being always a point at the same distance on the other side of the plane of reference, and the horizontal distance between the two being the same as in the preceding case. This property is illustrated by Fig. 2.

It is obvious that the conjugate foci will occur not in pairs merely, but in sets of unlimited number ; that is to say, raised proceeding originally from any one point will converge in succession to an indefinite number of other points, which will be alternately on opposite sides of the plane of reference. As every point on the surface of an object will thus have its conjugates, we shall have a succession of images of the object. The first image will be upside down, the second erect, and so on alternately. They will be what are technically called "real" images, and will be precisely equal and similar (except as regards inversion) to the object itself. It is of course to be understood that the action here described is confined to one dimension only, resembling that of a cylindrical rather than of a spherical lens. Rays are bent to and from the plane of reference, but in no other direction. This theoretically simple case is so important for the light which it throws upon the possibilities of atmospheric refraction, that we shall examine some of its consequences a little further.

What will be the appearance presented to the eye of an observer in any given position ?

The case differs greatly from that of the images in ordinary optics, where the refracting instruments are glass lenses, and the eye sees the image by means of rays which travel in straight lines.

In the case now before us, the observer will in general see a virtual image, differing considerably, both in size and direction, not only from the object itself, but also from any one of the real images. The apparent direction of any point of the visible image is of course determined by drawing a tangent to the ray which enters the eye [*] (Figs. 6 and 7) ; and the visual angle, or, as we may call it, the apparent size of the object, will be the angle between two of these tangents. If the eye is a little distance (say a few feet) behind one of the real images, enormous magnification will be produced, for the image has the same linear height as the object, and is seen from a distance of a few feet, instead of from the real distance of the object, which we may suppose to be a few miles. We shall thus have enormous magnification of the vertical diameter of the object, while the horizontal diameter will of course be only of the natural size, since the rays have undergone no bending except up and down. An object whose breadth is equal to its height will thus be magnified into a tall column. Some appearances of this kind, copied from Scoresby's "Greenland," are represented in the first two figures of Plate II. The following is Scoresby's description ("Greenland," p. 96) :—

"Hummocks of ice assumed the forms of castles, obelisks, and spires, and the land presented extraordinary features. In some places the distant ice was so extremely irregular, and appeared so full of pinnacles, that it resembled a forest of naked trees ; in others it had the character of an extensive city crowded with churches, castles, and public edifices."

Again, on page 163 of the same work :—

"At one period the phenomenon was so universal that the space in which the ship navigated seemed to be one vast circular area, bounded by a mural precipice of great elevation, of basaltic ice."

The magnificent columns which constitute a portion of the wonders of the Fata Morgana, at the Straits of Messina, are in like manner to be attributed to vertical magnification. And an appearance of the same kind, known as "the merry dancers," is often seen by boatmen off the Giant's Causeway, in looking over the Skerries towards Portrush.

[*] The letter E, in all the figures, denotes the position of the observer's eye.

If we could have density distributed symmetrically round an axis, instead of on the two sides of a plane, we might of course have magnification without distortion. But we can scarcely conceive of any arrangement at all resembling this existing in the atmosphere.

It is further to be remarked, that the apparent distance of one of our columnar images from the observer's eye is an ambiguous quantity. If judged by left and right displacement, it is the real distance of the object. If judged by up and down displacement, it is much less, being approximately the distance of the real image.

(*To be continued.*)

SOME REMARKS ON DALTON'S FIRST TABLE OF ATOMIC WEIGHTS [*]

AS the Society is aware, the first table, containing the relative weights of the ultimate particles of gaseous and other bodies, was published as the eighth and last paragraph to a paper by Dalton on the absorption of gases by water and other liquids, read before this Society on Oct. 21, 1803, but not printed until the year 1805. There appears reason to believe that these numbers were obtained by Dalton after the date at which the paper was read, and that the paragraph in question was inserted at the time the paper was printed. The remarkable words with which he introduces this great principle give us but little clue to the methods which he employed for the determination of these first chemical constants, whilst in no subsequent publication, as in none of the papers which have come to light since his death, do we find any detailed explanation of how these actual numbers were arrived at. He says,[†] " I am nearly persuaded that the circumstance " (viz., that of the different solubilities of gases in water) " depends upon the weight and number of the ultimate particles of the several gases : those whose particles are lightest and single being less absorbable, and the others more, according as they increase in weight and complexity. An inquiry into the relative weights of the ultimate particles of bodies is a subject, as far as I know, entirely new. I have been lately 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."

Here follows the table of the relative weights of the atoms.

Table of the Relative Weights of the Ultimate Particles of Gaseous and other Bodies.

Hydrogen	1
Azot	4·2
Carbon	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 carbon	9·8
Nitrous oxide...	13·7
Sulphur	14·4
Nitric acid	15·2
Sulphuretted hydrogen	15·4
Carbonic acid...	15·3
Alcohol	15·1
Sulphurous acid	19·9
Sulphuric acid	25·4
• Carburetted hydrogen from stagnant water...	6·3
Olefiant gas	5·3

In the second part of his "New System of Chemical Philosophy," published in 1810, Dalton points out, under the description of each substance, the experimental evi-

dence upon which its composition is based, and explains, in some cases, how he arrived at the relative weights of the ultimate particles in question. Between the years 1805 and 1810, however, considerable changes had been made by Dalton in the numbers; the table found in the first part of the "New System" being not only much more extended, but, in many cases, the numbers differing altogether from those given in the first table published in 1805. It is therefore now, to a considerable extent, a matter of conjecture how Dalton obtained the first set of numbers; all we know is that it was mainly by the consideration of the composition of certain simple gaseous compounds of the elements that he arrived at his conclusions, and in order that we may form some idea of the data he employed, we must make use of the knowledge which chemists at that time (1803 5) possessed concerning the composition of the more simple compound gases.

As I can find no record of any explanation of these early numbers, I venture to bring the following attempt to trace their origin before the Society to whom we owe their publication.

The first point to ascertain, if possible, is how Dalton arrived at the relation between the atomic weights of hydrogen and oxygen given in the table as 1 to 5·5 (but altered to 1 to 7 in 1808). The composition of water by weight had been ascertained by the experiments of Cavendish and Lavoisier to be represented by the numbers 15 of hydrogen to 85 of oxygen, and this result was generally accepted by chemists at the time, amongst others doubtless by Dalton. Whether in those early days Dalton had actually repeated or confirmed these experiments appears improbable. At any rate, he formed the opinion that water was what he called a binary compound, *i.e.*, that it is made up of one atom of oxygen and one atom of hydrogen combined together. Hence, if he took the numbers 85 to 15 as giving the composition of water, the relation of hydrogen to oxygen would be 1 to 5·6, or nearly that which he adopted. It does not appear possible to explain why Dalton adopted 5·5 instead of 5·6 for oxygen; it may, perhaps, have been a mistake, as there are two evident mistakes in the table, viz., 13·7 for nitrous oxide instead of 13·9, and 9·3 for nitrous gas instead of 9·7.

Let us next endeavour to ascertain how he obtained the number 4·3 for carbon (altered to 5 in 1808 and to 5·4 later on). Lavoisier, in the autumn of 1783, had ascertained the composition of carbonic acid gas by heating a given weight of carbon with oxide of lead, and he came to the conclusion that this gas contained 28 parts by weight of carbon to 72 parts by weight of oxygen. Now Dalton not only was acquainted with the properties and composition of carbonic acid, but he was aware that Cruikshank had shown in 1800 that the only other known compound of carbon and oxygen, carbonic oxide gas, yields its own bulk of carbonic acid when mixed with oxygen and burnt; and also that Desormes * analysed both these gases, finding carbonic oxide to contain 44 of carbon to 56 of oxygen, whilst carbonic acid contained to 44 of carbon 112 of oxygen, being just double of that in the carbonic oxide. Dalton adds: "This most striking circumstance seems to have wholly escaped their notice." Hence Dalton assumed that one atom of carbon is united in the case of carbonic oxide with one atom of oxygen, whilst carbonic acid possessed the more complicated composition and contains two atoms of oxygen to one of carbon. Now, if carbonic acid contains carbon and oxygen in the proportion of 28 to 72, carbonic oxide must contain half as much oxygen, viz., 28 of carbon to 36 of oxygen; and assuming that the atomic weight of oxygen is 5·5, that of carbon must be

$$\frac{28 \times 5\cdot5}{36} = 4\cdot3.$$

Having thus arrived at the number 4·3 as the first

* Ann. der Chemie, tome 39, p. 38.

atomic weight of carbon, it is easy to see why Dalton gave 6·3 as the atomic weight of carburetted hydrogen from stagnant water, and 5·3 as that of olefiant gas. The one represents one atom of carbon to two of hydrogen, the other one of carbon to one of hydrogen; or, olefiant gas contains to equal quantities of carbon only half as much hydrogen as marsh gas. This conclusion doubtless expressed the results of Dalton's own experiments upon these two gases, which were made, as we know from himself, in the summer of the year 1804. He proved that neither of these gases contained anything besides carbon and hydrogen, and ascertained, by exploding with oxygen in a Volta's eudiometer, that if we reckon the carbon in each the same, then carburetted hydrogen contains exactly twice as much hydrogen as olefiant gas does, and that "just half of the oxygen expended on its combustion was applied to the hydrogen, and the other half to the charcoal. This leading fact afforded a clue to its constitution." Whereas, in the case of olefiant gas, two parts of oxygen are spent upon the charcoal, and one part upon the hydrogen.

The atomic weight of nitrogen (azote = 4·2) was doubtless obtained from the consideration of the composition of ammonia, whose atomic weight is given in the table at 5·2. Ammonia was discovered in 1774 by Priestley, but the composition was ascertained by Berthollet in 1775 by splitting it into its constituent elements by means of electricity, when he came to the conclusion that it contained 0·193 parts by weight of hydrogen to 0 807 parts by weight of nitrogen. Dalton assumed that this substance is a compound of one atom of hydrogen with one of nitrogen, and hence he obtained for

the atomic weight of azote $\frac{807+1}{193} = 4\cdot2$; and $4\cdot2+1 = 5\cdot2$

as the atomic weight of ammonia. It is also probable that Dalton made use of the composition of the oxides of nitrogen for the purpose of obtaining the atomic weight of nitrogen. If we take the numbers obtained partly by Davy and partly by himself, as given on page 318 of the "New System," as representing the composition of the three lowest oxides, it appears that the mean value for nitrogen is 4·3 when oxygen is taken as 5·5. In all probability the number in this table (4·2) was obtained from an experiment of Dalton's made at an earlier date.

It is not possible to ascertain the exact grounds upon which Dalton gave the number 7·2 for phosphorus; its juxtaposition, however, in the table, to phosphuretted hydrogen, shows that it was probably an analysis or a density determination of this gas which led him to the atomic weight 7·2, under the supposition that this gas (like ammonia) consisted of one atom of each of its components. In the second table, published in 1808, Dalton gives the number 9 as that of the relative weight of the phosphorus atom, and we are able to trace the origin of this latter number, although that of 7·2 is lost to us. On p. 460, Part II. of his "New System," Dalton states that he found 100 cubic inches of phosphuretted hydrogen to weigh 26 grains, the same bulk of hydrogen weighing 2·5

grains. Hence $\frac{26 - 2\cdot5}{2\cdot5} = 9$ gives the atomic weight of

phosphorus. It was probably by similar reasoning from a still more inaccurate experiment than this one, that he obtained the number 7·2.

Sulphur, which stands in the first table of 1803 at 14·4, was altered in the list published in the "New System" to 13. These numbers were derived from a consideration (1) of the composition of sulphuretted hydrogen, which he regarded as a compound of one atom of sulphur with one of hydrogen, and (2) of that of sulphurous acid, which he supposed to contain one atom of sulphur to two of oxygen. Dalton knew that the first of these compounds contained its own volume of hydrogen, and he determined its specific gravity, so that by deducting from the weight of one volume of the gas that of one volume of hydrogen, he

would obtain the weight of the atom of sulphur compared to hydrogen as the unit. The specific gravity he obtained was about 1·23—corresponding nearly, he says (p. 451) to Thénard's number, 1·23. Hence (as he believed air to be twelve times as heavy as hydrogen) he would obtain the atomic weight of sulphur as (12 × 1·23) − 1 = 13·76, which number, standing half way between 14·4 as given in the first table, and 13 as given in the second, points out the origin of the first relative weight of the ultimate particle of sulphur. So from sulphurous acid he would obtain a similar number, taking the specific gravity as obtained by him (Part ii., 389) to be 2·3, and remembering that this gas contains its own bulk of oxygen (p. 391), he obtained (2·3 − 1·12) × 12 = 14·16 for the atomic weight of sulphur. As, however, we do not possess the exact numbers of his specific gravity determinations, and as we do not exactly know what number he took at the time as representing the relation between the densities of air and hydrogen (in 1803 he says that the relation of 1 : 0·077 is not correct, and that $\frac{1}{90}$ is nearer the truth), it is impossible to obtain the exact numbers for sulphur as given in the first table.

In reviewing the experimental basis upon which Dalton founded his conclusions, we cannot but be struck with the clearness of perception of truth which enabled him to argue correctly from inexact experiments. In the notable case, indeed, in which Dalton announces the first instance of combination in multiple proportion (Manch. Mem. vol. i., series ii., p. 250), the whole conclusion is based upon an erroneous experimental basis. If we repeat the experiment as described by Dalton, we do not obtain the results he arrived at. Oxygen cannot as a fact be made to combine with nitric oxide in the proportions of one to two by merely varying the shape of the containing vessel; although by other means we can now effect these two acts of combination. We see, therefore, that Dalton's conclusions were correct, although in this case it appears to have been a mere chance that his experimental results rendered such a conclusion possible.

INTERNATIONAL METRIC COMMISSION AT PARIS

THE Permanent Committee of the International Metric Commission, elected from among the members at their general meeting at Paris, in 1872, has just concluded a series of meetings, the first of which was held on October 6. The Committee were directed to meet at least once a year, in order, amongst other things, to examine the progress of the work of the French Section, to whom the construction of the new standards was entrusted, with a view to the concurrence of the Committee as the executive organ of the Commission.

At their recent meetings, the Committee fully considered and discussed a detailed report of the proceedings of the French Section since the melting of the great ingot of platinum-iridium on May 13 last, from which all the new International Metric Standards are to be made (an account of which was given in NATURE, vol. x. p 130); and, generally speaking, the Committee expressed their unanimous concurrence and satisfaction at the mode in which the French Section have hitherto executed the duties entrusted to them by the Commission, and they also gave their decisions on certain points submitted to them for the guidance of the French Section in their future operations.

The first operation to which the great ingot of 250 kilogrammes of platinum-iridium was submitted, when in its rough state, and cleansed from all extraneous matter, was to have all the inequalities on its surface, that had been in contact with the lime of the calcined furnace, removed with a cold chisel. The ingot with its surface thus smoothed was found to weigh 236·330 kilogrammes. In this state it was exhibited to the Académie des Sciences at their *séance* of July 2, 1874. A portion of this large homogeneous mass of metal, when analysed by M. Henri Saint-Claire Deville, showed the proportion of iridium to be 10·29 per cent.

The ingot was next forged by M. M. Farcot under a steam hammer weighing 5,000 kilogrammes, until by successive hammerings and annealings, in a single day, it was brought to the form of a bar five centimetres square in section. By similar operators this bar, divided into convenient lengths, was afterwards further reduced to eight bars 2·5 centimetres square in section, and of a total length of 16·405 metres.

A remarkable phenomenon was observed by M. Tresca during the forging of these bars, and was communicated by him to the Académie des Sciences at their *séance* of July 9. At the moment when the hammer struck the bar, lines of light were seen to pass downwards from the edges of the hammer, and to cross each in the form of an X on each of the side surfaces of the bar. These lines continued afterwards distinctly visible in a certain light, appearing like slightly burnished marks.

The next operation was to prepare the bars for drawing into the X form, by cutting longitudinal grooves along the middle of each of the four sides of the bars by means of a planing machine. A further object of cutting these grooves was to ascertain if there were any flaws on the surface of the metal so exposed, as it was found absolutely necessary to remove any such flaws, else they would remain as blemishes on the surfaces of the bars when drawn.

The eight bars were next submitted by M. Gueldry, at the Audincourt foundry, to successive operations of drawing out and annealing, until they were accurately reduced to the X form of the Tresca section, when each was extended to a length sufficient to make three or four metre bars. The first of the grooved bars was passed through the dies no less than 220 times, and was as often subjected to annealing. It was afterwards ascertained that the rigidity of the drawn bars was but little affected by the process of annealing, their co-efficients of elasticity being found as follows :—

Before annealing	...	...	21·2085
After annealing ...	...	...	21·0073

Their co-efficient of expansion was also found to be but very slightly changed, and in the opposite direction, viz.—

	Co-efficient of expansion for 1° C. at mean t. 40° C.	Variation for mean t. 1° C.
Before annealing	0·000008880,2	0,84
After annealing	881,9	0,86

When divided into finished bars of the X section, 1·02 m. in length, each bar is made perfectly straight by special arrangements contrived for this purpose. Four straight edges of steel are made exactly to fit into the grooves of the X bar, and to form, when so fitted, a rectangular bar two centimetres square in section. This squared bar is then enclosed between the plane surfaces of four solid rectangular iron bars; and all being tightly compressed with iron clamps in the form of hollow squares and with iron wedges, the whole is heated in a furnace till red hot, when the clamps are further tightened and the mass of metal is left to cool. By this operation, each of the X metre bars is made perfectly straight. Up to the present time bars of the X section have been made sufficient for more than thirty metres.

The polishing of the surface of the X bars next follows. This is effected by the use of polishing powder and powdered charcoal. Particular attention is given to the polishing and subsequent burnishing of that portion of the surface of the metal on which the defining lines are to be cut. Several experiments which have been made tend to show that the best surface for cutting the lines will be obtained by the final operation of slightly impressing a stamp of highly polished steel, of the dimensions of 3 mm. by 2 mm. By this means an identical