CHAPTER II.

HIS EPOCH OF GREATEST FERTILITY.

THE work on meteorology was published in September, 1793. Dalton had come to Manchester in the spring of that year. He was now twenty-seven years of age. The removal to an active town seems to have satisfied his cravings for a larger sphere of labour which were forcing him from his attachment to his neighbourhood. He was self-taught, a raw countryman, in many respects rather rough in his acquired habits, although of a naturally gentle disposition. Such a distance from active life would have made many men idle, such a sudden entrance into it has often the same effect on others. Neither seemed to affect him, there was little change of habit, he was still in the streets of Manchester as on the hills of Cumberland, the active observer and thinker. On October 3rd, 1794, he first appears as a member of the Literary and Philosophical Society of Manchester, having been proposed by Thomas Henry, Dr. Percival, and Robert Owen, the veteran enthusiast who would willingly compel all mankind to be reformed by his simple formula. On the 31st, he read his first paper to the society, an event to him of great importance, greatly influencing all his future life, as he soon after became the representative of that body, continuing so for the remainder of his life.

This paper was entitled " Extraordinary Facts relating to the Vision of Colours."* He says there, p. 30,

" It may be proper to observe, that I am shortsighted. Concave glasses of about five inches focus suit me best. I can see distinctly at a proper distance; and am seldom hurt by too much or too little light; nor yet with long application."

* Memoirs of the Philosophical Society of Manchester. Vol. V., p 28.

"I found that persons in general distinguish six kinds of colour in the solar image; namely, red, orange, yellow, green, blue, and purple. To me it is quite otherwise; I see only two, or at most three, distinctions; these I should call yellow and blue, or yellow, blue, and purple. * * * * My yellow comprehends the red, orange, yellow, and green of others, and my blue and purple coincide with theirs."

He sums up the peculiarities of the vision of himself and others who have been found similarily affected thus; p. 40.

"1. In the solar spectrum three colours appear—yellow, blue, and purple. The two former make a contrast; the two latter seem to differ more in degree than in kind.

2. *Pink* appears, by day light, to be sky-blue a little faded; by candle light it assumes an orange or yellowish appearance, which forms a strong contrast to blue.

3. *Crimson* appears a muddy blue by day; and crimson woollen yarn is much the same as dark blue.

4. *Red and scarlet* have a more vivid and flaming appearance by candle light than by day light.

5. There is not much difference in colour between a stick of red sealing wax and grass, by day.

6. Dark green woollen cloth seems a muddy red, much darker than grass, and of a very different colour.

7. The colour of a florid complexion is dusky blue.

8. Coats, gowns, &c., appear to us frequently to be badly matched with linings, when others say they are not. On the other hand, we should match crimsons with claret or mud; pinks with light blues; browns with reds; and drabs with greens.

9. In all points where we differ from other persons, the difference is much less by candle light than by day light."

He found several persons having the same peculiarity of vision, and says (p. 43), "It appears, therefore, almost beyond a doubt, that one of the humours of my eye, and of the eyes of my fellows, is a coloured medium, probably some modification of blue."*

Although this paper was an observation on himself, it is in reality a discovery; the facts had not been arranged before he arranged them, and found out other persons similarly situated. A peculiar keenness of reasoning was needed to find it out, as we must remember that with such persons there is little community of feeling on colour, and scarcely a mode of judging whether they see any colours exactly as the normal eye does. It would probably explain many strange occurrences if we were to consider that there are really persons in the world who see all crimsons as "dark blue" or "a muddy blue," and who would "match crimsons with claret or mud; pinks with light blues; browns with reds; and drabs with greens;" who see the healthful tints of a florid complexion to be like "dilute black ink on white paper," or "a dull opake blackish blue, upon a white ground." How many strange mistakes and visions might be accounted for by this defect of sight. A fair face with glowing veins would be to Dalton as a corrupting corpse. But it may be said that custom would make all appear as well to him as to others; no, it cannot be so: a defect must constantly carry with it the consequences of a defect, and in this case the established difference which nature has made between life and death, beauty and horror, was hidden from the eye, and therefore to a great extent must have been concealed from the intellect. To this cause partly we may refer that want of fine sensibility to external things which peculiarly marked his scientific as well as social life.

Dr. Whewell has called such persons *idiopts*, because their vision is peculiar; this is not sufficiently characteristic, and

* Mr. J. A. Ransome, who examined the eye after death, found nothing whatever to account for the peculiarity of vision. Certainly colours appeared as usual through it. He believed that the cause was a deficient sensorial or receptive power.

as has been remarked sounds badly, Sir John Herschel having changed it to Dichromic vision, believing that one of the three colours is lost to the eye entirely. Such a vision there seems to be, but this extent has not been observed in any instances, by Dr. George Wilson, who thinks that there is no colour quite lost, although the power of perceiving be feeble, and he names it *Chromato-Pseudopsis*, or a false vision of colours. This he has translated by Sir D. Brewster's term, colour-blindness, which appears much too strong when we consider that some colours are well seen, and others seen in part. It seems, in fact, to be an imperfection in the power of distinguishing colours, which may exist to any extent, either very slightly, as is seen in every-day life, where, for example, among the many workpeople in a large mill, only a few are found fit for arranging yarns with accuracy. A nice perception of colour is there a valuable gift, and is paid for accordingly. Or it may occur decidedly defective, as with Dr. Dalton and others. Dalton's brother had the same defect, and one or two others in the neighbourhood of Eaglesfield, of whom I have lately heard. It is probable that there are many gradations, beginning with deficient colour sight and ending in Dichromic, or perhaps Monochromic or Achromic vision, or true colour blindness. Dr. Wilson well remarks, that Daltonism, under which it has been known, is not a proper name for the peculiarity, as it connects his name with a defect. Indeed few eyes are found equal to Dalton's, if we judge of them by their results. Dr. Wilson has made the remarkable discovery that this defect may almost be called common.

Dalton remained without giving anything to the public until 1799. In the College his order showed itself in the careful list of students and their lessons, still remaining. Possibly his duties occupied too much of his time to allow of experiment, but he comes out so suddenly after that as physikist and chemist, that his time must have been spent in suitable studies. On March 1st, 1799, he read to the Literary and Philosophical Society* "Experiments and observations to determine whether the quantity of rain and dew is equal to the quantity of water carried off by the rivers and raised by evaporation; with an inquiry into the origin of springs." In this he treats,—

"1. Of the quantity of rain and dew.
2. Of the quantity of water that flows into the sea.
3. Of the quantity of water raised by evaporation.
4. Of the origin of springs."

The first three are accompanied by experiments, but there is a looseness in the calculations which renders the paper rather like a sketch of the subject. He, however, collects a great deal of information as to the annual fall of rain in various places, and in a note explains clearly, as before alluded to, his ideas as to the state of aqueous vapour in the air. The looseness of expression is not at all times with him an indication of want of decision, but his peculiar style of writing, as if every one knew the subject, and were ready to draw out his reasoning into all its details, as soon as expressed. His experiments, begun with the hand, seem often finished with the head, so rapidly are his conclusions come to, and the natural law established in his mind. Even now we can add little to the relation between evaporation, rain, and dew, and on the origin of springs he is clear, quick, and decisive, saying that they come from the rain. This subject had been much disputed; filtration from the sea having been a favourite method of obtaining the water, as well as subterranean reservoirs like those of Father Kircher, who shows them in engravings continually boiling out from the centre of the earth. Dalton was not the first to suggest the explanation, of course, but the subject was sufficiently uncertain to call for elucidation. On April 12th,

* Memoirs of the Literary and Philosophical Society of Manchester. Vol. V., p. 346.

1799, he read a paper entitled "Experiments and observations on the power of fluids to conduct heat, with reference to Count Rumford's seventh essay on the same subject."*

He seems evidently to have made up his mind at once that Count Rumford had drawn a wrong conclusion from his premises, and we see in the reasoning much minute ingenuity and acuteness. As an example of his mode of experimenting and reasoning, the following may be given, p. 381.

"Exp. 3. Took an ale glass of a conical figure, 2½ inches in diameter, and 3 inches deep; filled it with water that had been standing in the room, and consequently of the temperature of the air nearly. Put the bulb of a thermometer to the bottom of the glass, the scale being out of the water; then having marked the temperature, I put the red-hot tip of a poker half-an-inch deep into the water, holding it there steadily about half-a-minute; and as soon as it was withdrawn, I dipped the bulb of a sensible thermometer about ¼ inch, when it rose in a few seconds to 180.°

TEMPERATURE.

Time.	At Top.	Middle.	Bottom.
Before the poker was immersed	...	...	47°
—	180°	—	47°
5 min.	100°	60°	47½°
20 ,,	70°	60°	49°
1 hour	55°	—	52°"

After other experiments he says, p. 385, "We must conclude, therefore, that the quick circulation of heat in water over a fire, &c., is owing *principally* to the internal motion excited by an alteration of specific gravity; but not *solely* to that cause, as Count Rumford has inferred."

A very simple and ingenious experiment is related on the same page. He mixed hot and cold water, stirred for half-a-minute, and tried if the upper part became hotter than the lower, it

* Same vol., p. 373.

did not do so, on which he says, "If the particles of water during the agitation had not actually communicated their heat, the hot ones ought to have risen to the top, and the cold ones subsided, so as to have made a material difference in the temperature." This shows, that even at that period he was accustomed to think habitually of matter as decidedly atomic in its constitution.

On the theoretical conclusion to be drawn here, we find his genius taking the lead; he is accurate in spite of the rudeness of his experiments. He concludes that water conducts heat a little, and that the expansion of water is the same both above and below the point of maximum density. But when he comes to determine the precise place at which that point is found, as it is a matter of experiment, and cannot be got by the mind only, he is at fault; in subsequent experiments learning to become accurate.

He seems to have lowered the point to 36°, and afterwards considered it 38°, the point now apparently fixed on is 39°, or 39.101. (Playfair and Joule.) Dr. Hope's experiments gave it as between 39½ and 40 degrees. In this investigation Dalton's mind again analyses itself, dividing to great clearness of conception on the one side, and carelessness of minute observation on the other.

In 1830 on reading over some old letters which he was arranging, he found one from Dr. Hope, saying, "notwithstanding the caution you gave me, I venture to publish my pamphlet on the contraction of water by heat," Dr. Dalton said, "aye, he had the advantage of me there, but not so much as it appeared at first sight."

In this paper he makes an observation on the power of capillary tubes to prevent the freezing of water, a circumstance which has not been thoroughly inquired into, nor the cause assigned its proper place.

In May, 1800, Dalton was elected secretary of the Literary and Philosophical Society of Manchester, in the place of Dr. William Henry, and having as his colleague Dr. Hull. This office he retained until the year 1808, when he was made vice-president in the room of Dr. Roget, who then lived in Manchester. Soon after, on June 27th, he read to that Society, "Experiments and observations on the heat and cold produced by the mechanical condensation and rarefaction of air." * Here by well devised experiments he endeavoured to shew what however had been before held by Lambert, Saussure, and Pictet, "that the capacity of a vacuum for heat is less than an equal volume of atmospheric air, and that the denser air is, the less is its capacity for heat," indicating a mode of ascertaining "the absolute capacity of a vacuum for heat," and "likewise the capacity of the different gases for heat by a method wholly new." An important result of these experiments was, that the temperature of air mechanically compressed to one-half its volume was raised 50°. This, although much underrated, was the first numerical result of importance on this subject. p. 524.

In this paper we find that he had ascertained that gases expand 1-10th of their volume nearly for 50° of heat, or nearly 1-500th of their bulk, a subject which he treated of at a later period.

Whilst engaged in teaching at the academy in Manchester, his classes or scholars seem to have been as miscellaneous as they had been at Kendal. We may infer this from the appearance of an English grammar, the preface of which is dated March 10th, 1801. He seems to have looked on this as a recreation, but he never afterwards appears to have had recourse to literature for amusement or for variety. As he has never been looked on as a grammarian, it may be of some interest to see what his views on such points were.

He says, p. 8. "It may be taken as an axiom that all time or duration in the strict sense of the terms, is either

* Mem., Vol. V., p. 515.

past or *future.* But for the purposes of speech we must have a present time of some duration, which must necessarily be comprised of a portion of past and a portion of future, having the present, *now* or *instant*, as a boundary between them. Its length may be what we please to make it.

" Grammatically speaking therefore, there are three times, present, past, and future; though strictly and mathematically speaking, we can admit only two, past and future. Moreover we find it expedient in the course of conversation, not only to mention actions as whole and entire, but also their commencement, their being in a passing or middle state, and their termination; accordingly our language furnishes us with four forms of speech for each of the times or tenses, which are exemplified in their proper place, both for the active and passive verb, with appropriate names to them."

His active verb is given thus :—

Indicative mood.

Present time.

I serve, &c.

Beginning present.

I am about to serve, &c.

Middle present.

I am serving, &c.

Ending present.

I have served or been serving, &c.

Past time.

I served, &c.

Beginning past.

I was about to serve, &c.

Middle past.

I was serving, &c.

Ending past.

I had served or been serving, &c.

Future or present time.

I shall, will, may, can *or* must serve, &c.

Beginning future or present.
I shall, will, may, can *or* must be about to serve, &c.
Middle future or present.
I shall, will, may, can *or* must be serving, &c.
Ending future or present.
I shall, will, may, can, *or* must have served *or* have been serving.

In grammar it is difficult to have absolutely new ideas, the subject has been so belaboured, and at the same time it is not easy to keep rigidly to any system proposed, so many of the treatises have wanted clearness. We may see that in that department Dalton was inclined to be an innovator, although he has not the honor of being a discoverer, indeed his mind was much too rigid to be inclined to yield to all the flexions and variations of a subject so bordering on metaphysics as grammar. Horne Tooke is the writer which he most admired on that subject, using sometimes his very words, although not in all things following him. But innovators are more dangerous in grammar, and are less easily received, than in the physical sciences which have no ancestry.

Some years afterwards he went into the shop of the publisher of his grammar, and asked for a copy; he was told they had none, but insisting on it, a parcel of them was found in some dusty corner, very few having ever been sold. Still he assures us that a Sheffield man had published it some years later as his own, with some additions.

In October of the same year he read a paper which occupied three evenings of the Literary and Philosophical Society. It is composed of four "Experimental essays on the constitution of mixed gases; on the force of steam or vapour from water and other liquids in different temperatures, both in a Torricellian vacuum and in air; on evaporation; and on the expansion of gases by heat." (Mem. Vol. V., p. 535.)

The four laws given by him are—

"1. When two elastic fluids, denoted by *A* and *B*, are mixed together, there is no mutual repulsion amongst their particles; that is, the particles of *A* do not repel those of *B*, as they do one another. Consequently, the pressure or whole weight upon any one particle arises solely from those of its own kind.

"2. The force of steam from all liquids is the same, at equal distances above or below the several temperatures at which they boil in the open air; and that force is the same under any pressure of another elastic fluid as it is in vacuo. Thus the force of *aqueous* vapour of 212° is equal to 30 inches of mercury; at 30° below, or 182°, it is of half that force; and at 40° above, or 252°, it is of double the force; so likewise the vapour from sulphuric ether, which boils at 102°, then supporting 30 inches of mercury, at 30° below that temperature it has half the force, and at 40° above it, double the force; and so in other liquids. Moreover the force of aqueous vapour of 60° is nearly equal to ½ inch of mercury, when admitted into a Torricellian vacuum; and water of the same temperature, confined with perfectly dry air, increases the elasticity to just the same amount.

"3. The quantity of any liquid evaporated in the open air is directly as the force of steam from such liquid at its temperature, all other circumstances being the same.

"4. All elastic fluids expand the same quantity by heat; and this expansion is very nearly in the same equable way as that of mercury; at least from 32° to 212°. It seems probable the expansion of each particle of the same fluid, or its sphere of influence, is directly as the quantity of heat combined with it; and consequently the expansion of the fluid as the cube of the temperature, reckoned from the point of total privation."

The first law accounts for a diffusion of gases to a great extent, but not entirely. It would result from it, if not qualified, that there would be a diminishing quantity of oxygen, which is the heaviest gas in the atmosphere, according as the height increased. This was Dalton's opinion, but it has not turned out to be the case. This law was much assailed, and at the same time much misunderstood. The objection that vapour did not rise so rapidly in air as in a vacuum seemed to him a strong one, which he did not quite get over, but considered it as presenting the same difficulty to all theories of the solution of water in air.

The law was stated too broadly, it did not even allow room for the impenetrability of matter to have its due place, and many persons supposed it to mean that a space filled with one gas, might be filled with an equal quantity of another.

He subsequently stated these two propositions in the following form, which he published in the second edition of his "New system of chemistry," when, after many years, he reviewed himself and his reviewers. p. 191, Part I., 1842.

"1. The diffusion of gases through each other is effected by means of the repulsion belonging to the homogeneous particles; or to that principle which is always energetic to produce the dilatation of the gas.

"2. When any two or more mixed gases acquire an equilibrium, the elastic energy of each against the surface of the vessel or of any liquid, is precisely the same as if it were the only gas present occupying the whole space, and all the rest were withdrawn."

There is no doubt that the law had been hastily expressed: explaining some points, it contradicted others. The phenomenon of the mixing of gases is easily explained, if we admit the constant intestine motion of the particles to be a necessary condition of the existence of a body in a gaseous state. (See a paper "On the changes of temperature produced by the rarefaction and condensation of air," by J. P. Joule. Phil. Magaz., May, 1845.)

The second essay is on the force of steam or vapour. He gives a long table of the force of aqueous vapour at different temperatures, from 40° to 325°. Between 32° and 312° the numbers are given from experiment; above and below these limits the numbers are from calculation. These tables were afterwards modified by himself, and others have also reduced them to greater accuracy. He objects to the tables from water and alcohol given by M. Betancourt in 1790, and to that in the Encyclopædia Britannica, because the authors had assumed the force of that from water, at 32°, to be nothing. This constituted one of the steps which the subject made in its rather retarded progress.

He gives a series of experiments on the power of vapour from liquids, supporting his conclusions by experiments on ether, alcohol, water of ammonia, solution of muriate of lime, mercury, and sulphuric acid, and says "That the variation of the force of vapour from all liquids is the same for the same variation of temperature, reckoning from vapour of any given force; thus assuming a force equal to 30 inches of mercury as the standard, it being the force of vapour from any liquid boiling in the open air, we find aqueous vapour loses half its force by a diminution of 30° of temperature; so does the vapour of any other liquid lose half its force by diminishing its temperature 30° below that in which it boils, and the like for any other increment or decrement of heat." p. 564.

When speaking of vapour of water in air, he says "the results of all agree in one general rule or principle, which is this; let 1 represent the space occupied by any kind of air of a given temperature, and free from moisture; $p =$ the given pressure upon it in inches of mercury; $= f =$ the force of vapour from any liquid in that temperature in vacuo; then the liquid being admitted to the air, an expansion ensues, and the space occupied by the air becomes immediately and in a short time $= 1 + \frac{f}{p-f}$; or which is the same thing $= \frac{p}{p-f}$. Thus in water for instance, let $p = 30$ inches $f = 15$ inches to the given temperature 180°. Then $\frac{p}{p-f} = \frac{30}{30-15} = 2$ for the space; or the air becomes of twice the bulk." p. 572. "In short, in all cases the vapour arises to a certain force according to temperature, and the air adjusts the equilibrium by expanding and contracting as may be required."

"The notion of a chemical affinity subsisting between the gases and vapours of different kinds cannot at all be reconciled to these phenomena." p. 574.

This notion of chemical affinity holding the gases in solution had begun to die out.

In essay third, "*On evaporation*," he concludes that the quantity of any liquid evaporated in the open air is directly as the force of steam from such liquid at its temperature, all other circumstances being the same. He adds a "table shewing the force of vapour, and the full evaporating force of every degree of temperature from 20° to 85°, expressed in grains of water that would be raised per minute from a vessel of six inches in diameter, supposing there were no vapour already in the atmosphere." p. 585. He obtained the evaporation from a surface when the air was still and when in motion. He adds also rules to find the amount of water that can be evaporated from a given surface when the temperature of the air is given, and the condensing point, and to find the force of the aqueous vapour.

The fourth essay on the expansion of elastic fluids by heat proves the law already stated.

The position of the question when he took up the subject may best be explained by himself, he says, p. 595, "The principal occasion of this essay is another on the same subject by Messrs. de Morveau and du Vernois, in the first vol. of the Annales de Chimie. It appearing to them that the results of the experiments of De Luc, Col. Roy, de Saussure, Priestley, Vandermonde, Berthollet, and Monge, did not sufficiently accord with each other; and that it would be of importance to determine not only the whole expansion of each gas from two distant points, such as the freezing and boiling, but likewise whether that expansion be uniform in every part of the scale, they instituted a set of experiments expressly for those purposes. The result of which was; that betwixt the temperatures of 32° and 212°, the whole expansion of one gas differs much from that of another, it being in one instance about 4-10ths of the original, and in others, more than twelve times that expansion; and that the expansion is much more for a given number of degrees in the higher than in the lower part of the scale. These conclusions were so extremely discordant with and even contradictory to those of others, that I could not but suspect some great fallacy in them, and found it in reality to be the fact; I have no doubt it arose from the want of due care to keep the apparatus and materials free from moisture."

After giving his experiments on air, hydrogen, oxygen, carbonic and nitrous gas, in which "the small differences never exceeded six or eight parts, on the whole 345," he adds, "Upon the whole, therefore, I see no sufficient reason why we may not conclude that all elastic fluids under the same pressure expand equally by heat, and that for any given expansion of mercury, the corresponding expansion of air is proportionally something less, the higher the temperature."

"This remarkable fact that all elastic fluids expand the same quantity in the same circumstances, plainly shews that the expansion depends solely upon heat; whereas the expansion in solid and liquid bodies seems to depend on an adjustment of the two opposite forces of heat and chemical affinity, the one a constant force in the same temperature, the other a variable one, according to the nature of the body; hence the unequal expansion of such bodies. It seems, therefore, that general laws respecting the absolute quantity and the nature of heat, are more likely to be derived from elastic fluids than from other substances." There is an admirable clear-sightedness in his short and rapid conclusions. The same law of equal expansion of gases was published six months later by Gay Lussac, and is often called by his name. Dr. Ure says the experiments were made by Gay Lussac with much more care and exactness, but the newest results obtained by Regnault by no means speak so in favour of Gay Lussac. The difference between his results and Dalton's were only trifling. Gay Lussac gave the expansion per degree at 480, Dalton 483, Regnault 491. In this country we have generally used Gay Lussac's for no sufficient reason. On the Continent Dalton has almost been entirely deprived of his merit, and is not even mentioned in connection with it in many French and German works: but such circumstances are unfortunately of constant occurrence. It is difficult to find the reason of this, but it happens so often that our countrymen are quite omitted in their works, that it must in a great measure arise from their neglect of our literature. This certainly must be the cause, as we find that both French and Germans of high name can treat latent heat without even mentioning the name of Black, whose claims are not even disputed; this last occurs even with the very systematic Gmelin. We can readily imagine how some of the other papers of Dalton have been overlooked as merely additions to a subject, whereas he who gives the polish and establishes the law has been allowed the entire credit. They were certainly put within the reach of inquirists, as he says in a letter quoted in Dr. Henry's life of him, p. 50. "My lately published essays on gases, &c., together with the more recent ones read at our society, and of which I gave the result in my late lectures, have drawn the attention of most of the philosophers of Europe. They are busy with them at London, Edinburgh, Paris, and in various parts of Germany, some maintaining one side and some another. The truth will surely out at last." Although not alluding specially to the last mentioned memoir, this letter alludes to his investigations generally, which had been everywhere discussed.

On November 12th, 1802, he read to the Literary and Philosophical Society an "Experimental inquiry into the proportion of the several gases or elastic fluids constituting the atmosphere."*

These he made by weight, p. 257.

Azotic gas	75.55
Oxygenous gas	23.32
Aqueous vapour	1.03
Carbonic acid gas	10
	100.00

In another place we find, by bulk	79 azote.
	21 oxygen.

In describing his Eudiometric process he has a few observations of great importance, indications of the direction in which he was moving, but given in such a way as to lead us to the conclusion that he had not yet seen their value; teaching us also that an idea of definite proportions may exist without any distinct nature of the completeness of the law of equivalents as it stands. At page 249 he says,—

"2. If 100 measures of common air be put to 36 of pure nitrous gas in a tube 3-10ths of an inch wide and 5 inches long, after a few minutes the whole will be reduced to 79 or 80 measures, and exhibit no signs of either oxygenous or nitrous gas.

"3. If 100 measures of common air be admitted to 72 of nitrous gas in a wide vessel over water, such as to form a thin stratum of air, and an immediate momentary agitation be used, there will, as before, be found 79 or 80 measures of pure azotic gas for a residuum.

"4. If in the last experiment, less than 72 measures of nitrous gas be used, there will be a residuum containing oxy-

* 1st vol. of Memoirs, new series, p. 244.

genous gas ; if more, then some residuary nitrous gas will be found.

"These facts clearly point out the theory of the process: the elements of oxygen may combine with a certain portion of nitrous gas, or with twice that portion, but with no intermediate quantity. In the former case *nitric* acid is the result, in the latter *nitrous* acid; but as both these may be formed at the same time, one part of the oxygen going to one of nitrous gas and another to two, the quantity of nitrous gas absorbed should be variable; from 36 to 72 per cent. for common air. This is the principal cause of that diversity which has so much appeared in the results of chemists on this subject."

In the paper on the expansion of elastic fluids, he had already, in a plate, shown that he was accustomed to view gases as composed of definite particles, having drawn each with a different form.

Immediately after this, January 28th, 1803, he read, an inquiry "On the tendency of elastic fluids to diffusion through each other."* This subject was first begun by Priestley. The memoir which he has written on the transmission of gases through porous vessels, entitled "Experiments relating to the seeming conversion of water into air," is certainly one of the most beautiful specimens of investigation that can anywhere be found. He there establishes the fact, that through porous vessels, gases pass one way, vapour of water and other liquids another; and observed, that the mercury in one experiment had risen 3½ inches above the level on the outside. He afterwards found that what could take place with "air and water, will be done with any two kinds of airs."

He failed, however, to make the next step, having said that it is probable "that if two kinds of air of very different specific gravities, were put into the same vessel with very great care, they might continue separate," although his

* Memoirs, Vol. I., New Series, p. 259.

own experiments justified a different opinion. Dalton took the subject up at this stage, and says the result "establishes this remarkable fact, that a lighter elastic fluid cannot rest upon a heavier, as is the case with liquids; but they are constantly active in diffusing themselves through each other until an equal equilibrium is effected; and that without any regard to their specific gravity, except so far as it accelerates or retards the effect according to circumstances."*

"The only apparatus found necessary, was a few phials and tubes with perforated corks; the tube mostly used was one 10 inches long, and of 1-20th inch bore; in some cases a tube of 30 inches in length, and 1-3rd inch bore was used; the phials held the gases that were subjects of experiment, and the tube formed the connection." p. 261. This tube was often a piece of tobacco pipe.

He believes that this proves his theory of elastic fluids to be correct, that gases are as a vacuum to each other, and it no doubt does favour it, especially as he added that they might be obstructed as a stream of water by a stony bed. Still this very explanation takes away much of the original meaning, and any of his difficulties as to the mutual action of gases must be cleared by further experiments, as has been the case with the laws of diffusion which have already been shewn to us by Professor Graham. There is no doubt that Dalton's expression is an useful attempt to grasp a great difficulty, not yet grasped, we shall see him returning to it again in the next paper.

On October 21st, 1803, he read to the Literary and Philosophical Society, another investigation "On the absorption of gases by water and other liquids." p. 271., Vol. I., New Series.

In this he says, 2. "If a quantity of water freed from air be agitated in any kind of gas not chemically uniting with water, it will absorb its bulk of the gas, or otherwise a

* Page 260, Vol. I., New Series.

part of it, equal to some one of the following fractions, namely, 1-8th, 1-27th, 1-64th, 1-125th, these being the cubes of the reciprocals of the natural numbers 1, 2, 3, &c.;" This has not found general assent, nor can it flow from any known natural law; indeed if it were true it would not shew itself by the usual mode of experimenting, as we can readily imagine one part of the water having 1-4th, another 1-5th, both being distinct parts of the whole, but so mixed with each other in the water that no result is perceived.

4. "If a quantity of water free from air be agitated with a mixture of two or more gases, such as atmospheric air, the water will absorb portions of each gas the same as if they were presented to it separately in their proper density."

5. "If water impregnated with any one gas (as hydrogenous) be agitated with another gas equally absorbable (as azotic), there will apparently be no absorption of the latter gas; just as much gas being found after agitation as was introduced to the water; but upon examination the residuary gas will be found a mixture of the two, and the parts of each, in the water, will be exactly proportional to those out of the water."

"10. Pure distilled water, rain and spring water, contain nearly their due share of atmospheric air; if not, they quickly acquire that share by agitation in it, and lose any other gas they may be impregnated with. It is remarkable however that water by stagnation in certain circumstances loses part or all of its oxygen, notwithstanding its constant exposition to the atmosphere. This I have uniformly found to be the case in my large wooden pneumatic trough, containing about 8 gallons. * * * * The quantity of azotic gas is not materially diminished by stagnation, if at all." He has not here considered the action of the organic substances.

Theory of the absorption of gases by water. p. 283.

"1. All gases that enter into water and other liquids, by means of pressure, and are wholly disengaged again by the removal of that pressure, are mechanically mixed with the liquid, and not chemically combined with it."

He had already mentioned Dr. Henry's discovery, that the quantity of gas absorbed is as the density or pressure.

"2. Gases so mixed with water, &c., retain their elasticity or repulsive power amongst their own particles, just the same in the water as out of it, the intervening water having no other influence in this respect than a mere vacuum."

"3. Each gas is retained in water by the pressure of gas of its own kind incumbent on its surface abstractedly considered, no other gas with which it may be mixed having any permanent influence in this respect."

"4. When water has absorbed its bulk of carbonic acid gas, &c., the gas does not press on the water at all, but presses on the containing vessel just as if no water were in. When water has absorbed its proper quantity of oxygenous gas, &c., that is, 1-27th of its bulk, the exterior gas presses on the surface of the water with 26-27ths of its force, and on the internal gas with 1-27th of its force, which force presses upon the containing vessel, and not on the water. With azotic and hydrogenous gas the proportions are 63-64ths and 1-64th respectively. When water contains no gas, its surface must support the whole pressure of any gas admitted to it, till the gas has in part forced its way into the water."

"5. A particle of gas pressing on the surface of water is analogous to a single shot pressing upon the summit of a square pile of them. As the shot distributes its pressure equally amongst all the individuals forming the lowest stratum of the pile, so the particle of gas distributes its pressure equally amongst every successive horizontal stratum of particles of water downwards, till it reaches the sphere of influence of another particle of gas. For instance, let any gas press with a given force on the surface of water, and let the distance of the particles of gas from each other be to those of water as 10 to 1, then each particle of gas must divide its force equally amongst 100 particles of water, as follows: It exerts its immediate force upon 4 particles of water; those 4 press upon 9, the 9 upon 16, and so on according to the order of square numbers, till 100 particles of water have the force distributed amongst them; and in the same stratum each square of 100, having its incumbent particle of gas, the water below this stratum is uniformly pressed by the gas, and consequently has not its equilibrium disturbed by that pressure."

"When water has absorbed 1-27th of its bulk of any gas, the stratum of gas on the surface of the water presses with 26-27ths of its force on the water, and with 1-27th of its force on the uppermost stratum of gas in the water; the distance of the two strata of gas must be nearly 27 times the distance of the particles in the incumbent atmosphere, and 9 times the distance of the particles in the water. This comparatively great distance of the inner and outer atmosphere arises from the great repulsive power of the latter, on account of its superior density, or its presenting 9 particles of surface to the other 1. When 1-64th is absorbed, the distance of the atmospheres becomes 64 times the distance of two particles in the outer, or 16 times that of the inner. The annexed views of perpendicular and horizontal strata of gas in and out of water will sufficiently illustrate these positions."*

"7. An equilibrium between the outer and inner atmospheres can be established in no other circumstance than that of the distance of the particles of one atmosphere being the same or some multiple of that of the other; and it is probable the multiple cannot be more than 4. For in this case the distance of the inner and outer atmospheres is such as to make the perpendicular force of each particle of the former or those particles of the latter that are immediately subject to its influ-

* A plate accompanied this.

ence, physically speaking, equal; and the same may be observed of the small lateral force."

"8. The greatest difficulty attending the mechanical hypothesis arises from different gases observing different laws. Why does water not admit its bulk of every kind of gas alike? This question I have duly considered, and although I am not yet able to satisfy myself completely, I am nearly persuaded that the circumstance depends upon the weight and number of the ultimate particles of the several gases; those whose particles are lightest and single being least absorbable, and the others more, according as they increase in weight and complexity — (subsequent inquiry made him think this less probable). An inquiry into the relative weights of the ultimate particles of bodies is a subject, as far as I know, entirely new; I have lately been prosecuting this inquiry with remarkable success. The principle cannot be entered upon in this paper; but I shall just subjoin the results, as far as they appear to be ascertained by my experiments."

He then gives a list of *relative weights* of 21 substances, constituting the first attempt to form a table of atomic weights.

"Table of the relative weights of the ultimate particles of gaseous and other bodies.

Hydrogen	1
Azot	4.2
Carbone	4.3
Ammonia	5.2
Oxygen	5.5
Water	6.5
Phosphorus	7.2
Phosphuretted hydrogen	8.2
Nitrous gas	9.3
Ether	9.6
Gaseous oxide of carbone	9.8

Nitrous oxide	13.7
Sulphur	14.4
Nitric acid	15.2
Sulphuretted hydrogen	15.4
Carbonic acid	15.3
Alcohol	15.1
Sulphureous acid	19.9
Sulphuric acid	25.4
Carburetted hydrogen from stagnant water	6.3
Olefiant gas	5.3"

I have given as much as possible, in his own words, the most important points attended to by Dalton up to this date. It was not my intention to inquire into the particulars relating to the novelty of the views taken by him, except on the atomic theory, and have therefore purposely left out any such opinions as might require discussion; nor have I shewn in all cases where advancing science has differed from his results. Some things in the papers alluded to were bold and strikingly new, some things are improvements on the old, some are mere re-statements of the old, but all is done in a firm, clear, and determined manner, as by a master in the business, going to the real point of difficulty in every case, and at all times avoiding unimportant details or vain ornament. He drives on like a new settler, and clears the ground before him, leaving it rather rugged it is true; nevertheless it is resolutely cleared.