For example, as 100 measures of olefiant gas afford by detonation 200 of carbonic acid, 200 measures of oxygen must have united with the charcoal of the olefiant gas. But beside these 200 measures, an additional 100 measures of oxygen are found to be consumed, and these must have combined with hydrogen, the other ingredient of the gas, the volume of which in its full state of expansion would be 200 measures, as determined by the fact, that oxygen gas uniformly takes for saturation double its volume of hydrogen gas, and no other proportion.

Nature of the Gas from Coal.

The opinion which I formerly advanced on this subject,* though opposed by writers of so much authority as M. Berthollet and Dr. Murray, still appears to me to be much more probable, than that the varieties of gas from inflammable substances, which may be almost infinitely diversified by modifications of temperature, are, as those philosophers suppose, so many distinct compounds of hydrogen and charcoal, or of hydrogen and charcoal in combination with oxygen. The reasons that induce me to abide by my original view of the subject are the following:

1. We are acquainted with two distinct and well characterized compounds of hydrogen and charcoal, in one of which a given weight of charcoal is united with a certain quantity of hydrogen, and in the other with double that quantity. Besides these two, no

* Nicholson's Journal. 8vo. xi. 68.

other compound of those two elements has been hitherto proved to exist.

2dly. It is inconsistent with experience, that two bodies which, like hydrogen and charcoal, unite by an energetic affinity, should combine in all possible proportions. On the contrary, it is to be expected from analogy in general, and from that of the compounds of charcoal and oxygen in particular, that hydrogen and charcoal unite in few proportions only, and in such a manner that these proportions are multiples or divisors of each other by some entire number.

3dly. All the phenomena may be satisfactorily explained by supposing the gas from coal, and from other inflammable substances, to be mixtures of this kind. For example, referring to the one hour's gas in the first table, we shall find that it contains, in 100 measures, 18 of olefiant gas, which require for combustion 54 measures of oxygen, and afford 36 of carbonic acid. The same gas contains also $77\frac{1}{4}$ measures of another inflammable gas, in the combustion of which $210-54=156$ measures of oxygen have been spent, and which have afforded $112-36=76$ measures of carbonic acid. This is as near an approach as can be expec.ed to the properties of carburetted hydrogen, the $77\frac{1}{4}$

measures having consumed very nearly twice their bulk of oxygen, and given an equal volume of carbonic acid. We may, therefore, consider the early products of the gas from cannel as a mixture of about one volume of olefiant gas and four volumes of carburetted hydrogen.*

The early product of gas from Clifton coal does not admit of being thus theoretically resolved into a mixture of olefiant and carburetted hydrogen gases only. For after deducting from the oxygen consumed (164 measures) that spent in saturating the olefiant gas ($10 \times 3 = 30$) we have only 134 measures of oxygen left for the combustion of 90 measures of inflammable gas. These 90 measures, it appears, afford $91 - 20 = 71$ measures of carbonic acid. This portion of the gas does not, therefore, answer to the characters of carburetted hydrogen, since it neither gives an equal volume of carbonic acid, nor con-

* I am perfectly aware of the importance of taking the specific gravity of mixed gases, as one datum for determining their proportion in any mixture; but I was prevented from ascertaining it in these experiments by the state of the necessary apparatus, which was found, from long disuse, to have become unfit for the purpose. So far as respects the practical objects of this paper, the omission is of no consequence.

sumes a double volume of oxygen. In this case and a variety of similar ones, we can only at present explain the phenomena, by comparing them with hypothetical mixtures of the different known gases. As an example, I shall describe the particulars of the combustion of the first product of Clifton coal, and endeavour to explain the results in the manner which has been suggested.

Measures of the gas......11
Mixed with oxygen........39=37 pure oxygen+2 azote.

Total...... 50
Volume after firing.........31
do. after washing by potash 21=19 oxygen+2 azote.

18 oxygen consumed.

In this case, the diminution by firing is 19 measures; that by potash, which denotes the carbonic acid, 10 measures; and the gases consumed are 11+18=29. Let us examine what mixture of gases will account for the appearances.

m. of infl. Gas.		Take Oxyg.	Give Carb. Acid.	Diminution by firing.
1 . 1	Olefiant	3 . 3	2 . 2	2 . 2
7 .	Carb. hydr.	14	7 .	14 .
1 .	Carb. oxide	0 . 5	0 . 5	1 .
2 .	Hydrogen	1 .		3 .
11 . 1		18 . 8	9 . 7	20 . 2

The sums of the numbers thus theoretically obtained do not, it is true, exactly correspond with the experimental ones; but they approach as nearly as, from the nature of the subject, can be expected, the greatest disagreement (that in the diminution by firing) not much exceeding $\frac{1}{20}$th of the observed amount.

In a similar manner we may explain the composition of the lighter and less combustible products, obtained at advanced periods of the distillation. For example, a portion of the last product of gas from cannel, distilled in a glass retort, gave the following results.

Measures of gas.........20
Mixed with oxygen......30=28 pure oxygen+2 azote.

Total....... 50
Fired......................22
Washed with potash.....18=14 . 7 oxygen+3 . 3 azote.

13 . 3 oxygen spent.

In this experiment, 1 . 3 more azote were found in the residuum, than can be traced to the oxygen employed. The combustible gas was, therefore, only $18\frac{2}{3}$ measures; the carbonic acid produced 4; the oxygen spent 13 . 3; and the diminution by firing 28. The following supposed mixture will explain these facts.

Measures of	Take Oxyg.	Give Carb. Acid.	Diminished by firing.
2 Carb. hydr.	4	2	4
2 Carb. oxide	2	2	2
15 Hydrogen	$7\frac{1}{2}$		$22\frac{1}{2}$
19	$13\frac{1}{2}$	4	$28\frac{1}{2}$

In this instance the hypothetical constitution coincides even more nearly with the facts, than in the former case. It must indeed, be acknowledged that the explanation rests on hypothesis only; but it is on an hypothesis, which is perfectly consistent with a copious and increasing induction of facts, all tending to establish a limitation to the proportions in which bodies combine; while the opposite explanation is at variance with this general law of chemical union.

AN INQUIRY

INTO THE

EFFECTS PRODUCED UPON SOCIETY

BY THE

POOR LAWS.

BY JOHN KENNEDY, ESQ.

(Read March 5, 1819.)

THE various discussions which have taken place respecting the policy of the poor laws, and the contrariety of opinion that still exists on this important subject, have induced me to devote some attention to its investigation; and to endeavour to determine, from a comparison of the state of the poor in this and other countries, whether it is not for the advantage of the community at large that laws of this kind should continue to be in force.

In explaining the view of the question which I have been led to take, I think it unnecessary to say much respecting the origin of the poor laws; but I shall endeavour to shew that a provision for the poor does virtually, though perhaps not in the shape of formal enactments, exist in every civilized country; and I shall attempt to explain the influence of the various expedients, which are in use for supporting the poor in different countries, so far as these are calculated to promote, or to retard, the industry and activity of a people.

In prosecuting the inquiry, I shall assume that division of the inhabitants of a civilized country which is now generally adopted, viz. into the higher, the middle, and the lower classes; and it may contribute to perspicuity if we suppose them to be represented by a scale, resembling that employed to indicate the degrees of heat and cold. In a prosperous country, I believe we shall find that these three classes intimately sympathize with each other; so that supposing the lower order to advance a certain number of degrees in the enjoyments and comforts of life, the middle and higher classes will make a similar ascent, each preserving its original relative distance from the others, and *vice versa*.

Let us now examine how far this imaginary scale will assist us in our observations connected with the subject in these united kingdoms. In Ireland we shall suppose the lower class to be indicated by 32° of the above scale, the middle by 98°, and the higher class by 160° ; in Scotland the lower by 45°, the middle by 110°, and the higher by 180 ; and in England the lower by 65°, the middle by 130°, and the higher class by 200°.

In Ireland, I believe, there are no poor laws. In Scotland those laws exist nearly as in England, though they are rarely acted upon. But the poor, in every country, must be supported by some means, when want or misery, from whatever cause arising, reduces them below that degree, which indicates their natural situation upon the scale, relatively to the class above them.

Of the earliest establishment of the poor laws in England, we have but a very uncertain account, and it is difficult to determine, with any degree of accuracy, the exact period of time, at which they were first introduced. In all probability it was not much later than the decline of the feudal and monastic establishments; and we may conclude that consequences then ensued, in England, similar to those which took place in Scotland, at a later period, under like circumstances. It is well known that, in Scotland, great misery was produced when the lower orders were cast off by their chiefs, and deprived of the means of support, to which their state of vassalage had given them a right almost equal, in their estimation, to that of the Barons themselves. England, also, as would appear from the various enactments, made at that time to suppress mendicity, must have been overrun with beggars and free-booters, to such a degree as to render it absolutely necessary to bring those vagrants under controul. This could only be effected by either finding them employment, or subsistence; and arrangements were accordingly made, by which the poor became less dependent, and civilization received a considerable stimulus. The funds for their maintenance, operating in some degree as a capital for their use, enabled the poor to keep pace with the classes above them, in acquiring an increase of comforts and enjoyments. In Scotland and in Ireland, no steps seem to have been taken to provide for the cast-off vassals, with this exception, in regard to Scotland, that the means of education were there established by law. In those two countries, the consequence of the want of all provision for the poor was, that when thus abandoned by their lords, they were left in a state of deplorable dejection and misery; and to this want of a legalized system of relief, are we to ascribe the inferiority of the lower orders in Scotland and Ireland to those of the same rank in England, so far as respects the enjoyment of the comforts and conveniences of life.

Under the worst circumstances that can befal the English poor, they have still a parish to look to for support, and they seldom think of exchanging the mode of relief which is thus afforded, for the precarious subsistence of vagrant begging. Parochial relief is a certain provision, which, *when well administered*, becomes a stimulus to a degree of industry in the lower classes, which we never find in countries destitute of such regulations. The happy effects of this incitement on their minds are, so far as I have observed, apparent in preventing their spirit of activity from drooping or being broken down. But whenever the lower orders are reduced, as in other countries, to the extreme limit at which existence can be maintained, then, if unsupported, their energy entirely fails them; degradation, both moral and physical, immediately succeeds; and the prospect of restoring them to industry and activity becomes almost hopeless. The evil, however, does not rest with the debasement of the lower class; for this class may not unaptly be compared to an indispensible part of a moving piece of mechanism, of which, if the form or situation be altered, the whole machine is deranged. Exactly the same thing happens with respect to the different classes constituting a nation; for when the lower orders have, by the inefficiency of their exertions, lost all incitement to activity, the progress of general improvement receives an immediate check, which it may require centuries to counteract.

One great evil of throwing the poor upon the public as mendicants, is, that all controul over them ceases. For begging cannot be successfully prosecuted within the parish to which they belong, and therefore their first care is to quit their native place for another where they are not known. Thus is extinguished all that wholesome restraint, which arises from being subject to the observation of superiors, whom they have been accustomed to respect; and they indulge, among strangers, a profligacy of habits and conduct, from which an honorable shame and regard to character would have preserved them, if they had remained at home. Unfortunately, too, a vagrant life, to persons who possess few or no domestic comforts, is generally very fascinating, and it is found almost impossible to reclaim those, who are addicted to it, to habits of regular and stationary industry.

Among the lower orders, it may, I think, be safely affirmed, that industry can only be found, where artificial wants have crept in, and have acquired the character of necessaries. In England, especially, the greater energy of the lower rank is mainly to be attributed to that superiority in respect to domestic comforts, which they possess over those of the two sister countries; comforts, which having once enjoyed, they will never resign, whilst industry and activity can secure their continuance. Englishmen of the lower order, when scarcity visits the land, or when calamity in trade occurs of sufficient extent to deprive them of the power of subsisting by means of their own industry, fall back upon the poor-rate. By this they are relieved, until the cause of their depression is removed. The elasticity of their minds is thus preserved, and their spirits remain unbroken. In such cases, relief is generally administered, according to the wants of the several applicants, by the most respectable and conscientious individuals of the middle class, who, while their sympathy is sufficiently awakened, are restrained from inconsiderate or lavish bounty, by consequences in which they themselves must participate. This kind of relief, it appears to me, has no natural or direct tendency to debase those to whom it is extended; for the poor-rate is to them a capital, indirectly arising out of their own former labour, and upon this capital they have a claim, until, by the revival of trade, their industry and activity are again called forth unimpaired. Whilst preferring such a claim, no individual can possibly entertain a feeling of degradation. But on the other hand, no one can feel otherwise than debased, who is compelled, by the short sighted policy of the higher classes in society, to solicit alms as a vagrant beggar.

An opinion has been maintained by a political economist of great and deserved celebrity, that want and misery operate as checks to early marriages. This opinion appears to me to be ill founded; for prudential restraint exists, I believe, only, where a certain degree of comfort and luxury is enjoyed, and where the sacrifice of those comforts must be the inevitable consequence of an indiscreet marriage. On the principle assumed by Mr. Malthus, that the means of subsistence regulate the amount of population, without any reference to the habits of the people, the population of England, where there is comparatively little misery, ought to bear a much greater proportion to its extent than that of Scotland or Ireland, where this supposed check to natural increase exists in a far greater degree. The fact, however, is otherwise; and in those countries, the real increase is even much greater than it appears, on account of the constant emigration into other countries where a better subsistence may be procured.

In England, the lower classes now form a community of no small consideration in the state. They are great consumers both of the produce and of the manufactures of the country, and having acquired a taste for domestic comforts, and even for a degree of luxury, their wants have established a home demand, superior in its extent to the market afforded by all the world besides. It cannot, however, be said of the English poor that they have eaten the bread of idleness; for had that been the case, this country never could have attained its present state of wealth and comfort. Great wealth can only be accumulated where great industry prevails; and the superior opulence of the middle rank in England furnishes, in my view, a complete answer to the arguments against the poor laws. By those laws, the lower orders have been prevented, under adverse circumstances, from falling into that lethargy so fatal to industry, which seems to have seized the people of other countries where no adequate provision is made for the indigent. And the energy, thus excited and cherished, has amply repaid the middle and higher classes, by extending itself to them, and even to the government of the country.

Of the utility of the poor laws, in abating the calamities that must occasionally visit manufacturing countries, and in enabling the poor to wait patiently for better times, we had not long ago a striking instance in this district. At the period alluded to, our commercial channels were in an instant stopped, and it became necessary to discover new outlets for the produce of our industry. To accomplish this, a considerable time was requisite, and whilst the proper arrangements were in progress, an unavoidable stagnation in trade ensued, causing a scarcity of employment, by which misery threatened to become almost universal among our labouring poor. During this change, the middle and higher orders maintained their situation on the scale, without much apparent privation. But the poor, who have only their hands to support them, almost immediately fell into great distress. When they found it impossible to obtain employment, it surely could not have been expected that, without some such provision as the Poor Laws, they would have quietly borne the severest privations, and have calmly observed the class above them suffering comparatively little from the pressure of the times. From this example, we may fairly conclude, that when any change takes place in one class, it must, to preserve the peace of society, be accompanied by a relative movement in the class above it, consisting in the sacrifice of property to the exigencies of the needy. The compulsory support given by our Poor Laws, at the period we have alluded to, prevented distress from extending to any great length, and from producing rebellion. Nor, except in comparatively few instances, was reluctance shewn to relinquish relief from this source, when employment once more became obtainable. In Scotland and Ireland, the poor, under similar circumstances, obtained relief by soliciting charity from door to door; and the sum, levied in this manner, probably amounted to at least as much in proportion, as was produced in England by the compulsory rate.

It is generally admitted, that all conditions of society have been gradually improving for the last fifty years; and this improvement is alone attributable to the increased industry and enterprize of the community. It is to this cause that such a wonderful accumulation of wealth has taken place in England, notwithstanding the immense sums expended in carrying on foreign wars; for what would all the gold in the world, at the disposal of the wisest and best men, have accomplished, had we not been supported by the labour of the industrious? It is to that ardent desire for comforts, and to the artificial wants pervading the lower orders in England, that we are to ascribe that industry which creates the wealth of the country, and adds to the prosperity of every rank.

To complain of the tax which the poor laws exact—to assert that it will absorb the whole rent roll of the kingdom, appears to me to be equally unfounded and unjust. That such consequences will ever happen, can scarcely be credited; for it may be demonstrated that the gross product, both of the land and of the manufactures of England, has for the last two years, in consequence of the low rate of wages, been brought into the market at a much smaller expense than for the twenty years preceding, even taking into the account the £8,000,000 said to be annually paid to the poor. I am inclined, however, to question whether so large a sum has ever been expended upon the poor in the space of one year. But even admitting the fact, it will be found, that eight millions, so distributed, do not amount to more than one shilling per week, to each individual labourer. During emergencies, however, the working poor themselves are virtually contributing a much larger amount, by the sacrifice of a considerable part of their wages; for while the individual receives one shilling from the poor-rate, he is perhaps deprived of two or three shillings by the reduced price of his labour; and this sacrifice benefits the consumer, by enabling him to obtain the produce of labour at a cheaper rate. In Scotland and Ireland, on the other hand, the prevailing system of vagrancy has a tendency to enhance, on the consumer, the price of articles, by withdrawing a large portion of labour from the market. For labour is like all other commodities, and will at all times bear a price regulated by the demand for it, and the quantity which the market affords. Nor is the labour, to which vagrant gangs have occasional recourse in other countries, of much efficacy in regulating its price, for it is always found that the habits of such persons effectually prevent them from entering into competition with the regular and stationary labourer.

The small expence at which the Scotch and Irish support their poor, compared to the amount of the English poor-rate, has been brought forward as an argument in favor of the system pursued in those countries. But the expence would be very nearly as small in England, were the poor reduced to the miserable fare, which is the lot of the same class in Scotland and Ireland. To obtain, however, this reduction of comfort among the poor in England, were it practicable, would, for reasons which have already been explained, be highly impolitic, and would totally extinguish one of the most powerful incitements to that regular and persevering industry, which brings in its train comfort, cleanliness, health, and a feeling of self-respect highly favourable to good morals. Let us dispassionately consider the increased population and capital of the country; the increase of indulgences among the middle and higher classes; the depreciation of the value of money within the last 50 years; and the advanced price of all the necessaries of life, and we shall find, I believe, that the sum now expended upon the poor, throughout England, is not much greater than it was fifty years ago.

In estimating the condition of the poor of any one country, we are by no means to compare it with that of the same class in another country, but always to view it with reference to that of the order above them in their own. To judge of the degree of comfort suitable to their station, we must compare, for instance, the English poor with the classes in this country who stand above them, and not with the lower orders of French, Scotch, or Irish. Were the comparison of the poor of one country with the poor of another made the ground work of our reasoning, we might with equal propriety compare the lower orders of English with the Laplanders or Hottentots. But to a just consideration of the subject, it is essential that we extend our view to all those circumstances that arise out of local or peculiar causes. And even among the poor of different districts in the same country, we shall find varieties in their habits and wants, which will materially influence our judgment of their condition, relatively to that of the other classes of society, and of the extent of their claims for relief.

The conclusions to which I have been led, are not, I am well aware, those which promise to become most popular. The tide of general opinion has long shewed a tendency to set against the expediency of any fund, drawn by compulsion from the higher classes of society, for the support of the poor; and both argument and eloquence have been employed to prove such a provision to be a bounty held out to idleness and indiscretion. But I trust it is a conclusion, borne out by the facts which have been stated, that *when properly administered,* (for I am fully aware that great abuses have grown out of the system,) the poor laws, while they afford a refuge to the indigent from unavoidable misery, benefit the higher classes of the community by insuring a permanent supply of active labourers;—that they operate as checks to mendicity and vagrancy;—that they do not encourage an unnatural increase of population; and that the funds, which they supply, are in truth to be considered as derived from the industry of those, for whose benefit they are destined.

MEMOIR
ON
SULPHURIC ETHER.

BY JOHN DALTON.

(Read April 16, 1819.)

In my essay on the force of steam, read before the Society in 1801, and published in the 5th volume of the Memoirs, I stated some experiments on the force of vapour from sulphuric ether, at different temperatures, as exhibited in a Torricellian vacuum, also the force of the same when admitted into a limited portion of air. From these experiments, as well as from corresponding ones made with water, alcohol and other fluids, I was led to adopt the important conclusion, that steam acquires the same force in air as in a vacuum, and that it ought to be considered the same independent fluid in both cases. Consequently if p denote the pressure of any given volume of air (1), and f denote the pressure of steam of a given temperature, such steam being admitted to the air, the volume of both in due time becomes $\frac{p}{p-f}$.

This theorem is most beautifully illustrated by sulphuric ether. Let a common barometer have a drop of ether let up into the vacuum; it will instantly depress the mercury several inches, more or less according to the temperature. Suppose it were 10 inches, the barometer being 30; then $\frac{p}{p-f}=1.5$; that is, if ether be passed up into air under those circumstances, it will in due time increase the volume of air 50 per cent.

For six years after this I was occasionally engaged in the further investigation of the nature and properties of ether, in which several additional facts, and some corrections of those antecedently announced, occurred. The combustion of ether was effected in various ways, as well as its analysis, by heat and by electricity.

During all this time I procured my ether in small quantities at a time, and of various druggists as suited my convenience. Once or twice I ascertained the specific gravity of the article to be at or near .75; and I never found reason to suspect there was much difference in the specimens. Occasionally when great part of the ether was evaporated by time and neglect, I found a few drops at the bottom of the phial, which did not possess the properties of ether, but this was too small to be much regarded.—In an excursion to Edinburgh and Glasgow in 1807, I exhibited the steam of ether as above described, to a few persons in those two places; when at the latter place, Dr. Ure was so good as to supply me with ether, but upon trial it did not present the properties I had usually recognized, which at the time I attributed to accidental impurities, acquired in the laboratory; upon this he accompanied me to a druggist, where I was immediately supplied with ether of the requisite purity. I apprehend Dr. Ure's ether must have been the *spiritus ætheris sulphurici* of the Edinburgh college, made by adding 2 parts alcohol to 1 of ether; or perhaps ether not rectified.

In 1808 I published the first part of my New System of Chemical Philosophy, in which I digested all the knowledge I then had on the force of steam from ether in a tabular form. I had acquired from actual observation, the forces in a range of temperature from 0° to 212°. In my former publication I had concluded that the variations in the force of steam from water and ether were the same for the same intervals of temperature; that is, if the force of steam from water was diminished from 30 to 15 inches of mercury, by a diminution of temperature of 30°; then that of ether would be diminished from 30 to 15 inches by the same number of degrees, though in a much lower part of the scale; the former being from 212° to 182°, and the latter from 98° to 68°. Subsequent experience, however, led me to apprehend that the above intervals of temperature, though expressed by equal expansions of mercury, are not in reality equal intervals; but that equal intervals are rather denoted by the forces of steam being in geometrical progression. Consistent with this view I found that steam from water and ether would concur, for a long range of temperature, with the difference of ratios only; that of water being 1.321 for 10° of temperature, whilst that of ether was 1.2278.

In the above work occurs the following observation; "Ether as manufactured in the large way appears to be a very homogeneous liquid. I have purchased it in London, Edinburgh, Glasgow and Manchester, at very different times, of precisely the same quality in respect to its vapour." This observation, though warranted from my limited experience at the time, I now find not altogether correct; I am sorry that it has occasioned an ingenious experimentalist to be led into a labyrinth of error.

The bulk of the ether used in this country has, I find, of late years been prepared by one manufacturing house in the neighbourhood of London. Three qualities of the article are made according to the different uses intended. The highest quality is only made for particular purposes, and is therefore not very commonly met with; it is about .73 specific gravity; the second quality is that intended for medicine; it is of .75 specific gravity, and is that with which all the country druggists and apothecaries are or ought to be supplied as a standard uniform article; it is that which I have always met with in the shops, and which I have taken for genuine ether in my former experiments.—The third quality is of the specific gravity .78 or .79 usually; of course it is much inferior to the last in purity. But it may be proper to observe, that this is the first state of the other two qualities; they being produced from this by ulterior processes called *rectification.*

It is well known that sulphuric ether is procured by distilling a mixture of sulphuric acid and alcohol. The proportions usually prescribed are equal weights of concentrated acid and alcohol. By due management a liquid of the specific gravity .785 or .79 is obtained, called ether. It is the ether of the third quality, just mentioned, and is in fact, a compound of alcohol and ether chiefly, in proportions to be investigated hereafter. In this state it is usually called *unrectified* ether.

When this last liquid is redistilled by a moderate heat till one half has passed over, the liquid in the receiver is denominated *rectified* ether. It is usually about .75 specific gravity, corresponding to the *second* quality. It still consists of ether and alcohol, but with much less alcohol than before. There is great reason to believe that both the *unrectified* and *rectified* ether, as thus prepared, are destitute of water, except so far as it is an essential element of the two liquids, ether and alcohol in their purest states; the sulphuric acid being well able to retain all the excess of water of common alcohol in the temperatures employed in the two distillations.

Ether of the first quality, or that in its purest state, is to be obtained from the rectified ether just mentioned. The object is to abstract the alcohol still remaining in the rectified ether. This may be done in great part by repeated distillations; always taking the first produce and setting aside the remainder for other use; but this method is tedious and expensive. A more ready method is to agitate the rectified ether with about its own bulk of pure water; after agitation the mixture resolves into two fluids, a heavier and a lighter; the lighter may be decanted, and will be found about $\frac{2}{3}$ of the volume of ether used; it will have the specific gravity .73 nearly, and may be considered as ether of the first quality. But it is demonstrable that it still contains *some* alcohol, and has besides acquired a portion of water from this process. The watery stratum below contains the greatest part of the alcohol, and has also taken along with it a portion of ether, as is evident from the smell, which is much the same as that of ether itself. This heavy liquid has the specific gravity of .96 or .97 usually. If this ether of .73 specific gravity be again treated with water it will be reduced nearly to .72 specific gravity; but it still contains minute portions of both alcohol and water, the quantities of which are not easily appreciated. Subsequent distillation would doubtless improve the quality a little; but for most practical purposes there is reason to believe that no material difference would be found between the above and ether of absolute purity.

Having obtained ether of the sp. gravity .72 and alcohol of .83 specific gravity, both of which may be considered as very nearly pure or free from water; mixtures of these two liquids may be made in any proportions and the resulting specific gravities ascertained; from which we may be enabled to estimate the proportions of the two fluids in any specimen where no water is present.

This operation however is more difficult than may be imagined. By taking ether and diluting it successively with equal portions of alcohol, the resulting specific gravities may be found in the usual way, provided we could guard against any loss of the mixture. But such is the evaporating power of ether, especially when pure, that it is impossible to pour it from one vessel into another in the open air without much loss.—In one instance I found that after 6 successive dilutions and 12 transfers, made with great care, I had lost $\frac{1}{5}$ of the whole weight used. In such case, if the diluting portions are not diminished duly, the results must be erroneous. One circumstance is favourable, the increase of density by chemical action appears to be very small; so that the densities may be *calculated* without very material error. The following table will afford a moderately good approximation, which may have its use till a better is made.

Table of the Specific Gravities of mixtures of Ether and Alcohol.

Ether.		Alcohol.	Specific Gravity.
100	+	0	.720
90	+	10	.732
80	+	20	.744
70	+	30	.756
60	+	40	.768
50	+	50	.780
40	+	60	.792
30	+	70	.804
20	+	80	.816
10	+	90	.828
0	+	100	.830

From this table it would seem that ether of the second quality, or that of the shops in general, contains about 25 per cent. of alcohol; and that of the third quality from 55 to 60 of alcohol; and the proportion of this article will be still greater on the probable supposition that pure alcohol is as low as .82 in specific gravity.

So far we have considered the mixtures of ether and alcohol in their purest states or nearly such; and it has been observed that in the ordinary course of manufacture, it is these mixtures, only varied in proportion, that occur. But if we introduce water so as to vary the proportions of ether, alcohol and water indefinitely, then some new phenomena occur, and the quantity of ether in such mixtures is no longer to be determined by the specific gravities. These mixtures are in some proportions uniform throughout; in others they resolve into two fluids of different specific gravities, alike transparent and colourless, but easily distinguishable from a filmy like surface between the two fluids. Both the heavy and light, or as they may be called, the watery and ethery fluids, contain in all cases less or more of all the three ingredients. They seem to vary in their sp. gravities according to this law; whenever the upper fluid is extremely light, the under one is extremely heavy, namely, about .72 and .98 respectively; and whenever the under fluid is extremely light, then the upper one is extremely heavy, but the two never approximate nearer than .93 and .82 respectively. As far as I have found I am pretty well convinced, that in this last case the heavy fluid is constituted of 1 atom of ether, 1 of alcohol, and 5 of water; and the light fluid of 1 ether, 1 alcohol and 1 water, being a true ternary compound of the three elements. These facts are beautifully exhibited by a single experiment. Let equal volumes of pure ether and water be agitated together; on subsiding, the very heavy and very light fluids are immediately perceived; let then pure alcohol be added by degrees and agitated; it will be observed that both fluids have increased in volume upon each addition, till at length the upper fluid arrives at its maximum volume and specific gravity. A further addition of alcohol then diminishes the volume of ether till at length it disappears, and the whole becomes one uniform fluid.

The boiling point of ether I find forms a curious part of its history. I mean that point of temperature when its vapour is of sufficient force to balance the weight of the atmosphere. In my early experiments I found the point by immersing a thermometer in the boiling fluid, when it stood at 102°; but in subsequent experiments I used a barometer tube bent about $\frac{1}{3}$ from the sealed end and the legs laid parallel. A small portion of ether was let up to the sealed end, and the tube from thence to a little past the turn was filled with mercury. The instrument thus prepared was immersed in a tall jar of warm water till the vapour arose from the ether and depressed the mercury, which ascending in the other leg, was brought to a level in the two. In this way, the same ether, in the temperature of 98°, exhibited a force equal to the atmosphere. Something like this I find takes place in alcohol of .83 specific gravity. It boils in a phial at 176°; but in a tube, its vapour is equal to the atmosphere in a temperature of 172°. Pure ether, of .72 specific gravity, boils in the tube at 95 or 96°, as Gay Lussac has observed; but in a phial I find the thermometer may be raised to 98° in the boiling liquid. The boiling point of a mixture of pure ether and pure alcohol may be made to vary from 96 to 170°; but we cannot infer the boiling point from a knowledge of the proportions of the mixture; it is always much nearer that of ether than the proportions would indicate. Indeed it is the same with alcohol and water and all similar mixtures. A mixture of equal parts of alcohol and water boils at 183°; whereas by the rule of proportion it ought to boil at 194°. A mixture of 4 parts ether and 3 parts alcohol I found boiled at 117° in the tube, and 122 or 123° in the air, which by proportion should have boiled at 127°. It was of specific gravity .769, and might therefore be considered as between the second and third quality.

The modifications of the boiling point of ether produced by *water*, however, are the most astonishing. The heavy fluid arising from the washing of ether by water, which is of the specific gravity .96, and which consists of 8 or 10 parts of water and 1 or 2 of ether and alcohol, boils at 103° in the tube; but if the temperature be increased it soon ceases to manifest the increasing progressive elasticity of pure ether, as may well be expected. The reason of this is pretty obvious; water possesses little or no affinity for ether; it yields readily the few atoms it possesses to the influence of heat, and when they are raised the supply ceases. Hence we see the necessity of using a pure ether when the tension at various successive temperatures is to be found.

Specific Gravity of Ether Vapour.

In 1803 and 1804 I made a great many experiments on the combustion of ether vapour mixed with oxygen gas by electricity. These sufficiently demonstrated the great specific gravity of this vapour, as it was sufficient to have 4 or 5 per cent. of volume of it to produce abundance of carbonic acid and to require a greater abundance of oxygen. I found it expedient to ascertain as near as possible the exact specific gravity, and attempted it as follows, in September 1803.

I took a balloon glass, of the capacity of 253 cubic inches, having a wide neck, to which was adapted a brass cap and stopcock. Into this a graduated tube, $\frac{3}{10}$ of an inch diameter, containing ether of .758 specific gravity, and a manometer were introduced; the manometer was as usual a tube of $\frac{1}{15}$ inch bore, closed at one end and duly graduated, with a globule of mercury sliding in it. The vessel was immediately made air tight, and kept so for several days, during which time the progress of the evaporation and of the gage was occasionally noted. The temperature of the air in the room was usually about 55°; but as this was of no importance, it was not particularly noted.—The observations follow:—the ether tube was graduated into water grain measures: barometer 30 inches.

		Manometer.	Measures of Ether evaporated.
Sep. 23.	2 P. M.	885	0
	5 ——	868	6.5
	8 ——	858	10.—
24.	1 A. M.	848	16.5
	9 ——	830	20.5
	3 P. M.	825	24.6
	10 ——	818	28.5
25.	1 P. M.	800	34.0
	12 ——	795	38.9
26.	2 P. M.	790	42.—
	9 ——	780	46.5
28.	9 A. M.	772	49.5

Now, 49.5 measures of ether=37.5 grains; and this quantity being by the manometer $=\frac{113}{772}$ of the atmospheric pressure, we have 113 : 37.5 :: 772 : 256 grains, the weight of 253 cubic inches of ethereal vapour of atmospheric force; but the weight of the same volume of common air=77 grains. Hence ethereal vapour=3.3 times the specific gravity of air.

I find amongst my notes in 1805, a similar experiment, from which the specific gravity was deduced=2.65 only. This difference occasioned me to repeat the experiment as follows:

Balloon containing 404 *cubic inches*=123 *grains of air.*

Barometer 30 inches.
Ether .728 specific gravity in the temperature 48°.

			Manometer.	Measures of Ether evaporated.
1819.	Feb. 25.	10 A. M.	... 4100	 0
		1 P. M.	... 4052	.... 8—
		2¼ ——	... 4040	.... 9.5
		3 ——	... 4025	.... 11.—
		5¼ ——	... 4000+	.... 13.5
		7 ——	... 3990	 15
		9 ——	... 3966	 17
	26.	9 A. M.	... 3908	 25
		11+ —	... 3903	.... 27—
		2 P. M.	... 3900—	 28
		6 ——	... 3881	 30
		9 ——	... 3870	 32
	27.	9 A. M.	... 3824	 36
		9 P. M.	... 3812	 39

At this period the cock was turned and the air and vapour let out, till the equilibrium was restored with the atmosphere, the barometer being then 29.5; the thermometer was not noted. In a few minutes the cock was again turned and the experiment continued.

Feb.	27.	$9\frac{1}{2}$ P. M.	... 4077	....	0
	28.	2 ——	... 4040	....	5.5
		9 ——	... 4025	....	7.5
March	1.	9 A. M.	... 4006	....	10
		9 P. M.	... 3985	....	14
	2.	9 A. M.	... 3969	....	15.5
		9 P. M.	... 3950	....	18.—
	3.	9 A. M.	... 3937	....	19.5
		9 P. M.	... 3919	....	22
	4.	9 A. M.	... 3908	....	24
		9 P. M.	... 3890	....	25
	5.	9 A. M.	... 3885	....	27
	6.	9 ——	... 3874	....	28
		9 P. M.	... 3874	....	28+
	7.	9 A. M.	... 3874	....	28+

For the last two days there was only a drop of fluid left at the bottom of the tube (nearly 5 inches deep) which seemed to be not evaporable; but it was judged proper to continue the experiment in order to ascertain whether the vessel was perfectly air-tight, and of course the gage would continue stationary. The drop of fluid smelled of alcohol, and when diluted and treated with muriate of barytes was milky.

By making the calculation as above, the specific gravity of ether vapour from the first part of the experiment comes out 3.05, and from the last part 3.2.—The slow manner in which ether evaporates in these circumstances is surprising;—In the latter part of the experiment it is to be ascribed to the depth of the surface of fluid in the tube, and the partly saturated air.

Though convinced the above results were very good approximations, I was desirous to have a confirmation of it by some more direct method.—I took a bottle of the capacity of 2600 grains of water, and graduated accordingly; this being filled up to 1100 grains with dry mercury, was inverted in the mercurial trough with 1500 common air. Through this mercury were passed 1, 2, 3 or more grains of ether, which expanded the air, and from the quantity of expansion, compared with the weight of ether let up, the specific gravity of the vapour was inferred. This method did not give uniform results owing to a considerable portion of such minute quantities of ether being entangled by the mercury in its passage. To remedy this, I took a small tube, $\frac{1}{7}$ of an inch in diameter internally, and 2 inches long, which was sealed at one end, and then graduated into water grains, which was such as to allow nearly $\frac{1}{4}$ of an inch for one grain. This was filled with mercury, except for 1, 2 or more grain measures, which were afterwards filled with ether, and the finger being applied, the tube was plunged into the mercury and passed through the neck of the bottle up to the surface of the mercury in the bottle. In this way the ether was conveyed through the mercury without quitting the tube, and by gentle agitation was ejected and dissipated in vapour in a few minutes afterwards. The results in several experiments were nearly uniform, giving an increase of volume of gas from 255 to 275 grain measures for each grain of ether in weight. This gives the specific gravity of ether vapour from 3.1 to 3.3. On the whole, I think 3.1 is probably the nearest expression in two places of figures that can be attained.

Elasticity of Ether Vapour, the same in Air and in a Vacuum.

The same tension or elasticity of ether vapour takes place in air as in a vacuum, just as with the steam of water and other liquids. But this is not true of impure ether if it be made to pass through water into the air, because by this operation it is improved in quality, though greatly diminished in quantity.

When the temperature of the air was 43° and barometer 29.70, I passsed up through water into a graduated tube containing 51 grain measures of air, about 3 or 4 grains of .73 ether. The aír was in a few minutes expanded to 74 measures; and the ether barometer (that is, a barometer with the same kind of ether thrown up into the vacuum) stood at 20.5 in the same temperature; hence we have $\frac{29.7}{20.5}\times 51\left(\frac{p}{p-f}\times 51\right)=74$ nearly; which accords with the beforementioned theorem. The tube being afterwards immersed in water of 66°, gave 104 measures of vapourized gás; and in 70° gave 118 measures.—It stood for some months in water, still retaining a fluctuating volume of gas, according to the changes of barometer and thermometer; and at last the gas was passed through water, and instantly gave the original 51 measures of air.

The quality of ether may be judged of from passing a small portion of it through water in a graduated tube. Thirty grain measures of the best ether (.73), passed up a tube of 8 inches long filled with water, lost 4 or 5 grains. Thirty grains of another ether, consisting of a mixture of 15 ether (.735) and 15 alcohol, (.85) when passed in like manner, only gave 5 measures of fluid ether, swimming on the surface of the water.

Relation of Ether Vapour to Liquids.

Gases vapourized by ether may be kept over dry mercury, and transferred through the same without loss. But they are not kept over water, alcohol and other liquids without loss of vapour, though this is variable according to the nature of the fluid and other circumstances.

Alcohol absorbs ether vapour out of air much faster than water does.—I filled two similarly graduated tubes with etherized air, and placed them over alcohol and water respectively: they lost vapour as under:

Tube over Alcohol.	Tube over Water.
155 measures.	155 measures.
116 in 5 minutes.	142 in 10 minutes.
112 in 8 minutes.	138 in 13 minutes.
104 in 30 minutes.	130 in 30 minutes.
100 washed.	100 washed.

The non-efficiency of water in abstracting ether vapour, is farther manifested by the following experiment.

I took a tall graduated cylindric jar, of 3 inches diameter, into which 20 oz. measures of air were passed over water. Thirty grain measures of ether (.73) were then passed up into the air, through a volume of

5 inches of water, which was of course diminished a little in its passage, and then spread over the surface of the water, to the thickness of $\frac{1}{200}$ of an inch nearly.—The volume of air and vapour varied as under.

H.	M.	oz.
—	--	20
—	3	$22\frac{1}{2}$
—	6	24
—	12	26
—	20	$27\frac{1}{2}$
—	26	$28\frac{1}{4}$
—	32	$28\frac{1}{4}$
—	50	28
2	27	27
1 Day		23
1 Week		$21\frac{1}{2}$
	Washed	20

Here it is observable, the vapour increased for half an hour and then began to decline again, but slowly. It increased the volume by $8\frac{1}{4}$ oz.=3960 grain measures, which is equal to 15 grains in weight by the preceding determination; but the ether weighed 22 grains; so that a loss of $\frac{1}{3}$ of the weight of the ether only, was occasioned by the action of so great a surface of water on it for half an hour.

Force of Ether Vapour.

My former experiments on the force of ether having been made with an article not of the highest purity, they ought all to exhibit a force *too low* for the temperature. Such I find to be the fact; at least within a range of temperature of easy investigation, that is, from 30° to 140°.—The difference however is but small, and may, without much error, be corrected by deducting 2 or 3° from the respective temperatures, as given in my table. (New System of Chemistry, page 14). The apparatus to be used, consists of a common barometer tube, one bent into a syphon at $\frac{1}{3}$ of the length from the sealed end, and a tall smaller one bent six or seven inches from the sealed end, and having the other leg 40 inches long. The first of these instruments is best used for atmospheric temperatures, having a drop of ether let up into the vacuum. The second, is to have its short leg filled with mercury, and an inch of the other leg, a drop of ether being at the top of the mercury in the short leg. This is used from temperature 80° to 110 or 120°. The third is to have its short leg filled with mercury, and a drop of ether as the other, and its long leg filled th various heights with mercury, according to the temperature. It may be advantageously used from 120 to 140°. For temperatures between 140° and 212°, I have always used a tube similar to the last mentioned, but having its upper extremity sealed, and containing air of common density over the mercurial column, and nearly equal in volume to the capacity of the other leg. When the ether vapour is formed in force, it condenses the said air, and from the condensation, the force is inferred by a well known law.—Having had some reason to suspect my former results by this instrument were somewhat too high; I have been induced to examine the defects to which this instrument is liable. The end of the tube must be drawn out to a point before sealing, and suffered to cool to the temperature of the air; after this the end must be closed by the point of a flame, otherwise the air in the tube may be rarefied by the heat, in which case the force of the steam will be overrated. Another cause of similar error, is the existence of ether vapour in the air at the moment of sealing; this will happen if the tube is not carefully dried inside after the instrument is filled with mercury. In this case, the air in the tube is rarefied by the steam.

and consequently is of an unknown, but reduced density. The opposite error is liable to be induced, by the frequent use of the instrument. By the motion of the mercury, the small remains of ether mechanically mixed with it rises to the top, and a visible stratum of ether is thereby exposed to the air. In this case an addition of force is given to the air; but as the quantity of this force is known for any temperature, it may be allowed for accordingly.—I prefer, however, sealing the tube when well dried and the air of atmospheric density at the time; and if the ether appear to rise to the surface afterwards, the correction must be applied. In order to have a complete check upon this instrument, it should be adapted so as to be applicable at some temperature (as 140°), where the force is known by other direct means. The error, if any, will thus be shewn, and may be calculated for other temperatures.

I have lately made, for the first time, various experiments on the force of steam from water, in temperatures from 212° to 300°; the results which convince me that the theoretic forces which I gave in the 5th Vol. of the Memoirs, as also, those subsequently in my Chemistry, are both erroneous; the former being about as much too small as the latter are too large, so that the mean of the two series is a near approximation to the truth.

Experiments on the force of aqueous steam in high temperatures, have been lately made by Mr. Southern of the Soho, Birmingham,* and by Dr. Ure of Glasgow,† the results of which agree very well with each other, and with the mean of my two theoretic tables. As for the force of steam below 212°, no one has found any material variation from those in my first table; indeed scarcely any one seems to have attended much to those below

* Dr. Robison's Works by Dr. Brewster.

† It would have given me great pleasure to have been able to adduce Dr. Ure's experiments on ether also, in corroboration of my early experiments, and of the general principles thence derived; a stronger condemnation of those principles could not have been brought forward, than their agreement with the results of Dr. Ure on ether vapour.—All the information we have given as to the quality, &c. of his ether, is contained in the following paragraph. "The ether of the shops, as prepared by the eminent London Apothecaries, boils generally at 112°; but when washed with water or redistilled, it boils at 104° or 105°. It may, by rectification, however, be made to boil at a still lower temperature".—We are presented with two series of experiments on the force of ether vapour; the first begins at 34° with the force 6.2, and ends at 104°, with the force of 30 inches of mercury; the second begins at 105° with the same force, and ends at 210° with the force 166 inches.

100°, which I was most anxious to have correct.—The force of steam at 32° is an important element; I have spent much time and labour upon it, both before and since my first table was published; it is not less, I think, than .2 of an inch, nor more than .3; these being the extremes of my experiments; perhaps .25 is very near the truth.

My table of the force of alcoholic vapour represents it too high for temperatures below 60°, and for those above rather too low. These errors arose partly from the alcohol not being free from water, and partly from a mistake, as I now apprehend, in fixing a standard mark on the alcohol barometer.—They are but small and of little importance, as the observations were not used in establishing general principles. An improved and more extended series of observations on the force of alcohol vapour has recently been published

What the specific gravities of the two kinds of ether used were, and whether the ethers used were obtained from the very inferior ether of 112°, by washing, or by distillation, are important points, concerning which we are not informed. However, Dr, Ure contrives to blend these two disjointed series, and to compare the results with those of mine made upon ether which boiled at 98°; and finding great discrepances, he concludes my results on ether and principles deduced from them are pregnant with errors.

by Dr. Ure as mentioned above, the results of which fall in as well as can be desired with those from water, in establishing a general law that the vapours of homogeneous liquids expand in geometrical progression to equal intervals, or at least to *the same* intervals of temperature. I may add, my own experiments recently made for the first time, corroborate those of Dr. Ure in the interval of temperature from 175° to 212°.*

The following skeleton of a table of the force of vapour from water, alcohol and ether, is formed from what I consider as the most correct experiments hitherto made on these subjects, and may have its use, though it will be found not to differ very materially from my former tables, except where they differ from each other.

* Philosophical Transactions.—1818.

Table of the Forces of Aqueous, Alcoholic, and Ethereal Vapours.

Temperatures. (common scale.)	Aqueous Vapour. Ratio 2.6 Inch.	Alcohol. Vapour. Ratio 2.7 Inch.	Ethereal Vap. Ratio 2. Inch.
36°	.29	.56	7.5
64°	.75	1.51	15.—
96°	1.95	4.07 (*f*)	30
132°	5.07 (*a*)	11.00 (*g*)	60
173°	13.18 (*b*)	29.7 (*h*)	120
220°	34.2 (*c*)	80.2 (*i*)	240
272°	88.9 (*d*)	———	———
340° (*e*)	231.—	———	———

Dr. Ure's numbers for ether corresponding to the above, the last exclusive, are, 6.55, 13, 25.7, 49. 8 [49], 96.4; the ratio is of course less than two, and a descending one, namely, 1.98, 1.97, 1.94 and 1.93; this last circumstance characterizes a mixed liquid.

I have not extended the experiments on ether further than 212°; but as that temperature gives a force of 207 or 209, I estimate the force to be 240 at 220°, nearly.

(*a*) Southern 4.71.— Ure 4.7
(*b*) ——— 13. .— —— 12.95
(*c*) ——— 35.2 .— —— 35.5
(*d*) ——— 88+.— —— 89.— ——— 90—The mean of my two tables.

(*e*) This observation is Mr. Southern's. There is reason to suspect his temperatures too high for his forces in the high pressures. They exceed Dr. Ure's.

(*f*) Ure 4.02
(*g*) — 11.2
(*h*) — 30.00
(*i*) — 78.5—Bettan. 82.

If the forces registered in the preceding table be allowed as near approximations to the truth, it must, I think, be admitted that they increase in geometrical progression, to the same intervals of temperature for a range of 200° at least. Whether those intervals of temperature are equal one to another successively is another enquiry, which the above facts and observations do not enable us to decide.

Analysis of Ether by Electricity, &c.

When a little fluid ether is let up into Volta's eudiometer, either over mercury or water, and a small portion of azotic gas is likewise sent up, in order to be vapourized by the ether; then if the vapourized air be electrified for an hour, some permanent gas is produced, and charcoal is precipitated. The gas when washed is chiefly or wholly carburetted hydrogen; for it takes two volumes of oxygen, and yields one of carbonic acid gas. If the vapourized gas be dry and over mercury, a volume of vapour yields two volumes of carburetted hydrogen, and moisture is perceived within the tube.—If the electrification were continued, no doubt the volume gas would be greatly increased, and end in pure hydrogen mixed with azote.

These experiments are not decisive; but they evidently point out the composition of the atom of ether to be 1 carburetted hydrogen, 1 charcoal and 1 water, or 2 olefiant gas and 1 water.

The best method of analysis is, by firing the vapour of ether mixed with oxygen gas in Volta's eudiometer. This method I discovered in September 1803, and have used it occasionally ever since. It may be proper to describe the various modifications of which this process is susceptible.

When a few drops of ether are passed through water into the eudiometer containing oxygen gas, the volume of the gas is in a few minutes enlarged more or less, according to the temperature. In temperatures from 60° to 70°, the volume is about doubled; but below those it is less than doubled, and above more than doubled, agreeably to the principle before explained.

(*a*) If the air be doubled or more, and an electric spark be taken in it, the probability is, that no explosion will ensue; if by repeated sparking an explosion take place, it is feeble, and may be repeated a few seconds afterwards, sometimes once or twice. The residue of gas being examined, is found to contain a little carbonic acid, some new combustible gas, and oxygen in various proportions. In short, the operation is very incomplete, owing to an excess of ether vapour.

(*b*) If the oxygen gas be good, and the volume be increased from 100 to 150 by the vapour (which will naturally arise in temperatures between 40° and 50°, and in higher temperatures the volume may be reduced by cautious agitation, till the water has absorbed part of the superfluous ether and vapour), then a spark produces a violent explosion. The gaseous volume is doubled, or from 150 becomes 300; and upon examination is found to consist of carbonic acid and new combustible gas, but chiefly the latter. Little or no oxygen is found.

If the ether vapour be only from 3 to 10 per cent. of the volume of oxygen, the explosion is vigorous, and a complete combustion takes place. The residue consists of carbonic acid and oxygen gases only. Ten volumes of ether vapour require about 60 of oxygen, and produce about 40 of carbonic acid.

(*c*) If 100 oxygen be increased by ether vapour to 120 or 130, a violent explosion ensues, and the whole of the vapour is converted into carbonic acid, water, and new combustible gas; a little charcoal is sometimes deposited, so as to make the air muddy at the instant after explosion; no oxygen is found in the residue.

(*d*) The combustion of ether vapour may be effected by common air as well as by oxygen gas, only the proportion of vapour to air is very small and limited. If the vapour exceed 5 per cent., it will not fire; and if it fall short of two per cent., it rarely fires. The combustion is attended with the production of new cambustible gas, or otherwise complete, according to the greater or less proportion of vapour, as is the case with oxygen gas.

In respect to the *new combustible gas* in the above paragraph, its nature may be ascertained by abstractihg the carbonic acid in the usual way, and then exploding it with oxygen. In the paragraph (*a*), the new gas is often nearly pure carburetted hydrogen; but in (*c*) and (*d*), it is always a mixture of carbonic oxide and hydrogen, in nearly equal volumes; as is proved from its requiring 50 per cent. of oxygen, and producing 5 0per cent. of carbonic acid. In (*b*) it is chiefly these two gases, but has a little carburetted hydrogen occasionally mixed with them.

When a certain volume of ether vapour is completely burnt at one operation, or it is partially burnt at the first, as in *(a)*, *(b)*, *(c)* and *(d)*, and the combustion finished by a second operation, still the same volume of vapour requires the same volume of oxygen for its complete combustion, and produces the same volume of carbonic acid. And it is always found that the carbonic acid contains $\frac{2}{3}$ of the oxygen spent, and consequently the hydrogen engages $\frac{1}{3}$ of the oxygen to form water.—Hence it appears that the combustible element of ether is olefiant gas; but as there is reason to conclude that oxygen is one of the elements of ether, it must be combined with hydrogen, so that water must be the incombustible element.

In order to find what number of atoms of water and olefiant gas must be combined, to form one of ether, we must have regard to the *weights* of the different elements which combine. Now, from the experiments above related, it appears that 1 measure of ether vapour (weighing 3.1) requires 6 measures of oxygen gas (weighing 6.6); but 2 atoms of olefiant gas weigh 12.8, and 1 of water weighs 8, making together 20.8, which would require 6 atoms of oxygen, weighing 42, for their combustion; that is, such compound atom would require rather more than double its weight of oxygen, which is the proportion I find by experiment for ether vapour. Hence then we may conclude, that the atom of ether weighs 20.8, and is compounded of 1 atom of water and 2 of olefiant gas.

In January 1809, I made an experiment on the slow combustion of ether in a lamp, in a large balloon glass. The capacity of the balloon was 2 cubic feet; hence the oxygen of the common air in it would weigh 250 grains, nearly.—A small lamp with ether was lighted and instantly dropped into the balloon, which was immediately closed. The lamp burned till it was extinguished for want of air. After a few minutes it was taken out, and the loss of weight ascertained to be 31 grains. The residuary gas being examined, was found to contain 16 per cent. oxygen, and 3 or 4 carbonic acid; but in order to obtain the carbonic acid more accurately, the whole volume of air was subjected to lime water, in such manner, that all the air which came out was agitated in the lime water that entered the balloon. The quantity of lime water requisite to saturate the carbonic acid, was as much as saturated 107 grains in weight of dry sulphuric acid=60 grains of carbonic acid=17 charcoal+43 oxygen. But the oxygen spent in the combustion was $\frac{5}{21}$ of 250 grains=60 grains, nearly, of which we find $\frac{2}{3}$, or rather more, in the carbonic acid produced; the rest must have combined with the hydrogen. And the ether consumed was rather more than one half of the weight of the oxygen, which may well be supposed to arise from a little loss by evaporation. This experiment therefore corroborates the conclusion above obtained.

My first idea of the ether atom, published in the table on the absorption of gases by water in 1803,* was 2 atoms of carbone and 1 of hydrogen. This incorrect notion was formed from some of my early experiments combined with the analysis given by others. M. Saussure, in his last essay on ether, has determined its proportions as under; which being compared with mine, are found to differ from them materially.

	Saussure's.	Mine.
Carbone	67.98	51.9
Oxygen	17.62	33.7
Hydrogen	14.40	14.4
	100	100.0

* Memoirs, Vol. I. (second series).

In the present essay I have alluded to the weight of an atom of alcohol; but this weight is not that given in my Chemistry, part 1. From recent experiments on the combustion of alcoholic vapour in oxygen by electricity, as well as from the combustion of alcohol by the platina wire lamp without flame, I believe the alcohol of .82 specific gravity, is constituted of 1 atom carburetted hydrogen, and 1 of water, as it seems to give carbonic acid=half the volume of oxygen consumed, or very little more. But there is a remarkable difference in the results when alcohol is burned in a lamp in common air. This combustion gives carbonic acid nearly=$\frac{2}{3}$ of the volume of oxygen, and would imply alcohol to be 1 water and 1 olefiant gas. At present I have not leisure to clear up this difficulty.

OBSERVATIONS

ON THE

BAROMETER, THERMOMETER,

AND

RAIN,

AT MANCHESTER:

From 1794 *to* 1818 *inclusive.*

BY JOHN DALTON.

(Read November 13, and December 11, 1818.)

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1. *Of the Barometer.*

IN a long series of observations it is scarcely to be expected that the same instruments can be used throughout. Accidents are occurring which either derange or destroy them. It is expedient, therefore, to notice such occurrences; as it seldom happens that instruments, particularly barometers, are replaced or renovated in like circumstances as before; and if this is not the case, they must necessarily mislead in comparison.

During the first period of five years, I had a barometer consisting of a straight tube of the usual length, and between 1 and 2 tenths of an inch internal diameter; it was carefully filled with dry mercury, and inverted into a cylindrical cup containing mercury; the diameter of the cup was such as to require scarcely any sensible allowance for the rise and fall of the mercury in the tube. It suffered no material change, as may be inferred indeed from the annual means, till near the end of the period. During my absence in August 1798, it had been in unskilful hands; a part of the mercury had been lost out of the cup, and probably a few atoms of air had got into the tube. Not being at that time particularly interested in meteorology, I contented myself with noting the daily observations as usual, without summing up the monthly means, or making any comparative observations. The consequence was, that some years elapsed before I was struck with the depression that had taken place in the mercury, which upon examination, appears to have been about $\frac{2}{10}$ of an inch. It was not till after another period of 5 years that I determined to renew the barometer. A tube was taken of about $\frac{1}{7}$ of an inch internal diameter, having a large bulb at the lower extremity; this was filled with mercury that had a few minutes before been boiled in order to expel the air and moisture more effectually. This was found to stand nearly $\frac{2}{10}$ of an inch higher than the previous one, and a few hundredths higher than it stood at originally; owing in part at least to the elevation or height of the barometer, above the level of the sea being 10 or 12 feet more in the first period than in the latter. In order to allow for the rise and fall of the mercury in the bulb, the scale of 4 inches was made only 3.98 inches, and subdivided into tenths as usual. The observations throughout the whole series were taken 3 times each day, namely, at 8 A.M. and at 1 and 11 P.M.—The new or last mentioned barometer has been used for the last 15 years, commencing with January 1804; and it may be right to observe, that for the 3 last months of 1798 it was used, but the reduction was applied to the monthly means, in order to make the observations of the whole year uniform. As the adhesion of the mercury to the tube is more or less observable in great variations, I make it a practice to give one or two gentle vibrations to the mercury prior to any observation, which I think is more accurate than using a wider tube without such vibrations.

The elevation of my barometer above the

level of the sea is nearly 180 feet; it is about 100 feet above the level of the Duke of Bridgewater's canal, and that is nearly 80 feet above the sea. This elevation is rather greater than would be deduced from the observations of the barometer for the last period of 15 years, in which the annual average height of the instrument is about 29.91. On the supposition that the average height of the barometer on a level with the sea is 30 inches, my barometer would be inferred to be about 90 feet above the sea; but I conceive the real average height of the barometer on a level with the sea has never been accurately determined. Possibly there may be some difference in the specific gravity of the mercury used for this instrument.

During so long a period of years, there must have occurred interruptions to the observations, but I never delegated any person to supply for me; these interruptions, however, scarcely happened except in the month of July, when they have been very frequent for half of the month. These blanks, however, have been filled up partly from the observations of Mr. Thomas Hanson of this town, who has kept a meteorological journal with great attention, for several years, and partly from the observations of the Royal

Society and other authentic sources, due allowance being always made for the difference in the absolute heights of the two barometers.

MEAN HEIGHT OF THE BAROMETER

At Manchester.

Yrs.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Octo.	Nov.	Dec.	Mean
1794	29.94	29.71	29.86	29.80	29.91	29.97	29.92	29.84	29.78	29.70	29.64	29.88	29.83
1795	30.03	29.60	29.76	29.71	30.11	29.80	29.89	29.89	29.99	29.52	29.70	29.82	29.85
1796	29 47	29.77	30.04	30.02	29.70	99.89	29.66	30.09	29.90	29.88	29.83	29.92	29.85
1797	30.00	30.23	29.94	29.77	29.78	29.83	29.96	30.06	29.64	29.81	29.87	29.60	29.87
1798	29.85	30.02	29.91	29.87	29.96	30.01	29.72	30.08	29.75	29.63	29.48	29.87	29.85
1799	29.74	29.49	29.66	29.46	29.62	29.80	29.57	29.50	29.53	29.52	29.61	29.80	29.61
1800	29.26	29.70	29.69	29.41	29.61	29.78	29.93	29.81	29.54	29.65	29.44	29.46	29.61
1801	29.59	29.56	29.61	29.86	29.65	29.88	29.65	29.88	29.73	29.62	29.55	29.29	29.66
1802	29.73	29.57	29.86	29.83	29.90	29.68	29.62	29.82	29.85	29.58	29.54	29.55	29.71
1803	29.55	29.67	29.88	29.67	29.77	29.83	29.93	29.86	29.87	29.87	29.36	29.46	29.72
1804	29.66	30.09	29.62	29.69	29.81	30.11	29.86	29.90	33.16	29.64	29.87	29.83	29.85
1805	29.57	29.77	29.91	29.87	29.95	30.04	29.97	29.97	29.97	29.94	30.28	29.80	29.92
1806	29.52	29.85	29.82	30.17	30.03	30.15	29.83	29.87	30.07	29.95	29.75	29.55	29.88
1807	30.10	29.81	30.08	29.76	29.85	30.07	29.96	29.94	29.84	29.90	29.55	29.85	29.90
1808	29.84	30.18	30.25	29.93	29.95	30.05	29.98	29.93	29.91	29.76	29.78	29.87	29.95
1809	29.57	29.72	30.12	29.86	29.96	29.98	29.98	29.80	29.80	30.23	30.03	29.82	29.91
1810	30.15	29.85	29.74	29.90	30.00	30.20	29.86	29.96	30.07	30.00	29.55	29.77	29.92
1811	29.95	29.53	30.18	29.86	29.90	30.05	30.18	30.02	30.07	29.70	30.01	29.79	29.94
1812	29.95	29.68	29.88	30.05	29.98	30.05	30.07	30.13	30.16	29.53	29.94	30.07	29.87
1813	30.14	29.77	30.23	30.05	29.84	30.13	30.00	30.17	30.11	29.75	29.81	29.90	29.99
1814	29.66	30.12	29.86	29.91	30.11	30.14	29.94	30.03	30.22	29.86	29.76	29.70	29.94
1815	29.94	29.83	29.77	30.02	30.00	29.97	30.20	30.00	30.05	29.92	30.04	29.83	29.96
1816	29.70	29.91	29.82	29.82	29.93	30.02	29.74	30.05	29.99	29.94	29.86	29.80	29.88
1817	29.84	29.97	29.68	30.35	29.87	29.26	29.80	29.83	30.09	30.11	30.02	29.66	29.85
1818	29.82	29.79	29.54	99.76	30.01	30.12	30.15	30.17	29.85	29.95	29.91	30.14	29.93
Means	29.78	29.81	29.87	29.86	29.89	29.98	29.89	29.94	29.92	29.80	29.76	29.76	29.85
Corrected for expan.	29.82	29.84	29.89	29.86	29.88	29.95	29.85	29.90	29.89	29.80	29.78	29.79	

In the above table, the means for each year are placed on the right, and the means for each month of the year at the bottom; by which we are enabled to judge whether any particular season or month of the year is more liable than another, to have an accumulation or deficiency of the atmosphere.—It is evidently only from a long series of years, that we are entitled to draw any conclusions of this kind.

From an inspection of the above table, it is obvious that the barometer appears to be higher in the summer than in the winter months; but this must arise in part at least from the expansion of mercury by heat; a correction therefore is necessary on that account, and the following table of temperature for each month in the year, with the known expansion of mercury, enables us to apply the proper correction. The correction being applied accordingly, we have the subjoined corrected heights for each month, On looking over this corrected column, we still perceive the mercury higher in the summer months than in the winter. The heights for March, April, May, June, July, August and September, are all at or above the mean; and June in particular, possesses a marked superiority of $\frac{1}{10}$ of an inch above the mean.

The heights of January, February, October, November and December, are all below the mean; those for November and December, are nearly $\frac{1}{10}$ of an inch below the mean.

These results were quite new to me. On looking over the observations in the 4th volume of the Memoirs, however, I found that similar results had been obtained by Mr. Hutchinson, from an average of 25 years' observations at Liverpool, namely, from 1768 to 1792 inclusive; and this period being entirely anterior to the one above, is of course totally independent of any fortuitous events in that period. Mr. Hutchinson's results, corrected for temperature, will stand as under; namely,

Jan.	Feb.	March.	April.	May.	June.
29.75	29.61	29.82	29.79	29.78	29.79

July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
29.78	29.77	29.67	29.71	29.67	29.68	29.73

Here the months March, April, May, June, July, and August, are all above the mean, and the remaining six months are all below it, except January.

Wishing to have further corroboration of the fact, I had recourse to the Register kept by order of the President and Council of the Royal Society, and collected the whole series of observations of the barometer for 38 years.—I found the results, corrected for temperature, as under; namely,

Jan.	Feb.	March,	April,	May,	June,
29.87	29.86	29.93	29.85	29.88	29 93

July,	Aug.	Sep.	Oct.	Nov.	Dec.	Mean.
29.87	29.91	29.88	29.84	29.82	29.84	29.87

Here again the months March, May, June, July, August and September, are all at or above the mean; and January, February, April, October, November and December, are all at or below the mean.*

By comparing all these together it would seem to be established as a fact, that from March to September, the weight of the atmosphere is greater than from September to March, in this part of the world.—This

* I cannot refrain from the remark that some of the annual tables of the Royal Society's meteorology, exhibit marks of extreme carelessness. In the table for 1815, I found *four* out of the twelve monthly means of the barometer greatly erroneous; namely, January, April, May, and October. The monthly means for August 1807, and September 1808, are also *greatly* wrong. These errors were detected by a comparison with my results for the same months; as I found the results at both places irreconcileable according to the known laws of barometry. As the above work, sanctioned by such authority, will naturally be regarded as a national standard in meteorology; it is greatly to be desired that the whole of the tables were revised and corrected.

cannot be ascribed to the account of rain; for the summer period contains wet months as well as dry; and in the London averages, the month of April is in the low period, though the driest in the year. It cannot be ascribed to temperature; for the month of November is warmer than March; yet this last is in the high period by all the tables, and the former in the low period. Nothing appears to me indicative of the periods, but the declination of the sun; it seems that when the sun is north of the Equator, the weight of the atmosphere increases in these parts, and again diminishes when south of the Equator.

The means by which the effect is produced, I conceive, are these: the sun's action is constantly increasing the mass of aqueous vapour in the atmosphere, during the period from the vernal to the autumnal equinox. That is, the whole mass of vapour existing in the atmosphere is daily increasing, notwithstanding the quantity precipitated. This fact is verified by the constant rise of the vapour point till the month of September, after which it commonly declines pretty rapidly. Now it is obvious that the addition of steam or aqueous vapour to the atmosphere, must add to the weight of the atmosphere;

and this is, I imagine, the cause of the increase of its weight in that season.

I am aware that another conclusion, the very opposite to this, may be deduced from the premises. Aqueous vapour, it may be said, is specifically lighter than dry air; and on that account, the greater the quantity of aqueous vapour, the less is the weight in any given volume of air of given elasticity. But it must be remembered that the aqueous vapour at the most, constitutes but $\frac{1}{50}$ part of the atmosphere, and any excess of this which may prevail in any one place, cannot be supposed powerful enough to move the rest of the atmosphere towards any other place, where the vapour is deficient. Now, we have no reason to believe that much intercourse takes place between the atmospheres of the northern and southern hemispheres. The great and unceasing currents of air are between the equator and the polar regions; but that any large volumes of air cross the equator from one hemisphere to the other, does not appear from any phenomena we are acquainted with. And if the air does not cross the equator, the vapour cannot, being so intimately blended with the air. Thus, although there may be a constant pressure or tendency of the atmosphere in the northern hemisphere to invade that of the southern, during our summer, and *vice versa*, in winter; yet I conceive it never can be so effectual as to restore a perfect equilibrium during the season, but will leave an excess of aqueous vapour in our hemisphere, unbalanced either by air or vapour, of the opposite hemisphere.

2. *Of the Thermometer.*

My thermometer is situated out of a window on the second floor, about 16 feet above the ground, and about 6 inches from the wall; it has an eastern aspect, and open, airy situation, is not affected by the sun, except in a summer's morning, and it is then duly shaded to prevent the sun's influence. The observations are taken three times a day, as with the barometer, at 8 in the morning, and at 1 and 11 in the afternoon. I have some reason to think the observations give a mean temperature, rather below than above the true mean. The temperature of springs in this place is usually between 48° and 50°; probably the mean annual temperature may be nearly 49°. The general annual mean as determined by my thermometer, is between 47° and 48°.—The monthly means for July

I have had usually to borrow from Mr. Hanson's observations, or from those of others at some distance, as for the barometer.

MEAN HEIGHT OF THE THERMOMETER

At Manchester.

Years.	Jan.	Feb.	Mar	Apr.	May	Jun.	July	Aug	Sep.	Oct.	Nov	Dec	Mean
	o	o	o	o	o	o	o	o	o	o	o	o	o
1794	31.7	42.3	42.4	49.7	49.5	59.9	62	59.5	54.1	47.7	41	36.2	48
1795	24.3	31.4	37.8	44.9	46	55	61	61	60.9	51.9	38.7	43	46.4
1796	44.	37.6	41.4	52.7	52	56.9	57.5	61	60.3	47.9	42	31.3	48.7
1797	40.6	41.3	41.4	47.2	54	57.7	61	60	55.7	48.4	42.5	41	49.2
1798	40.4	39.4	42	52.9	57.7	64.5	61	61	53	46.7	39.8	34.4	49.4
1799	34.6	36.2	37.3	40.4	47	55.2	57	55	53.5	45.2	40.4	33.5	44.6
1800	36.5	35.6	38.8	47.3	51.8	53.1	58.8	60.7	55.4	47.1	40.1	37.3	46.9
1801	39.3	39.4	42	46.5	51.9	56.3	58	62.1	56	49.2	39.6	34.6	47.9
1802	34.1	38.1	41	47.2	52.1	55.2	55.3	61.3	57	49.1	43.5	38.9	47.4
1803	35.1	38.2	42.7	47.4	49.5	55.6	61.7	58.7	51.7	47.9	39.9	37.8	47.2
1804	43	37.1	40.3	43	54.4	58.6	57	57.4	57.3	50.7	43.6	36	48.2
1805	34.3	38.9	43.6	46.1	51.2	56	61	62	57.8	46.1	40.7	37.5	48
1806	38.1	38.8	40	43.7	52.2	58.1	59	58.9	54.5	49.6	43.8	41.9	47.5
1807	36.3	38.1	36.8	46.5	53.4	56.1	61.2	62.1	50.1	51.7	37	35	47
1808	36.2	37	37.6	42.5	57.2	57.7	64	61.1	55	44.3	42.5	36.2	47.6
1809	34	42	42.5	41.7	55.2	55.2	57.5	57.5	53.3	50.1	40	38.7	47.3
1810	35	37.5	40.5	47.1	48.2	57.9	58.8	58.8	56.8	48.6	41.5	38.5	47.4
1811	34.2	39.6	45.4	48.6	55	57.8	59.5	58.7	57.1	53.4	46.1	36.4	49.3
1812	36.9	41.3	38.2	42.6	52.7	56.2	56	58.8	57.1	49	40.9	35.5	47.1
1213	35.1	42.4	44.7	47.2	54.6	58.5	60	58.9	55.5	47.4	39.1	36.7	48.3
1814	26.5	34.5	37.3	50.1	49.5	55.1	58	58.6	55.1	45.8	38	35.9	45.4
1815	29.2	39.3	40.9	45.6	51	54.7	54	56.9	54.2	51	40.5	36.5	46.2
1816	37.2	36.7	40	45.6	51.1	56.6	57	58.3	55.2	51.7	39.6	37.5	47.2
1817	39.8	43.6	42.6	47.2	49.2	59.5	57.5	57.4	57.7	45.3	48.5	34	48.5
1818	38.8	36.6	38	44	53.6	61.4	66	59.8	56.8	54	49	38.7	49.7
Means.	35.8	38.5	40.6	46.3	52	57.2	59.4	59.4	55.6	48.8	41.5	36.9	47·7
Maxim.	44	43.6	45.4	52.9	57.7	64.5	66	62.1	60.9	54	49	43	
Minim.	24.3	31.4	36 8	40.4	46	53.1	54	55	50.1	44.3	37	31.3	

3. *Of Rain.*

In the 5th volume of the Memoirs, part 2d. published in 1802, I have given an account of the depth of rain which fell in Manchester during the 8 preceding years, with the average monthly and annual means.—Having now a further series of observations for 17 successive years, it may be proper to give a detail of these last, and to incorporate them with the former, so as to obtain a general average for the whole period of 25 years.

The rain-gage has been all the time situate in a garden on the S. E. side of Manchester; it is twenty yards distant from any house or elevated object that can influence the fall of the rain. The gage is a funnel of 10 inches diameter, and the top is surrounded by a perpendicular rim of 3 inches high, to prevent any loss by the spray; it is fixed in a proper frame with a bottle for the water, and it stands above 2 feet above ground.

TABLE OF THE FALL OF RAIN IN MANCHESTER.

Years.	1802	1803	1804	1805	1806	1807	1808	1809	1810	1811	1812	1813	1814	1815	1816	1817	1818	Means.
	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.
Jan.	1.967	1.654	2,700	2.180	5.851	2.340	2,676	—.800		2.116	1.500	1.500	.948	1.150	3.528	1.284	5.520	2.357
Feb.	3.728	2.664	1.468	2.988	1.604	3.220	1.512	1.650		3.118	3.760	2,500	1.254	2.984	1.828	4.354	3,458	2.631
March	0.860	2.589	2.037	2.450	1.060		—.180	—.444	2.000	3.566	4.584	1.154	1.440	4.132	2·830	2.190	5.026	2,283
April	1.972	1.333	3.060	1.134	—.500		1.982	—.994	1.368	1.738	1,270	1.818	2.864	—.944	2.318	—.170	3.154	1.663
May	0.860	2.496	1.364	2,164	1.114	2.976	2.702	3.366	—.948	5.136	3 022	5.574	—.300	3.966	3.538	2.702	1.188	2.653
June	3.120	3.534	1.802	2.626	1.400	2.160	2.078	3.083	2.208	1,676	3.018	1.698	2.076	1.734	2.762	4.044	2.072	2.417
July	6.233	1.105	3.170	3.663	1.970	2.267	2.012	2.152	2.980	2.812	4.415	3.208	2.354	3.100	6.888	6.468	2.134	3.349
August	3.221	1.728	2,890	2,918	2 904	2.512	3.828	4.656	4.082	3.508	1.058	1.932	4.490	4.646	1.184	5.507	1.088	3.109
Sep.	2.176	1.264	—.240	1.460	2.006	5.806	2.702	5.300	2.076	0.320	2.682	3.060	1.158	3.116	2.926	1.313	3.718	2.431
Oct.	6.126	1.398	6.023	1.269	1.944	2.216	5.648	—.760	5.091	5.270	4.942	5.304	5,368	3.182	4.236	—.604	3.292	3.628
Nov.	1.882	3.749	3.064	—.624	5.858	5.000	3.628	2.600	4.074	3.928	2.304	4.940	3.756	3.426	3.496	2.516	4.526	3.490
Dec.	3.592	3.958	1.492	4.064	7.284	2.710	2.208		6.234	3.528	1.+	1.140	5.332	5.126	4.051	5.260	0.390	3.585
Total	35.737	27.472	29.310	27.542	33.495		31.156			37.726	34.±	33.828	31.340	37,506	40.285	36.412	34.566	33.596

Now, by blending these 17 years with the 8 years before referred to, we obtain the mean monthly averages for 25 years, as under: namely,

	Inches.
January......	2.258
February...	2.507
March......	2.112
April........	1.915
May..........	2.698
June..........	2.206
July..........	3.400
August......	3.307
September..	2.984
October.....	3.734
November..	3.378
December..	3.369
Total......	33.868

I think there is reason to believe, that if we had the averages for a century, they would not be materially different from these, either in regard to the relative monthly quantities or to the annual quantity.

It may be proper, however, to observe, that the late Mr. George Walker, of Salford, gives an account of the rain at Manchester, for the 9 years immediately preceding the above period of 25 years, in the Memoirs, Vol. 4, page 585.—His observations have been incorporated with mine, so as to extend the period from a quarter to a third of a century; but I prefer having his results separate, for the following reasons. On a comparison of our results for 8 subsequent years, I found his average exceeded mine, by about 4 inches in the year. (See Memoirs, Vol. 5, page 668). On inspecting his gage, I had reason to think his mode of measuring the rain was not susceptible of sufficient accuracy, and suggested the same to him, with which he seemed to acquiesce. Besides this, the year 1792, (one of the eight), was a most remarkable one, in the north of England particularly. The annual depth exceeded the average amazingly; and it was occasioned by an excess, in two or three of the months chiefly. The rain at Kendal that year was nearly 85 inches; and it was nearly the same at Keswick. April produced 10 inches, September 11, and December 12 inches. Mr. Walker's rain in Manchester that year was $55\frac{1}{4}$ inches, which is far above the average; and nearly one half of this great quantity fell in three months, namely, May, September and December. The year 1789 was also unusually wet. These facts influence the annual and monthly averages of Mr. Walker materially, independently of any supposed error in the actual measurement.

Averages of Mr. G. Walker's account of Rain in Salford (Manchester), from 1786 *to* 1793 *inclusive.*

	Inch.	Incorporated with mine.
January	2.47	2.310
February	2.75	2.568
March	2.05	2.098
April	2.30	2.010
May	3.51	2.895
June	3.30	2.502
July	4.62	3.697
August	4.78	3.665
September	4.21	3.281
October	4.51	3.922
November	3.30	3.360
December	5.28	3.832
Total	43.08	36.140

Whether we consider the averages as deduced from Mr. Walker's observations, or from my own, or from the two united, the conclusion is equally obvious; namely, that the first six months of the year must be considered as *dry* months, and the last six months of the year as *wet* months; also, that April is the driest month in the year, and that the sixth after, or October, is the wettest, or that in which the most rain falls, in a long continued series of years, in the immediate neighbourhood of Manchester.

It would be interesting to enquire how far these conclusions apply, to Great Britain in general, or to Europe at large, or still more generally, to the Northern temperate zone.

In the 4th Vol. of the Society's Memoirs, page 576, is given an abstract or summary of Mr. Hutchinson's account of Rain, at Liverpool, for 18 successive years, namely, from 1775 to 1792 inclusive. The annual average is 34.4 inches. Every one of the first six months yielded less rain (on the average) than any one of the last six months of the year. March was the driest, and October the wettest month in the year.

In the same volume, page 580, there are given the results of 16 years' observations of the rain at Dumfries, by Mr. Copland, namely, from 1777 to 1793. The annual average was 37 inches. The driest month is April, and next to it March; the wettest is September, and next to it October; and each of the first six months of the year is drier than any one of the last six.

At Chatsworth, (Derbyshire) from the same volume, page 586, *et seq.* I deduce the following averages for 16 years, (1777 to 1792 inclusive,) as per table.

Here again we see that March is the driest, October the wettest, and all the former six months drier than any one of the latter.

By combining the 10 years' observations of Dr. Campbell, of Lancaster, (Memoirs, 4, page 264 and 591) we obtain similar results nearly. March is the driest, and August the wettest month at Lancaster: But ten years is too short a period to obtain true means.—I have the rain at Lancaster for a subsequent period of 10 years (1802—1811) furnished me by my friend John Ford jun. Esq. of Ellel; which likewise gives March for the driest, but October for the wettest month of the year.

In the Annales de Chimie & de Physique, (Vol. 8th—1818) there is an account of rain at Viviers, lat. 44° 29′ N. long. 2° 2′ E. of Paris, by M. Flaugerges: The monthly means for 40 years' observations (from 1777 to 1818,) are stated, from which it appears that February is the driest month in the year and October the wettest. The annual average is 34 inches (French). The year 1801 was the wettest in that period, yielding 48 inches (French), and 1779 was the driest, yielding 20 inches 7 lines. Viviers, which is in the S. E. of France, has however some essential differences from Great Britain in regard to rain. There the months of July and August are amongst the driest; the only months distinguished for heavy rain, are September, October, and November; whilst April and May yield each more than the monthly average.

I have collected the Royal Society's account of rain at London for 28 years, ending in 1806, and find the averages to stand as under; also those of Luke Howard, Esq. for a subsequent period of 12 years, ending with 1818, made in the vicinity of London.—These united are as per table.

The fall of rain at Kendal for 5 years (1788—1792) was published in my Meteorology; since that time the account has been continued by my brother for 18 years, with which he has favoured me; I have obtained also 2 years further from good authority, making in all, 25 years' rain. The monthly averages I have deduced as per table.

I deduced the average rain at Paris, from observations published in the Journal de Physique for the last 15 years, as per table.

The average rain at Glasgow for 17 years (1801—1818) was deduced from a paper in the Annals of Philosophy, Vol. 12, page 377.

MEAN MONTHLY AND ANNUAL QUANTITIES OF RAIN

At various places, being the averages for many years.

	Manchester. 33 years.	Liverpool. 18 years.	Chatsworth. 16 years.	Lancaster. 20 years.	Kendal. 25 years.	Dumfries: 16 years.	Glasgow. 17 years.	London. 40 years.	Paris. 15 years.	Viviers. 40 years.	General Average.
	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	F. Inch.	F. Inch.	Inch.
January...	2.310	2.177	2.196	3.461	5.299	3.095	1.595	1.464	1.228	2.477	2.530
February.	2.568	1.847	1.652	2.995	5.126	2.837	1.741	1.250	1.232	1.700	2.295
March....	2.098	1.523	1.322	1.753	3.151	2.164	1.184	1.172	1.190	1.927	1.748
April	2.010	2.104	2.078	2.180	2.986	2.017	—.979	1.279	1 185	2.686	1.950
May	2.895	2.573	2.118	2.460	3.480	2.568	1.641	1.636	1.767	2.931	2.407
June	2.502	2.816	2.286	2.512	2.722	2.974	1.343	1.738	1.697	2.562	2.315
July.......	3.697	3.663	3.006	4.140	4.959	3 256	2.303	2.448	1.800	1.882	3.115
August...	3.665	3.311	2.435	4 581	5.039	3.199	2.746	1.807	1.900	2.347	3.103
Septem...	3.281	3.654	2.289	3.751	4.874	4.350	1.617	1.842	1.550	4.140	3.135
October...	3.922	3.724	3.079	4.151	5.439	4.143	2.297	2.092	1.780	4.741	3.537
November	3.360	3.441	2.634	3.775	4.785	3.174	1.904	2.222	1.720	4.187	3.120
December	3.832	3.288	2.569	3.955	6.084	3.142	1.981	1.736	1.600	2.397	3.058
	36.140	34.118	27.664	39.714	53.944	36.919	21.331	20.686	18.649	33.977	

Observations on the Theory of Rain.

Every one must have noticed an obvious connection between heat and the vapour in the atmosphere. Heat promotes evaporation, and contributes to retain the vapour when in the atmosphere, and cold precipitates or condenses the vapour. But these facts do not explain the phenomenon of rain, which is as frequently attended with an increase as with a diminution of the temperature of the atmosphere.

The late Dr. Hutton, of Edinburgh, was, I conceive, the first person who published a correct notion of the cause of rain. (See Edin. Trans. Vol. 1 and 2, and Hutton's Dissertations, &c). Without deciding whether vapour be simply expanded by heat, and diffused through the atmosphere, or chemically combined with it, he maintained from the phenomena, that the quantity of vapour capable of entering into the air, increases in a greater ratio than the temperature; and hence he fairly infers, that whenever two volumes of air of different temperatures, are mixed together, each being previously saturated with vapour, a precipitation of a portion of vapour must ensue, in consequence of the *mean* temperature not being able to support the *mean* quantity of vapour.

This explanation may be well illustrated by contemplating a curve convex towards its axis, in which case the ordinates increase in a greater ratio than the abscissæ. The abscissæ represent temperature, and the ordinates the quantity of steam which the corresponding temperatures are capable of retaining.

In 1793 I published my Meteorological Observations and Essays, a few years after this theory of rain had been made known; as far as I was then acquainted with it from one of the Reviews, it appeared the most plausible of any I had seen; but on looking at my remarks, it is evident I had not been made acquainted with its distinguishing feature, and that on which its excellence depends, namely, a higher *solvent power* (if it may be so called) in the air, than what is proportionate to the increase of temperature; and that the precipitation of vapour in the form of clouds and rain is occasioned, not by mere cold, but a mixture of comparatively warm and cold air.

At the time of my publication of the Essay on Rain, &c. I had a strong bias to the opinion, that the steam or vapour in the atmosphere exists in a state of combination with heat, but without any chemical union with the elements of the atmosphere; only it is subject to be wafted along mechanically by the great body of the atmosphere, in its ordinary currents. This opinion was founded and supported on the authority of the late M. Saussure in part; he having determined by direct experiment, that a cubic foot of dry air of the temperature 66° would imbibe 12 grains of water for its saturation. Now, from experiments on the boiling of water in vacuo, I was persuaded that this quantity of vapour was nearly what would fill a cubic foot of empty space, in the temperature of 66°; and by analogy, I concluded that the quantity of steam necessary to saturate any given volume of air, at any temperature, was the same that would be requisite to fill an equal void space at the same temperature. This reasoning was of course hypothetical at that time, and unsupported by any direct experiment.

In 1801 a series of essays of mine were read before the society, and subsequently published in the 5th Vol. of Memoirs; one object of experimental enquiry was, whether steam of any kind was the same in quantity in air and in a vacuum, all other circumstances being the same. The result was decidedly for the affirmative.

Another object was to ascertain the true force of steam in all atmospheric temperatures. This was clearly proved to be progressively increasing with the temperature, as Dr. Hutton had rightly conjectured. Indeed, with a slight modification of the thermometrical scale, the temperature is an *arithmetical* progression, and the force of steam a *geometrical* one. Hence the curve shewing the force of steam, is what mathematicians call the *Logarithmic*, one remarkably convex to its axis.

The cause of rain, therefore, is now, I consider, no longer an object of doubt. If two masses of air of unequal temperatures, by the ordinary currents of the winds, are intermixed, when saturated with vapour, a precipitation ensues. If the masses are under saturation, then less precipitation takes place, or none at all, according to the degree. Also the warmer the air, the greater is the quantity of vapour precipitated in like circumstances, as is evident to any one, on inspecting the logarithmic curve, or on considering that the increments of a geometrical progression, are in proportion to the terms. Hence the reason why rains are heavier in summer than winter, and in warm countries than in cold.

We may now enquire into the cause why less rain falls in the first six months of the year than in the last six months. The whole quantity of water in the atmosphere in January, is usually about 3 inches, as appears from the dew point, which is then about 32°. Now, the force of vapour at that temperature is .2 of an inch of mercury, which is equal to 2.8 or 3 inches of water. The dew point in July, is usually about 58 or 59°, corresponding to .5 of an inch of mercury, which is equal to 7 inches of water; the difference is 4 inches of water, which the atmosphere then contains more than in the former month. Hence, supposing the usual intermixture of currents of air in both the intervening periods to be the same, the rain ought to be 4 inches less in the former period of the year than the average, and 4 inches more in the latter period, making a difference of 8 inches between the two periods, which nearly accords with the preceding observations.

In the preceding estimations of the whole quantity of water in the incumbent atmosphere of any place, I take for granted, that an atmosphere of steam is blended with the general atmosphere throughout, in the same vertual column, and subject to the common law of rarefaction in ascending. This is a view of the aqueous atmosphere, which no one seems to have entertained but myself. I have been making experiments almost annually on the subject since 1802, on the mountains in the North of England, and particularly on Helvellyn. These experiments have been materially facilitated of late years by masses of snow, which have been found near the summit, in the month of July; but it has often happened, that the cold springs of water near the summit, have been adequate. By one or other of these the dew-point of the air may be found at any required elevation on the mountain, and the law by which it is regulated in the ascent, may be investigated.—On some future occasion, I intend to draw up a memoir on this subject. In the mean time I may observe, that all the phenomena concur in exhibiting the same variation of density in the aqueous vapour atmosphere in its present mixed state, as would no doubt be observed in an atmosphere of pure steam of equal density.

INDEX.

A

ANALYSIS of spring and mineral Waters, remarks on, 52.
Alphabetical characters, origin of, 77.
Assyrian Monarchy, scriptural and profane accounts of, 298.

B

Bones calcined, components of, 5.
Black-lead mine, Borrowdale, account of, 168.

C

Carbonate of ammonia, properties of, 23.
CREIGHTON, Mr. Henry, on the refractive power of muriatic acid, 70.
Cotton trade, rise and progress of in Great-Britain, 115.
Cog or Toothed wheels, new system of, 138.
CARBUTT, Edward, M. D. on the signs of ideas. 241.
Callous tumour, observations on, 275.
Coal gas, experiments on, 391.

D

Deoxidation of silver and copper in remarkable circumstances, 271.
DALTON, Mr. John, on phosphoric acid and phosphates, 1—on carbonate of ammonia, 18—on the analysis of spring and mineral waters, 52—on sulphuric ether, 446—on meteorology, 483.

E

Ether sulphuric, memoir on, 446.

F

Floating Island in Derwent Lake, account of, 64.
Flexibility of mineral substances, 161.

G

Gough, Mr. John, on the laws of statical equilibrium, 381.

H

Henry, Thomas, F. R. S., &c. his memoirs of the late Charles White, Esq. F. R. S., 33.
——————— List of Papers furnished by him to the Society, 219.
Henry, William, M. D. F. R. S. his tribute to the memory of the late President of the Manchester Society, 204.
——————— On the gas from coal, 391.

I

Ideas, on the signs of, 241.

K

Kennedy, John, Esq., on the rise, &c. of the cotton trade, 115.
——————— Inquiry into the Poor Laws, 430.
Kenrick, Rev. John, A. M., on the different accounts of the Assyrian monarchy, 298.

L

Longmire, Mr. John B., on the flexibility of mineral substances, 161.
Lucas, Samuel, Esq., on the deoxidation of metals, 271.

M

Muriatic acid and water, refractive powers of, 70.

MOORE, John, jun. Esq., his agricultural remarks, &c., 179.
Meteorological Observations, &c., 483.

O

OTLEY, Mr. Jonathan, on the floating island in Derwent lake, 64—his account of the black-lead mine in Borrowdale, 168.

P

Phosphoric acid, procured from bones, 1.
Phosphates, proportions of, 13.
PARKES, Samuel, F. L. S., on the manufacture of Tin-plate, 347.
Poor-Laws, on their effects on society, 430.

R

Rainbow, white solar, account of, 176.
Remarks, agricultural, &c., in Westmoreland and Cumberland, 179.

S

Subcarbonate of ammonia, properties of, 29.
Spinning Jenny invented, 1767, 119.
SMETHURST, Rev. R., his account of a white solar rainbow, 176.
Statical Equilibrium, Laws of, analytically investigated, 381.

T

Tests for the analysis of mineral waters, 55, &c.
TURNER, Rev. William, jun. A. M., on the origin of alphabetical characters, 77.
Tin-plate, on the manufacture of, 347.

W

WHITE, Charles, Esq., F. R. S., Memoirs of, 33.
WHITE, Mr. James, on cog or toothed wheels, 138.
WOOD, Mr. Kinder, on the callous tumour, 275.