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ANNALS OF PHILOSOPHY;

OR, MAGAZINE OF

CHEMISTRY, MINERALOGY, MECHANICS,

NATURAL HISTORY,

AGRICULTURE, AND THE ARTS.

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ERRATA IN VOL. XV.

- Page 335, line 7, for the sines read the series.
 336, 11, for 5 read 50.
 22, for x^2 read $x^{\frac{n}{2}}$.
 337, 24, for the series read the infinite series.
 338, 13, for sines read series.

ERRATA IN VOL. XVI.

- Page 194, line 6, for $91^\circ 10'$ read $91^\circ 20'$.
 247, 1, for or $\cdot 125$ read $0\cdot 125$.
 248, 10, for $1\cdot 9694$ read $0\cdot 9694$.
 11, for $1\cdot 9700$ read $0\cdot 9700$.
 12, for $1\cdot 9790$ read $0\cdot 9700$.
 267, 4 from bottom, for $1\cdot 76$ read $1\cdot 75$.
 6 from bottom, for $1\cdot 76$ read $1\cdot 75$.
 277, 18, end of the solar eclipse (in a few copies only printed incorrectly),
 for $3^h 14' 47''$ read $3^h 14' 57''$.
 313, 26 (third observation of the temperature during the eclipse), for
 $58\cdot 5^\circ$ read $68\cdot 5^\circ$.

ANNALS
OF
PHILOSOPHY.

JULY, 1820.

ARTICLE I.

HISTORICAL SKETCH OF IMPROVEMENTS IN PHYSICAL
SCIENCE DURING THE YEAR 1819.

1. *The Chemical Sciences.* By Thomas Thomson, M.D. F.R.S.

OF all the sciences which require the aid of experiment, there is none which embraces so vast a field, and which of course requires so many cultivators, as *chemistry*. Its object is to determine the composition of all the substances in nature whether mineral, vegetable, or animal, to ascertain the simple bodies of which they are composed, and to determine the laws by which the union of these simple bodies is regulated. From the vastness of the field, and the comparatively short period of the existence of chemistry as an analytical science, which can scarcely be reckoned longer than 60 years, it is not to be expected that great progress can have been made in the exact knowledge of the composition of bodies as they exist in the various kingdoms of nature. The constituents of the atmosphere and of water are perhaps as accurately known as those of any other body whatever; yet even in these comparatively simple inquiries, there are several points which still remain to be determined. Thus the most accurate experiments hitherto made induce us to conclude that air is a compound of 21 volumes of oxygen gas and 79 volumes of azotic gas; yet the atomic theory requires the constituents to be 20 volumes of oxygen and 80 of azote. We have no conception whatever respecting the nature or properties of the contagious matters, which are supposed occasionally to mix with the air, and to induce certain diseases into living beings.

The mineral kingdom contains a much smaller number of species than either the vegetable or the animal. The substances of which it is composed are of such a nature that they admit of a much easier investigation than those of which vegetables and animals are composed. The consequence is, that very considerable progress has been made in ascertaining their nature and constitution. The greater number of minerals possess the characters of salts containing some one or more of the salifiable bases united to about 10 acids, one or more of which occur in almost every stone. Now when the mineral happens to be a compound of a single acid and a single base mutually saturating each other, there is no great difficulty in determining its composition, and accordingly such minerals may be considered as sufficiently well known. *Calcareous spar, sulphate of barytes, molybdate of lead*, may be given as examples. The first of these is a compound of one atom of carbonic acid and one atom of lime, the second of one atom of sulphuric acid + one atom of barytes, and the third of one atom of molybdic acid and one atom of oxide of lead. Of such simple and well-defined species, a considerable number occur in the mineral kingdom. They are of easy investigation, and their composition may be considered as pretty accurately determined. But a great many minerals are of a more complicated nature than these. They either contain one acid united to two or more bases, or they contain at once several acids and several bases. Thus, for example, felspar is a compound of silica (acting the part of an acid) and potash and alumina (acting the part of bases). In such cases it is much more difficult to determine the true composition of the mineral, and the way in which the various constituents are combined. Accordingly very little progress has hitherto been made in these investigations. The simple salts (by this term, I mean salts composed of one acid and one base) may be decomposed and made again at pleasure. We can produce exact imitations of them in our laboratories; but we cannot, in the same way, produce any thing exactly similar to the more complex minerals. Now till this be done it cannot be said that we have made any considerable progress in the investigation of these complex minerals.

It is not in our power to unite together artificially the constituents of any one vegetable or animal substance so as to make a body exactly similar in its nature and properties; so that we cannot say that at present we are acquainted with the composition of a single animal or vegetable substance. It is true that we have it in our power to induce certain changes in vegetable substances, and by that means to convert them into certain other substances possessed of quite different properties. Thus when the juice of an onion is fermented, a quantity of *mannan* makes its appearance in it, which did not exist in it before. When starch and sulphuric acid are boiled together with a sufficient quantity of water, the starch is converted into a saccharine

matter exactly similar to the sugar of grapes. The same sugar makes its appearance in barley when it is malted; but hitherto it has been out of our power to explain any of these complicated processes, or to show how any of these changes is induced.

But notwithstanding the imperfection of our present knowledge, I see no reason why we should despair of being able hereafter to account for many of those processes which puzzle us at present, and even of being able to form artificially various substances, both animal and vegetable, which we cannot do at present. The first thing is to determine with accuracy the substances of which these bodies are composed; and the next to make ourselves acquainted with the laws by which the combinations of these bodies are regulated.

The great number of distinguished individuals at present devoted to the study of chemistry, and the very considerable progress which it annually makes, renders it more and more probable every year that these hopes will be ultimately realized. My object in this paper is to give a view of the progress which our science has made, or rather of the new chemical facts which have come to my knowledge, since the last annual historical sketch was drawn up. I do not know how far these annual views may interest or inform the reader; but I never fail myself to add to my own knowledge while engaged in drawing them up. The very necessity of classifying the facts, and of comparing them with each other, serves materially to make them sink deep into the memory; while they often suggest new experiments and new views which I am afterwards led to prosecute with considerable advantage. I shall as usual arrange the facts which I have to mention under distinct heads, without being very solicitous about minute precision of arrangement.

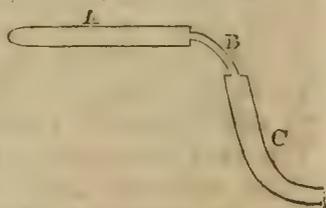
I. APPARATUS.

A good treatise on chemical apparatus, and on the mode of conducting chemical experiments, is at present a desideratum in this country. I have been making arrangements for some time to endeavour to supply this defect, and hope before many years elapse to publish a treatise on the subject, which will be of some utility to practical chemists. I conceive it to be requisite that such a book should contain little except what has been actually verified by the author. This of necessity renders it a work which can proceed only from one who has been for a number of years actually engaged in chemical investigations.

1. The only piece of apparatus which I consider it necessary to notice here is the one contrived by Dr. Prout for analyzing organized bodies by heating them with peroxide of copper. It is described, and a figure of it given in the *Annals of Philosophy*, vol. xv. p. 190. This apparatus is in itself so simple, while it is so distinctly described in the paper which accompanies the engravings, that it is only necessary to refer the reader to the

page of the *Annals of Philosophy* just quoted. I have no doubt that this apparatus furnishes an important addition to the means of analysis. It is a considerable simplification of Berzelius's original contrivance for the same purpose; while the small size of the tube renders the quantity of common air which can be mixed with the gas exceedingly trifling.

The apparatus which I myself use for similar experiments is fully as simple as that of Dr. Prout, and I consider it as possessing some advantages to those persons who are in possession of a mercurial trough of a sufficient size to admit experiments to be made over mercury on a tolerably large scale. It consists of three tubes, all fitted to each other by grinding. The first, which is of copper, is about 12 inches long, and the third of an inch in diameter. The second is of brass; its internal diameter does not exceed the tenth part of an inch, and it is about four inches in length. It is fitted by grinding to the open end of the copper tube. The third tube is of glass with a brass cape on one end to which the other extremity of the brass tube is fitted by grinding. This glass tube is about eight inches long, and its diameter is about 0.4 inch. The figure in the



margin will give the reader some idea of the arrangement of these tubes. A is the copper tube, B the brass tube, and C the glass tube. The glass tube is filled before the process with muriate of lime well-dried, and the weight of it is carefully taken. The tube A is put into a small choffer which is filled with red-hot charcoal. The furthest extremity of the tube C is plunged under a graduated jar inverted over the mercurial trough and filled with mercury. Sometimes it is necessary to expose the tube A to a strong heat: this is easily done by blowing the charcoal with a pair of bellows. Sometimes it is necessary to keep the substance subjected to analysis quite cold till the greater part of the tube A is red-hot. This I accomplish by introducing the mixture of the peroxide of copper, and the substance to be analyzed into the bottom of the tube A, so that it shall occupy not more than three inches of the length of the tube. This portion is coated with wet clay, and drawn entirely beyond the choffer, that portion of charcoal which is furthest removed from the coated part of the tube is first kindled, and the heat is allowed to penetrate very slowly to the coated part of the tube. When about four or five inches of the copper tube have become completely red-hot, the clay coating is removed from the tube; the heat soon makes its way to the furthest end of the tube, and in a few minutes gas begins to make its appearance. If the substance subjected to analysis be very volatile (camphor, for example), it is merely necessary to allow the apparatus to remain for about a quarter of an hour to obtain its complete decomposition and conversion

into carbonic acid gas and water ; but sometimes when the substance analyzed is of a fixed nature (Kilkenny coal, for example), we do not produce its entire destruction till we draw the coated end of the tube into the choffer, and heat it fully red-hot.

The capacity of the three tubes of which my apparatus consists, supposing them empty, amounts to two cubic inches ; but as the copper tube is completely filled with peroxide of copper, and the glass tube with muriate of lime, the capacity is reduced to 0.5 cubic inch. It is this considerable quantity of common air with which my gaseous products are mixed that constitutes the defect of my apparatus. The mean quantity of common air is 0.5 cubic inch ; but sometimes it is as small as 0.44, and sometimes as great as 0.60 cubic inch. This difference obviously depends upon two things : 1, the degree of heat which I apply ; 2, the care with which the tubes have been filled with peroxide of copper and muriate of lime. At present, when the substance subjected to analysis contains azote, and when consequently the gas which I obtain is a mixture of carbonic acid and azotic gas, I cannot be absolutely certain of the bulk of the azotic gas. My method is to make at least six trials, to reckon the bulk of the common air 0.5 cubic inch, and to reckon the bulk of residue after the carbonic acid gas has been absorbed by potash, exceeding 0.5 cubic inch, to be azote. This, in such a number of trials, cannot deviate far from the truth ; but the necessity of repeating the experiment so often as six times is a serious inconvenience. When the substance analyzed contains no azote, I obtain a very good result by taking the mean of two trials. I have no doubt that I shall succeed in correcting this defect by making my apparatus on a smaller scale. This I intend speedily to try.

In analyzing animal and vegetable substances by means of peroxide of copper, I have observed that if we make use of the same peroxide of copper for a number of successive experiments, heating it to redness after each, after a certain time it becomes useless as a reagent. It ceases to give out oxygen to the substance heated with it. Of course the results are nearly the same as if the substance subjected to analysis were merely heated in contact with so much sand. To bring back the peroxide of copper to its original state of goodness, I find it necessary to dissolve it in an acid, and precipitate it by an alkali. This change in the state of the peroxide is not owing to the oxygen which it contains being diminished, but to the density of the powder being greatly increased. After a certain portion of peroxide of copper has been employed in twelve successive experiments, its bulk is reduced to one-third of what it was at first ; but its weight is not sensibly altered.

2. I may here notice the method which I commonly employ to detect the quantity of lime, when that lime has been precipitated by oxalate of ammonia. It is connected with experimental

chemistry, and, therefore, is not far out of place, while I am treating of chemical apparatus. The method which I use, and to which I have alluded in the *Annals of Philosophy*, vol. xiv. p. 147, is simply this: I wash the oxalate of lime till it is perfectly clean, and then dry it by exposure to the open air, or to a heat not exceeding 212° . I then put it into a platinum crucible, the weight of which I have previously determined. This crucible I enclose in a common hessian or black-lead crucible, and expose it to a white heat for half an hour in a good wind furnace. I then take it out of the fire, and, when cold, weigh the platinum crucible again. The additional weight of the crucible gives the quantity of lime; for a white heat converts oxalate of lime into quicklime. On dissolving this residuary quicklime in acids, I never find any quantity of charcoal sufficient to make any sensible alteration in the result.

The oxalate of lime may be considered as a compound of

1 atom oxalic acid	=	4.500
1 atom lime	=	3.625
		8.125

Hence the lime which it contains ought to be $\frac{3625}{8125}$, or $\frac{29}{65}$, of the weight of the oxalate of lime. By this method we obtain results that approach the truth pretty nearly; but they are not absolutely to be depended on unless we could succeed in depriving the oxalate of lime of the whole of its water, which I find it very difficult to do. I find it, therefore, easier, and much more accurate, to convert the oxalate of lime into quicklime.

II. LIGHT.

1. *Magnetising Property of the Violet Ray.*—The readers of the *Annals of Philosophy* are aware of the experiments announced some years ago, as made by Morichini, that steel wire, when exposed for a certain time to the violet rays of the sun, are converted into magnets. These experiments had been tried by various philosophers, and generally without success, though there were some who affirmed that they had verified Morichini's discovery. The reality of this power of the violet rays, however, was generally doubted till it was announced by the editors of the *Bibliothèque Universelle* that the late Professor Playfair had witnessed a successful experiment of this kind. Since that period various other persons have tried this experiment with considerable industry; but, as far as I have heard, without the least success. A set of unsuccessful trials by M. Dhombres Firmas is related in the *Annales de Chimie et de Physique*, tom. x. p. 285. I have sometimes been tempted to suspect that Morichini's success in this experiment may have depended upon the position of his needles during his experiments. If the

needles had been placed nearly in the magnetic meridian, a certain temperature preserved for a certain time may be conceived to be capable of inducing permanent magnetism. Probably the red ray failed of this effect by inducing too high a temperature.

III. HEAT.

No great quantity of additional matter respecting heat has been brought into view during the course of the last year; yet there is a very extensive field in this department of chemistry quite open to a judicious experimenter. The facts upon which our opinions respecting heat are founded remain still in a very loose and unsettled state. An accurate set of experiments upon bodies as conductors of heat, upon their specific heat, their latent heat, and the temperature at which they change their state, would set the whole subject in a new point of view, and might even furnish us with some useful data towards resolving the long agitated and still unanswered question, whether heat be a substance or a property. Petit and Dulong seem to have taken up the subject in this judicious point of view, and we have reason to expect much valuable information from a continuance of their experiments; but I am not aware of any new paper on the subject which they have published since the historical sketch of last year was written.

The most remarkable paper on this subject which I have seen is an early production of Prof. Leslie, entitled "On Heat and Climate," which had been read at two meetings of the Royal Society as far back as the year 1793; but which was published for the first time in the *Annals of Philosophy*, vol. xiv. p. 5. The novelty of the matter contained in this paper, its total deviation from the opinions at that time generally received, and the originality of the reasoning, and the disregard of authority evinced by the author, appear to have startled the committee of the Royal Society, and prevented them from inserting it in their Transactions. I think this decision is to be regretted. It obviously tended to retard the progress of the investigation of heat; though as far as Mr. Leslie was concerned, it had a favourable effect. It induced him to reconsider the subject, and probably led to most of the investigations afterwards published in his "Inquiry into the Nature and Propagation of Heat," a publication which has raised the author to so high a rank as an original thinker and discoverer.

It will not be expected that I should enter into a minute analysis of this curious paper. It obviously contains many of the opinions afterwards brought forward in the "Inquiry," and shows us that the author's notions respecting heat had been taken up at a very early period. Heat he considers as only light fixed in bodies. It is incapable of radiating from bodies without being again converted into light. It only moves by conduction. Hence, in his opinion, if a hot body could be placed in a

perfect vacuum, it would never cool at all. I consider the experiments of Dulong and Petit to have overturned this opinion, and to have shown that even in a vacuum a hot body would cool, in consequence of the property which bodies have of radiating heat. Indeed Mr. Leslie's opinion could be considered in no other light than as a simple conjecture totally unsupported by experimental proof.

The investigation of the quantity of heat evolved by the compression of air contained in this essay is very ingenious; but the reasoning, probably from his not condescending to make us acquainted with the intermediate links, is not of such a kind as to prove perfectly satisfactory to the reader. The same observations apply I conceive to Mr. Leslie's explanation of the origin of heat from percussion and friction. The notion that the mean temperature of the earth is perpetually increasing has been entertained by other persons as well as Mr. Leslie; though I am not aware that any other person has attempted to estimate the rate, which, according to him, is about 1° in 800 years, or to deduce from that rate the period which has elapsed since the beginning of things. Nothing can exceed the ingenuity of this whole discussion; yet I cannot help being of opinion that it exhibits an example of the improper application of mathematics to a case that is not capable of bearing that science. I am not myself disposed to admit the data from which Mr. Leslie has calculated. Now it is needless to say that upon these data the whole value of the discussion depends. The reasoning respecting the rate at which the temperature of the air diminishes as we ascend in the atmosphere is good, and I suspect had it been published in 1793, it would have had the merit of originality. I am not so much satisfied with the observations on climate, contained in the last part of the paper, though I give full credit to the sagacity and originality of the author.

There is one assertion, however, of our author, which I must notice, because I consider it as inaccurate. He says that no sensible increase of temperature is ever observed when we descend into the deepest mines. Now the fact is that in most cases a very marked increase of temperature is observed in deep mines. In the copper mines of Cornwall, it is no uncommon thing to find the air hot enough to raise the thermometer to 100° . In the salt mines in Cheshire, the miners work without their clothes, and rather complain of heat than cold. I had no thermometer when I descended into the salt mine at Nantwich, but from the feel, I should rate the temperature of the air between 80° and 90° . It will be seen from a table published by Mr. Bald (*Edin. Phil. Jour.* i. 135), that the air and the water at the bottom of the deep coal mines in Durham, Cumberland, Northumberland, and Stafford, are from 12° to 19° higher than at the surface of the earth. In short, wherever the subject has been investigated, it has been uniformly observed that the mean tem-

perature of mines is higher than that of the air at the surface of the earth where these mines exist. I think that a very satisfactory explanation of this difference may be given without having recourse to the notion of a subterranean or central fire; but the fact itself is undoubted, and furnishes an example of the hazard which we run, if we venture to make general assertions respecting heat without being at the trouble of subjecting our notions to the test of experiment. Whoever wishes to see how far even a man of great ingenuity may deviate from the truth, when he proceeds in that manner, has only to peruse Boerhaave's treatise on heat, in his *System of Chemistry*.

I may mention here, though quite unconnected with the preceding observations, the great heat which Dr. Clarke has produced by his gas blow-pipe. He not only fused half an ounce of platinum, but kept it, he says, in a boiling state before his class.—(See *Annals of Philosophy*, xiv. 230.)

IV. GALVANISM.

Dr. Hare, Professor of Chemistry in the Medical Department of the University of Pennsylvania, has published (in a paper to be found in the *Annals of Philosophy*, xiv. 176) a theory of galvanism differing considerably from all those hitherto started. According to him, the galvanic fluid is a compound of caloric and electricity. The electricity is increased by the number of pairs of plates, and when this number is very great, as in Deluc's column, the calorific effects become evanescent. The caloric is evolved by the increase of the surface, and he has shown that it may be very intense when only a single pair of plates, or what is equivalent to it, is used. He has given us the drawing of a galvanic battery constructed on this principle, which produces intense ignition without any electrical phenomena.

As I have not yet had leisure to study the phenomena exhibited by this new modification of the galvanic battery, I should consider it as improper to make any remarks on Dr. Hare's ingenious hypothesis at present. What I have to say on the subject, therefore, I shall reserve for another opportunity.

V. SUPPORTERS OF COMBUSTION.

Nothing very important has been added to the facts previously ascertained respecting these very important bodies. The following are the only observations which I have been able to glean:

1. *Oxygen*.—Soon after the discovery of this gas, and the knowledge of its most remarkable properties, it was tried whether when breathed instead of common air, it would not act medicinally, and in particular whether it might not in all probability prove a remedy in consumption. The effects observed were directly the contrary of what had been anticipated. The velocity of the pulse was accelerated, and the fatal effects of the disease became more speedy. This led medical men not

merely to give up the use of oxygen as a cure for consumption, but to adopt a gas of a very opposite kind. Carburetted hydrogen gas diluted with a certain proportion of common air was inhaled by consumptive patients with apparent good effects. The late Mr. Watt, who had interested himself very much in these investigations, and who possessed much more sagacity and acuteness than any of his coadjutors in the promotion of pneumatic medicine, as it was called, retained his belief of the good effects of this last remedy in consumptions to the very last. The very last time that I was in his company (about a year before his death), he happened to broach the subject, and he expressed himself with considerable confidence. The readers of the *Annals of Philosophy*, to many of whom these facts must be familiar, would, therefore, see with surprize that a mixture of oxygen and azotic gases from saltpetre had been used in the United States with success as a cure for consumption.—(See *Annals of Philosophy*, xiv. 71.)

2. *Chlorine*.—It has been ascertained that chlorine has the property of combining with lime, barytes, strontian, potash, soda, and several other bases, and of forming with them *chlorides*, which possess the property of whitening vegetable bodies. One of these, the chloride of lime, has been long used to a very great amount in Great Britain and Ireland, to bleach cotton and linen cloth. It is known by the name of *bleaching powder*. Besides the manufactories of this powder in Great Britain, there are no fewer than four in Ireland. This powder varies considerably in its goodness, according to the care with which it is manufactured, and the length of time that it is kept, and hitherto no very unexceptionable mode of analyzing it had been suggested by chemists. I have some considerable hopes that this defect has been in a great measure supplied by the method of analysis which I inserted in the last number of the 15th volume of the *Annals of Philosophy*. I have repeated it several times, and found the results to agree so well with each other, and to correspond so well with the discolouring powers of the powder, that I consider it as entitled to confidence. The process is attended with as few difficulties as any other mode of analyzing so complicated a substance with which I am acquainted. Bleaching powder always contains the following substances :

1. Subbichloride of lime,
2. Muriate of lime,
3. Uncombined lime,
4. Water.

Of these, the only substance of any importance to the bleacher is the first. Now its weight may be determined by the first step of the analytical process, as follows :

Put 1000 grs. of the bleaching powder into a retort, the beak of which, by means of a long tube luted to it, is connected with a

water-trough. Expose the powder in a sandpot to a heat not under 600° or 700° . A quantity of oxygen gas will be driven off. Measure the bulk of this gas, and reduce it to what it would be, supposing the barometer at 30 inches, and the thermometer at 60° . Double this volume. It will indicate the cubic inches of chlorine gas contained in 1000 grs. of the bleaching powder. Turn this volume of chlorine into weight by having recourse to the table of the specific gravities contained in vol. iii. p. 25, of my System of Chemistry. Let this weight

be = x . Then say, $4.5 : 7.25 :: x : \frac{7.25}{4.5} x$ = the weight of lime

united to the chlorine; consequently the weight of subbichloride of lime in the 1000 grs. of bleaching powder is equal to $x + \frac{7.25}{4.5} x$. Now this is equivalent to $\frac{47}{18} x$. Therefore to find the weight of subbichloride of lime in the powder, we have only to multiply the number x by 47, and divide the product by 18, or, which comes to the same thing, we may multiply x by 2.61.

It will be seen by inspecting the table of the constituents of bleaching powder contained in my paper already alluded to, that the quantity of muriate of lime contained in recently prepared bleaching powder amounts to between 16 and 18 per cent. or about one-sixth of the weight of the powder. This portion of muriate seems to be formed at the time that the chlorine unites with the lime. Probably a small portion of muriatic acid gas is mixed with the chlorine gas extricated from the mixture of manganese, common salt, and sulphuric acid, in the stills; at least, I have always seen this the case when I myself prepared chlorine gas in that manner.

The water amounts likewise to about one-sixth of the weight of the powder; so that the muriate of lime and water together constitute about one-third of the powder. Not much less than another third is uncombined lime; so that the subbichloride of lime really contained in the bleaching powder, as it is usually furnished to the bleachers, does not much exceed one-third of the weight of the powder.

It will be seen from another analysis contained in my paper, that it is possible to make bleaching powder strong enough to contain rather more than half its weight of subbichloride of lime, and not more than five per cent. of uncombined lime. But I am satisfied, from a great many trials, of the powder from various manufactories, and from a careful inspection of the process, that the manufacturer could not make the powder so strong upon a large scale, except at a price that would render it useless to the bleacher.

3. *Iodine*.—Von Mons has described a pretty experiment with iodine, which I have not repeated; though I see no reason why it should not succeed. The experiment is this:

Dissolve some iodine in muriatic acid, and pour over the solu-

tion ammonia, taking care that the two liquids do not intermix. In a short time the iodine crystallizes between the two liquids in beautiful pyramids whose bases are turned towards the surface of the liquid. A slight agitation makes these crystals immediately disappear.—(Jour. de Pharm. vi. 95.)

Dr. Andrew Fyfe, of Edinburgh, has made some experiments to determine the substances which contain iodine. The following are the results of his researches :

(1.) Iodine cannot be detected in sea-water.

(2.) It is confined to marine productions alone.

(3.) He procured it from the *fucus nodosus*, *fucus serratus*, *fucus palmatus*, and *fucus digitatus*. He obtained it also from the *ulva umbilicalis*, and from a species of *conferva*. The *fucus vesiculosus* would not yield him any.

(4.) Neither oysters nor coral yielded him any; but the common sponge of the shops yielded it when treated in the usual way.—(Edin. Phil. Jour. i. 254.)

VI. SIMPLE COMBUSTIBLES.

1. *Hydrogen*.—I noticed in the historical sketch of last year, that a careful set of experiments on the specific gravity of hydrogen gas, made in my laboratory, gave for the result, 0·06933. This comes very near to the number deduced by Dr. Prout from the specific gravity of ammonia. It establishes, therefore, the relative specific gravities of hydrogen and oxygen gases to each other as 1 to 16. Hence it follows that exactly one-ninth of the weight of water is hydrogen, while eight-ninths are oxygen.

I propose very soon to give a set of careful experiments, which have been made in my laboratory, to determine the specific gravity of the gases.

2. *Sulphur*.—The only addition to the history of sulphur with which I am acquainted is the analysis of the chloride of sulphur, which I published in the last number of the *Annals of Philosophy*. I was myself a good deal gratified with the results which I obtained, and they will be viewed by chemists as of some value, because they account for the different properties of the chloride of sulphur as obtained by different chemists.

Chloride of sulphur obtained by passing a current of chlorine gas through flowers of sulphur till they are converted into a liquid is a compound of one atom chlorine + two atoms sulphur. Hence it is a subbichloride of sulphur. Its constituents are :

Sulphur.....	4·0	or 47·06
Chlorine.....	4·5	52·94
		<hr/>
		100·00

When agitated in water, one half of the sulphur separates, the other half is changed into hyposulphurous acid; while the chlo-

rine is converted into muriatic acid; so that water is decomposed, the sulphur combines with its oxygen, while the chlorine unites with its hydrogen. When the liquid is saturated with ammonia, we obtain hyposulphite of ammonia, and muriate of ammonia. Nitrate of silver, when dropped in, throws down the muriatic acid, and decomposes the hyposulphite of ammonia, forming a hyposulphite of silver, which gradually undergoes spontaneous decomposition, and is precipitated in the state of sulphuret of silver.

If the current of chlorine be continued after the formation of subbichloride of sulphur, an additional dose of it will combine with the liquid, and if the process be continued long enough, the whole will be converted into a chloride of sulphur, or a compound of one atom chlorine + one atom sulphur. From an experiment of Sir H. Davy, it would appear that this chloride may be formed directly by putting sulphur into a sufficient quantity of chlorine gas.

3. *Arsenic.*—The weight of an atom of arsenic, as I fixed it in the last edition of my *System of Chemistry*, namely, 4.75, did not accord very well with the atomic theory; for it obliged us to consider arsenious acid as a compound of 1 atom arsenic + $1\frac{1}{2}$ atoms oxygen, and arsenic acid as a compound of 1 atom arsenic + $2\frac{1}{2}$ atoms of oxygen. To get rid of these anomalies, it was only necessary to double the weight of an atom of arsenic, and to represent it by 9.5. We then have:

	Arsenic.	Oxygen.
Arsenious acid composed of.....	1 atom	+ 3 atoms
Arsenic acid:	1	+ 5

I have shown in a paper published in the *Annals of Philosophy*, xiv. 466, that the latest experiments of Berzelius (see *Annals of Philosophy*, xv. 352) entirely agree with the composition of these acids as I had previously established them, and that he had laid aside his notions respecting their composition, which he had founded on a set of hasty experiments. In another paper (*Annals of Philosophy*, xv. 81), I have given the analyses of arseniates of potash and of soda, and shown that these salts, when regularly crystallized, are neutral, and that the arseniate of potash is a compound of 14.5 acid + 6 potash; while the arseniate of soda is a compound of 14.5 acid + 4 soda. These salts, therefore, correspond with the notion, that an atom of arsenic weighs 9.5; an atom of arsenious acid, 12.5; and an atom of arsenic acid, 14.5. It will be seen too from the same paper that all the arseniates hitherto analyzed agree with the same thing. I consider it, therefore, settled, that an atom of arsenic weighs 9.5.

4. *Tellurium.*—Von Mons relates an experiment, said by him to have been just made by Sir H. Davy, in Italy, upon what authority I do not know; but I shall give it to my readers as he

states it. Davy attempted to alloy together tellurium and potassium. A good deal of heat was disengaged, and a gas was evolved which could not be condensed, and which broke the apparatus to pieces.—(Journ. de Pharm. v. 505.)

5. *Iron*.—Mr. Porrett has been led by the constitution of ferrochyzic acid, as determined by his experiments, and my own, to consider the weight of an atom of iron to be only 1.75, or half the number which I have assigned in the last edition of my *System of Chemistry*. I think it by no means unlikely that the opinion is well founded; though I am unwilling to introduce this new weight into the atomic theory till we have made further progress in our knowledge of the laws that substances observe with respect to their combinations. Mr. Porrett has given us the following table of the combinations of iron and sulphur, which he considers as highly favourable to his view of the subject:

1. *Cubic Pyrites.*

1 atom iron	=	1.75
1 atom sulphur.	=	2.00
		3.75

2. *Magnetic Pyrites.*

2 atoms iron	=	3.5
1 atom sulphur	=	2.0
		5.5

3. *Sesquisulphuret.*

3 atoms iron	=	5.25
2 atoms sulphur.	=	4.00
		9.25

4. *Tritosulphuret.*

4 atoms iron	=	7
3 atoms sulphur.	=	6
		13

The last two of these sulphurets Mr. Porrett informs us were formed by Proust, and described by him in his paper on the Native and Artificial Sulphurets of Iron; but it does not appear from his paper that Proust was aware of any other sulphurets of iron, except magnetic and cubic pyrites.

M. Duleau has published a set of experiments on the force of malleable iron to resist being crushed when placed in a vertical situation, or when constituting a part of a vault. The following propositions contain the most important consequences which he has deduced from his experiments:

For two rectangular pieces of different sizes charged perpendicularly to their length, the bendings, or the descents, of the

loaded point are inversely as the cubes of the lengths, and directly as the cubes of the thickness of the bars.

The resistance of a square piece, whether placed on a face, or on an angle, is the same.

The resistance of a round piece is to that of a square piece whose side is equal to the diameter of the former as three-fourths of the circumference of a circle to the contour of the circumscribed square. A consequence of this theorem is that supposing the volume the same, the resistance of a cylinder to that of a square piece is as 20 to 21 very nearly.

When a piece supported by its two extremities is left to the effect of its own weight, the bend which it takes is to that which the same weight would produce if concentrated in the middle as 5 to 8.

When a piece supported by the middle is left to its own proper weight, the bend is to that which the same weight would produce if divided into two equal parts, and placed at the two ends as 3 to 8.

The weight capable of bending a piece by pressing it in the direction of its length is proportional to the product produced by multiplying by the length of the piece the resistance which it presents when charged perpendicularly to the direction of its fibres.* Hence it follows, that for any piece whatever, this weight is inversely as the square of the length; and that for a rectangular piece, it is besides directly as the size and as the cube of the thickness.

When two rectangular pieces of equal dimensions are placed, the one above the other, at a distance a , and connected with each other, so as neither to be able to separate, nor to slide on each other. The resistance of the system of these two pieces is proportional to

$$(e + a)^3 - a^3$$

e denoting the sum of their thickness.

For a piece having the form of a cylindrical or square tube, the resistance is proportional to the difference between the fourth powers of the outer and inner diameters. It follows from this that a hollow tube, whose thickness is one-twentieth of its diameter, is $9\frac{1}{2}$ times stronger than a solid cylinder of the same weight.

These are the results of M. Duleau's experiments, upon which the greatest confidence may be placed. The reader is referred for a more complete account of these important experiments to the analysis of his memoir, printed in the *Ann. de Chim. et de Phys.* xii. 133.

M. Robiquet has ascertained that octahedral iron ore consists

* This is only true with regard to an elastic and flexible plate, but which is not extensible nor compressible in the sense of its length.

almost always of a combination or mixture of about 94 parts of protoxide of iron and six parts of oxide of titanium.—(Journ. de Pharm. v. 258.)

6. *Nickel*.—It seems barely necessary to put the reader in mind of Dr. Clarke's method of obtaining pure nickel by means of the blow-pipe, and of the process which I myself am accustomed to follow to procure this metal in a state of purity. They will both be found together in the *Annals of Philosophy*, xiv. 142.

7. *Manganese*.—Von Mons has stated a curious experiment, which, he says, may be made upon the black oxide of manganese. If a hot solution of binoxalate of potash be poured upon the black oxide of manganese, and the mixture be thrown upon the filter, we obtain a liquid of a fine red colour. During the action of the acid on the oxide some carbonic acid is disengaged. (Journ. de Pharm. v. 307.) It is obvious that the solution is analogous to the red sulphate of manganese. The oxide loses a portion of its oxygen, and is reduced to the state in which it dissolves in sulphuric acid, and forms the red sulphate. Would not pure oxalic acid answer better than the binoxalate?

8. *Lead*.—I have related a fact respecting the protoxide of lead which I think deserves the attention of those whose business it is to reduce that metal from the ore by smelting. I found that I could volatilize this oxide by an ordinary red heat; the consequence of this great volatility is, that much of the metal must make its escape during the process of smelting. (*Annals of Philosophy*, xiv. 314.)

A very elaborate set of experiments by Berzelius, to determine the composition of oxide of lead, will be found in the *Annals of Philosophy*, xv. 94. They appear to have been made with the most scrupulous attention to accuracy, and approach as near the truth as we can come experimentally in the present state of our apparatus. The result is, that protoxide of lead is a compound of

Lead.	100.000
Oxygen.	7.725

If we suppose this oxide a compound of one atom lead and one atom oxygen, and calculate from the analysis of Berzelius, we obtain for the weight of an atom of lead, 12.945. The number 13, which I have pitched upon, is within less than half a per cent. of the number obtained from the result of Berzelius's experiments. Now I am afraid that Berzelius deceives himself when he thinks that he can depend upon the result of his analyses within less than half a per cent. I have no doubt whatever that the difference between 13 and 12.945 is within the limits of unavoidable error. The simplicity and great convenience of 13 gives it in my eyes a preference, which will induce me to retain it till I see stronger reasons than have yet been advanced for

adopting a more complicated number. Nature delights in simplicity. Hence I am led to expect simple numbers for the weight of the atoms of the simple bodies.

9. *Cadmium*.—I have nothing to add to the account of this metal which I gave in last year's historical sketch, except to refer the reader to a paper drawn up by Prof. Gilbert, a translation of which I inserted in the *Annals of Philosophy*, xiv. 269. It contains all the characters of this new metal detailed by Stromeyer in his different papers on the subject, and must be of considerable utility to all those who undertake the examination of the zinc ores which contain cadmium.

The reader is aware of the interesting discovery by Dr. Clarke of cadmium in a silicate of zinc from Derbyshire, an account of which will be found in the *Annals of Philosophy*, xv. 272. I may mention that I have lately had an opportunity of examining a carbonate of zinc from Derbyshire, which was of a dirty white colour, with red spots here and there, which I found contained almost exactly half a per cent. of cadmium. I extracted the metal by the process of Stromeyer, and took the opportunity of subjecting the new metal to a few trials in order to make myself acquainted with the appearances which it exhibits when treated with various reagents. It is probable, I think, from these two examples of cadmium occurring in two different British ores of zinc that British chemists will soon have it in their power to obtain this new metal at least in quantities sufficient to subject it to a correct examination.

10. *Bismuth*.—The analogy between bismuth and lead is well known to chemists. The two metals resemble each other likewise in their volatility. It appears from the experiments of Chaudet, that bismuth is volatilized if it be kept in the temperature of 30° Wedgewood, even though it be covered with a coating of charcoal. (*Annals of Philosophy*, xiv. 229.)

11. *Mercury*.—Mr. Donovan's experiments to determine the composition of the oxides and salts of mercury, to which I alluded in the historical sketch of last year, have been since published in the *Annals of Philosophy*, xiv. 241. Every chemical reader who peruses the paper will agree with me in thinking that it possesses unquestionable merit, and that Mr. Donovan has added very materially to the accuracy of our knowledge of the combinations of mercury. For my own part at least I can safely say that I derived a great deal of additional knowledge from the perusal of Mr. Donovan's paper, and that I should have regretted exceedingly had it remained unpublished.

Mr. Donovan finds that the best way of procuring protoxide of mercury is to mix calomel at once with a considerable quantity of caustic potash, and triturate them together. When the globules of running mercury are separated, a black powder remains, which is protoxide of mercury. He finds the composition of the two oxides of mercury as follows :

Protoxide	100 mercury	+ 4.12 oxygen
Peroxide	100	+ 7.82

Mr. Donovan informs us, that though he repeated his experiments several times, the results were precisely as above stated. They do not accurately correspond with the atomic theory, and, therefore, cannot be quite correct; but if we take the mean of the two, we obtain the composition of the two oxides of mercury as follows :

Protoxide	100 mercury	+ 3.98 oxygen
Peroxide	100	+ 7.96

numbers which differ very little from those determined by former experimenters, and which, I believe, to approach very near the truth.

Mr. Donovan's experiments on the oxides of mercury have led him to suggest a very material improvement in the blue mercurial pills and ointment used externally to excite salivation in cases of the venereal disease. It has been long known that this ointment consists in a great measure of metallic mercury merely mechanically mixed with lard. He suspected that its efficacy was owing entirely to the small quantity of black oxide which had been chemically united to the lard. This induced him to try to form such a combination artificially. He found that when an ounce of lard and 21 grs. of oxide are kept together at a temperature of 320° for an hour, taking care to stir them constantly, they combine chemically, and form an ointment much more efficacious, much more cleanly, and much cheaper than the common ointment.

12. *Gold*.—Count Le Maistre has given a very simple process by which a fine purple colour, fit for painting in oil, may be obtained from the solution of gold. The colour I have myself seen, and think it fine. The process is simple. I need only refer the reader to the *Annals of Philosophy*, xiv. 361, where the method of proceeding is described by the Count himself.

13. *Platinum*.—Mr. Fox has pointed out the strong affinity which exists between platinum and tin; and Dr. Clarke has described the phenomena of the union of the two metals, when exposed to the heat of the blow-pipe (*Annals of Philosophy*, xiv. 229). I have myself repeated the experiment, and find it very similar to the appearance which takes place when tin is strongly heated by the blow-pipe, and then dropped upon a table. The metal burns with great brilliancy, and flies into a number of small pieces.

14. *Effect of Heat upon the Colours of Metals*.—M. Chaudet has published a set of experiments, which may have their utility, to determine the appearances which different pure metals exhibit when kept for some time in a strong heat on the cupel, and how these appearances are modified when these various

metals are alloyed together. The following are the principal facts which he has observed:

(1.) Pure tin becomes covered with a greyish-black oxide, increases much in bulk, then exhibits the appearance of combustion, and at last leaves a red-coloured oxide, which, on cooling, becomes first yellow, and at last white.

(2.) Antimony becomes first black, then melts, resuming its metallic splendour, and allowing a vapour to fly off. The whole metal is volatilized in a white smoke, leaving yellowish and reddish spots on the cupel.

(3.) Zinc melts, blackens on the surface, takes fire all of a sudden, and burns with a very brilliant greenish-white flame, giving out a white thick smoke. The oxide is gradually elevated into a cone. When removed from the fire, it is at first greenish, but, on cooling, becomes snow-white.

(4.) Bismuth soon melts, and is covered with a coat of oxide which melts likewise. A small portion of the oxide sublimes; the rest sinks into the pores of the cupel, leaving it of a fine orange-yellow colour with some spots of green.

(5.) Lead exhibits exactly the same phenomena, and differs from bismuth merely in the colour which it leaves on the cupel, which, when lead is used, is always lemon-yellow, becoming pale and dirty by exposure to the air.

(6.) Copper assumes on its surface different iridescent shades, which succeed each other with rapidity, leaving at last a coating of black oxide, which is detached as the metal cools. If the furnace be hot enough, the metal melts, and is soon covered with a coating of black oxide.

When tin is contaminated by any iron, the presence of this last metal becomes manifest by the spots of rust with which the white oxide is tarnished after the metal has been exposed on the cupel.

The presence of a quarter per cent. of antimony in tin may be recognized by the greyish-black spots with which the white oxide of the metal is mixed after exposure on the cupel.

When a small quantity of zinc is alloyed with tin, this last metal loses the property of burning by covering itself with incandescent points, as happens when the tin is pure. The oxide, when cold, has a shade of greenish-grey even when the zinc does not exceed one per cent.

Bismuth alloyed to tin, even when the proportion does not exceed five per cent. gives to the oxide a greyish colour mixed with yellow, or, if it does not exceed one per cent. merely a greyish colour.

Less than five per cent. of lead may be detected in tin by the colour of rust which it communicates to the oxide of this last metal.

Less than one per cent. of tin can be detected in lead, because the lead in that case, when exposed on the cupel, remains

tarnished, and exhibits on the surface small quantities of oxide of tin.

When tin is alloyed with some per cents. of copper, this last metal may be distinguished on the cupel by the rose-red colour which makes its appearance. (*Ann. de Chim. et de Phys.* xii. 342.)

VII. ACIDS.

1. *Boracic Acid*.—The experiments of Berzelius to determine the composition of boracic acid, given in the *Annals of Philosophy*, xv. 278, are merely a republication of the experiments made by him some years ago, and first published in an early volume of the *Annals of Philosophy*. I am still of opinion that the constitution of this acid which I have given in the fifth edition of my *System of Chemistry*, is the nearest the truth, according to the present state of our knowledge. I have lately had an opportunity of repeating and verifying some of the experiments on which this constitution was founded.

2. *Phosphorous Acid*.—Berzelius, in a set of experiments published some years ago in the *Ann. de Chim. et de Phys.* has endeavoured to prove that the oxygen in phosphorous and phosphoric acids are to each other as the numbers 3 and 5. I have stated shortly, but I trust with sufficient clearness, the reasons that lead me to conclude that the oxygen in phosphoric acid is exactly twice as much as that in phosphorous acid, and that the composition of the two acids is as follows :

Phosphorous Acid.

1 atom phosphorus	= 1·5
1 atom oxygen	= 1·0
	2·5

Phosphoric Acid.

1 atom phosphorus	= 1·5
2 atoms oxygen	= 2·0
	3·5

If the reader will turn to *Annals of Philosophy*, xv. 227, he will find the evidences on which the truth of this opinion depends.

3. *Sulphuric Acid*.—The latest experiments of Berzelius on the constituents of this acid (*Annals of Philosophy*, xv. 96) make it a compound of

Sulphur	100·00
Oxygen	148·44

This is a considerable approximation to the true composition, and indeed the nearest hitherto obtained by experiment. It is a good deal nearer the truth than any of his preceding determinations ; but it is easy to show, and I have done so in the last

edition of my System of Chemistry, that the true composition of sulphuric acid is

Sulphur.	100
Oxygen.	150

or 1 atom of sulphur = 2 + 3 atoms oxygen = 3.

We are now acquainted with four acid combinations of sulphur and oxygen ; namely,

	Sulphur.	Oxygen.
1. Hyposulphurous acid composed of. . .	1 atom	+ 1 atom
2. Sulphurous acid	1	+ 2
3. Sulphuric acid.	1	+ 3
4. Hyposulphuric acid.	2	+ 5

or this last acid may be considered as a compound of one atom of sulphurous acid and one atom of sulphuric acid.

Hyposulphurous acid makes its appearance in many cases not hitherto suspected. The knowledge of its properties is essential towards the knowledge of the phenomena which take place when the hydrosulphurets and hydroguretted sulphurets undergo spontaneous changes from exposure to the atmosphere. These changes will now admit of an easy explanation, since we are acquainted with the nature and properties of this peculiar acid, and of its salts.

Hyposulphuric acid has been only recently discovered by Gay-Lussac and Welter. They obtained it by passing a current of sulphurous gas through water, in which the black oxide of manganese was suspended. Sulphate and hyposulphates of manganese were formed. These salts were decomposed by means of carbonate of barytes. Nothing remained in solution but hyposulphate of barytes. It was crystallized, redissolved in water, and the barytes precipitated by the cautious addition of sulphuric acid. It appears from the experiments of these chemists that hyposulphuric acid is a compound of one atom of sulphuric and one atom of sulphurous acid. It may be concentrated to a certain point, but beyond that, if the concentration be urged, sulphurous acid escapes, and nothing remains but sulphuric acid. All the salts which this curious acid forms with bases appear to be soluble. (See *Annals of Philosophy*, xiv. 352.)

I translated and published in the *Annals of Philosophy*, xiv. 37, a paper by Sertürner on the action of sulphuric acid on alcohol. He concludes from his experiments, that a combination takes place between these two substances, and that three new acids may be formed to which he has given the names of *protiothionic*, *deutothionic*, and *tritiothionic acids*. These acids, he assures us, are capable of combining with the different bases, and of forming each a peculiar genus of salts. He assures us that this capacity of uniting is not confined to sulphuric acid

and alcohol, but extends to other acids, and to many vegetable substances.

These experiments of Sertürner have been repeated and confirmed by M. Vogel, of Munich, at least so far as the union of sulphuric acid and alcohol is concerned. He extracted the acid, combined it with different bases, and showed that the new acid of Sertürner possessed exactly the characters of hyposulphuric acid, with this difference, that it always holds in combination a peculiar oily matter, which somewhat modifies its properties. (*Jour. de Pharm.* vi. 1).

M. Vogel concludes from his experiments, that the theory of etherification advanced a good many years ago by Fourcroy and Vauquelin, is inaccurate; and that the objections started against that theory by M. Dabit, of Nantes, were well founded. M. Dabit actually formed the acid pointed out by Sertürner, and described several of the salts which it forms.

These important experiments of Vogel have been repeated and confirmed by Gay-Lussac (see *Ann. de Chim. et de Phys.* xiii. 62), who prepared sulphovinate of barytes (*sulphovinic acid* is the provisional name given to this acid by Vogel) in a state of purity, and subjected the acid to analysis. It crystallizes in fine rhomboidal prisms, terminated by four-sided pyramids, the faces of which correspond with those of the prism. They are transparent, and do not alter in the open air; but become opaque when kept under an exhausted receiver along with sulphuric acid: 100 parts of this salt dried in the air when calcined lost 45.07 parts, and furnished 54.93 parts of sulphate of barytes. The same quantity of salt calcined with chlorate and carbonate of potash, and afterwards precipitated by muriate of barytes, yielded 111.47 parts of sulphate of barytes, or nearly double what was obtained in the first experiment. Thus it appears that the acid possesses exactly the constituents and the capacity of saturation of hyposulphuric acid, and that the vegetable matter which it holds in combination produces no alteration in these particulars.

This curious subject deserves much fuller investigation than it has hitherto met with. Sertürner, though his experiments were far from precise, and his opinions very vague, has had the merit of drawing the attention of chemists to the action of sulphuric acid on alcohol, which was undoubtedly entitled to a much more accurate investigation than it has hitherto obtained. It was this that induced me to insert a translation of his paper in the *Annals of Philosophy*.

4. *Action of Nitric Acid on Uric Acid.*—When uric acid is dissolved in nitric acid, and the solution set aside for some time, transparent crystals are deposited, to which Brugnatelli, who first attended to these crystals, has given the name of *erythric acid*. The nature of these crystals has not hitherto been examined; nor do we know whether they be entitled to rank as a peculiar acid.

When pure uric acid is treated with nitric acid till the pink colour appears, Dr. Prout showed that a peculiar acid is formed, to which he gave the name of *purpuric acid*, and the peculiar properties of which were described in the historical sketch of last year.

It would appear from some notices published in the periodical publications, that Vauquelin has attempted without success to prepare purpuric acid. It is difficult to say to what cause the want of success of this very laborious and dexterous chemist is to be ascribed; but as I have myself had an opportunity of seeing purpuric acid, and even of examining its properties, it is not possible for me to entertain the least doubts about the reality of its existence.

5. *Ferrochyzic Acid*.—I have satisfied myself by the experiments to be stated in a future number of the *Annals of Philosophy*, that the constituents of ferrochyzic acid are the following:

2 atoms carbon.	= 1.500
1 atom hydrogen.	= 0.125
1 atom azote.	= 1.750
$\frac{1}{4}$ atom iron.	= 1.750
	5.125

so that it is a compound of five atoms (supposing the iron to be in reality one atom), and the equivalent number for it is 5.125. I have had the curiosity to analyze several of the ferrochyzates, and have had the satisfaction to find that the composition of all of them agrees very well with this equivalent number for the acid.

6. *Succinic Acid*.—A singular process of Dr. John by which he obtained *succinic acid*, is given in the *Annals of Philosophy*, xv. 388. I have not tried the experiment, and cannot pretend to form any opinion respecting it.

7. *Pyromalic Acid*.—It was observed by Vauquelin and by Braconnot, that when sorbic acid is distilled in a retort, it sublimes in part in white needles, and partly passes over into the receiver in the state of a liquid. When this liquid is evaporated, it yields acid crystals, differing in their properties from the acid from which they were produced. M. Lassaigne has examined the properties of this new acid, and has given it the name of *pyromalic acid*, in consequence of Braconnot's discovery of the identity of *sorbic* and *malic* acids.

These crystals are not altered by exposure to the air. They melt, when heated to the temperature of 117.5° , and, on cooling, assume the appearance of a pearl-coloured mass crystallized in diverging needles. When thrown upon red-hot coals, they fly off in an acid smoke, which excites coughing, without leaving any residue behind. When distilled in close vessels, the great-

ost part sublimes in long needles, but a portion is decomposed, as happens to most vegetable bodies.

Strong alcohol dissolves this acid very abundantly. Water, at the temperature of 50° , dissolves half its weight of it. The solution reddens vegetable blues, precipitates acetate of lead and nitrate of mercury in white flocks. It does not precipitate lime-water. It precipitates barytes water in a white powder, which is redissolved by the addition of water, and silvery plates gradually attach themselves to the sides of the vessel, which are pyromalate of barytes. This salt is a compound of

Acid	100·000	5·266
Barytes.	185·142	9·750

It would appear from this that the equivalent number for pyromalic acid is 5·25.

Pyromalate of potash crystallizes in feather-shaped crystals. It is somewhat deliquescent. Its solution in water does not precipitate the salts of iron, copper, manganese, zinc, nickel, or cobalt; but when dropped into nitrates of silver, mercury, or lead, a white flocky precipitate falls.

The combination of this acid with oxide of lead presents some remarkable phenomena. It is obtained at first in the state of white flocks, which, in a little time, assume the form of a semi-transparent jelly. When this jelly is diluted with water and separated by the filter, it collapses in proportion as the water leaves it, and after some time is completely crystallized in very brilliant pearly needles.

M. Lassaigne likewise examined the white crystals which sublime when sorbic acid is heated. He finds them to possess peculiar properties, and, therefore, considers them as entitled to rank as a peculiar acid. (See *Ann. de Chim. et de Phys.* xi. 93.)

8. *Menispermic Acid*.—M. Boullay made a set of experiments on the seeds of the *menispermum cocculus* in the year 1812, and extracted the ingredient to which these seeds owe their poisonous qualities. This substance he distinguished by the name of *picrotoxicine*. He has lately shown that it possesses alkaline properties, and that it exists in the seeds of the *menispermum cocculus* united to an acid which he considers as possessed of peculiar properties, and which he has distinguished by the name of *menispermic acid*.

This acid was extracted from the seeds by the following process: The seeds were macerated in five times their weight of water for 24 hours. They were then boiled for some time in a quantity of fresh water. Into the liquids thus obtained, subtriacetate of lead was dropped as long as any precipitate fell. The grey-coloured precipitate thus formed was mixed with water, and a current of sulphuretted hydrogen gas passed through it to separate the lead. The liquid was now heated to drive off the excess of sulphuretted hydrogen. Its taste was acid, though

still sensibly bitter. The acid was purified by repeated solutions in water and alcohol till it had lost its bitter taste. Thus purified, it possessed the following characters :

- (1.) It does not precipitate lime-water.
- (2.) With barytes it forms a somewhat soluble salt.
- (3.) It precipitates nitrate of barytes grey.
nitrate of silver, deep-yellow.
muriate of tin, yellow.
muriate of gold, reddish-brown.

(4.) It does not act upon the solution of protosulphate of iron, but, when dropped into the persulphate, it occasions a copious green precipitate, of a very dark colour.

(5.) It forms an abundant precipitate when dropped into the solution of sulphate of magnesia.

(6.) When treated with nitric acid, it is not converted into oxalic acid. (*Jour. de Pharm.* v. 6.)

9. *Pyro-uric Acid*.—This substance in a state of impurity was first noticed by Scheele. It was afterwards more particularly examined by Dr. Henry. MM. Chevallier and Lassaigne have lately procured it in a state of purity, and determined its properties and its composition.

When uric acid, or calculi, consisting chiefly of that substance are distilled in a retort, a sublimate arises in silvery white plates. This substance has been long known under the name of Scheele's sublimate. Chevallier and Lassaigne have ascertained it to be a pyro-urate of ammonia. They dissolved it in water, and precipitated the acid by means of subtriacetate of lead. The white precipitate, after being washed with water, was decomposed by means of sulphuretted hydrogen gas. The liquid being now evaporated yielded pure pyro-uric acid crystallized in small white needles. Its characters are as follows :

When heated, it melts, and sublimes entirely in white needles. When passed through a red-hot glass tube, it is decomposed into charcoal, oil, carburetted hydrogen, and carbonate of ammonia. Cold water dissolves about the fourth part of its weight of it. The aqueous solution reddens vegetable blues. It dissolves in boiling alcohol, and as the solution cools, precipitates again in small white grains. It dissolves in concentrated nitric acid. By evaporation to dryness, it is obtained without any alteration. This shows that pyro-uric acid differs from uric acid, which, by that process, is converted into purpurate of ammonia.

Lime combines with this acid, and forms a salt, which crystallizes in stalactites, and which has a bitter and slightly acrid taste. When slightly heated, it melts, and assumes on cooling the consistence of yellow wax. It is composed of

Acid	91.4	38.526
Lime	8.6	3.625

100.0

Pyro-urate of barytes is a white powder scarcely soluble in cold water. Potash, soda, and ammonia, form with it soluble salts, the two former of which are crystallizable.

The only metals precipitated by pyro-urate of potash are the peroxides of iron and copper, and the oxides of silver and mercury, and the subtritacetate of lead. The perpyro-urate of iron is chamois-yellow; that of copper, bluish-white; those of silver, mercury, and lead, perfectly white.

The subpyro-urate of lead obtained by decomposing pyro-urate of soda by means of subtritacetate of lead is composed of

Acid.	28.5	5.58
Protoxide of lead	71.5	14.00
		<hr style="width: 10%; margin: 0 auto;"/>	
			100.0

The equivalents for the pyro-uric acid derived from these two different analyses cannot easily be reconciled with each other, unless we suppose the subpyro-urate of lead to be a compound of one atom acid + seven atoms protoxide of lead, a supposition not very likely to be true.

Chevallier and Lassaigne analyzed pyro-uric acid by heating it with 20 times its weight of peroxide of copper in a glass tube. The only gases obtained were carbonic acid, and azotic in the proportion of four volumes of the former to one volume of the latter. They state the composition of pyro-uric acid as follows:

Oxygen	44.32
Carbon	28.29
Azote	16.84
Hydrogen	10.00
	<hr style="width: 10%; margin: 0 auto;"/>
	99.45

This is equivalent to the following volumes of these respective bodies, supposing them in the gaseous state :

	Volumes.
Oxygen	7
Carbon	12
Azote	3
Hydrogen	25½

Now this is equivalent to

14 atoms oxygen	=	14.0000
12 atoms carbon	=	9.0000
3 atoms azote	=	5.2500
25½ atoms hydrogen	=	2.1875
		<hr style="width: 10%; margin: 0 auto;"/>
		30.4375

According to this analysis, the equivalent number for an atom of pyro-uric acid is 30.4375—a number which neither agrees

with the analysis of pyro-urate of lime, nor subpyro-urate of lead. MM. Chevallier and Lassaigne have not been particular enough in their account of their analyses to enable us to see where the error lies. Had they given us the weight of water formed, and the bulk of the gases evolved, it would have been in our power to have applied the most correct data to establish the constitution of the acid. Such an omission is always greatly to be lamented. It renders their experiments much less valuable than they would otherwise have been. (See *Jour. de Pharm.* vi. 58).

VIII. ALKALIES.

Sertürner's paper on opium, and his discovery of the alkaline properties of morphia, has opened a new and a very extensive field to chemists, which hitherto has chiefly been occupied by the Parisian chemists connected with the pharmaceutical establishments in that capital. Already have four new alkaline substances been discovered and described by them, and the existence of four or five more has been announced. To each of these newly discovered substances, a name has been given by the discoverer, and these names, by the French chemists, have been made uniformly to terminate in *ine*. This was the termination which Lavoisier and his associates, when they formed the new chemical nomenclature, confined to vegetable and animal bodies. Alkaline bodies were made by them to terminate in *a*, and this rule has been uniformly observed by British chemists. I conceive, therefore, that as these new substances possess alkaline properties, it will be convenient in naming them to distinguish them from those animal and vegetable bodies that are destitute of the property of neutralizing acids, and to show their relationship to the class of alkaline bodies already known. This we can easily do by simply changing the French termination *ine* into *a*; therefore, instead of

Morphine,
Strychnine,
Brucine,

Delphine,
Picrotoxine,

I shall henceforth use the words,

Morphia,
Strychnia,
Brucia,

Delphia,
Picrotoxia.

And if the new vegetable alkaline bodies announced by Von Mons, as recently discovered by M. Brandes, prove peculiar bodies, we have only to call them

Datura,
Hyoscyama,
Aconita,

Atropa, and
Cicuta.

The readers of the *Annals of Philosophy* are already acquainted

with the properties of morphia and picrotoxia, and a brief account has been given of those of strychnia and brucia; but I think it requisite here to give the processes for obtaining these bodies, and to describe their characters, as well as those of delphia, of which hitherto no notice has been taken in the *Annals of Philosophy*.

1. *Strychnia*.—This alkaline substance was detected by Pelletier and Caventou in the fruit of the *strychnos nux vomica*, and *strychnos ignatia*, about the end of the year 1818. They at first gave it the name of *vauqueline*; but were induced to alter that appellation when it was represented to them that it was improper to give the name of so distinguished a chemist as M. Vauquelin to one of the most deleterious of the vegetable principles. It was obtained from the bean of the *strychnos ignatia* by the following process: The bean was rasped down as small as possible. It was then exposed to the action of nitric ether in a Papin's digester. The residue thus deprived of a quantity of fatty matter was digested in alcohol as long as that reagent was capable of dissolving any thing. The alcoholic solutions were evaporated to dryness, and the residue redissolved in water. Caustic potash being dropped into the solution, a white crystalline precipitate fell, which was strychnia. It was purified by washing it in cold water, dissolving it in alcohol, and crystallizing it. Strychnia was obtained likewise from the bean of the *strychnos ignatia* by boiling the infusion of the bean with magnesia in the same manner as Robiquet had obtained morphia from the infusion of opium.

The properties of strychnia, when in a state of purity, are as follows:

It is crystallized in very small four-sided prisms, terminated by four-sided low pyramids. It has a white colour, its taste is intolerably bitter, leaving a metallic impression in the mouth. It is destitute of smell. It is not altered by exposure to the air. It is neither fusible nor volatile, except at temperatures at which it undergoes decomposition. It is charred at the temperature at which oil enters into ebullition (about 580°). When strongly heated, it swells up, blackens, gives out empyreumatic oil, a little water and acetic acid; carbonic acid and carburetted hydrogen gases are disengaged, and a bulky charcoal remains behind. When heated with peroxide of copper, it gives out only carbonic acid gas and water. It is very little soluble in cold water, 100,000 parts of that liquid dissolving only 15 parts of strychnia; but it dissolves in 2,500 times its weight of boiling water. A cold solution of strychnia in water may be diluted with 100 times its volume of that liquid without losing its bitter taste.

When strychnia is introduced into the stomach, it acts with prodigious energy. A locked jaw is induced in a very short time, and the animal is speedily destroyed. Half a grain of strychnia

blown into the throat of a rabbit proved fatal in five minutes, and brought on locked jaw in two minutes.

Sulphate of Strychnia is a salt which crystallizes in transparent cubes, soluble in less than 10 times its weight of cold water. Its taste is intensely bitter, and the strychnia is precipitated from it by all the soluble salifiable bases. It is not altered by exposure to the air. In the temperature of 212° it loses no weight, but becomes opaque. At a higher temperature it melts, and speedily congeals again with a loss of three per cent. of its weight. At a still higher temperature it is decomposed and charred. Its constituents are :

Sulphuric acid	9.5	5.00
Strychnia.	90.5	47.63
	<hr style="width: 10%; margin: 0 auto;"/>		
	100.0		

Muriate of Strychnia crystallizes in very small needles, which are grouped together, and before the microscope exhibit the form of quadrangular prisms. When exposed to the air, it becomes opaque. It is more soluble in water than the sulphate; has a similar taste, and acts with the same violence upon the animal economy as all the other salts of strychnia. When heated to the temperature at which the base is decomposed, it allows the muriatic acid to escape.

Phosphate of Strychnia crystallizes in four-sided prisms. It can only be obtained neutral by double decomposition.

Nitrate of Strychnia can only be obtained by dissolving strychnia in nitric acid diluted with a great deal of water. The saturated solution, when cautiously evaporated, yields crystals of neutral nitrate in pearly needles. This salt is much more soluble in hot than in cold water. Its taste is exceedingly bitter, and it acts with more violence upon the animal economy than pure strychnia. It seems capable of uniting with an excess of acid. When heated, it becomes yellow, and undergoes decomposition. It is slightly soluble in alcohol, but is insoluble in ether.

When concentrated nitric acid is poured upon strychnia, it immediately strikes an amaranthine colour, followed by a shade similar to that of blood. To this colour succeeds a tint of yellow, which passes afterwards into green. By this action, the strychnia seems to be altered in its properties, and to be converted into a substance still capable of uniting with acids.

Carbonate of Strychnia is obtained in the form of white flocks, little soluble in water, but soluble in carbonic acid.

Acetic, oxalic, and tartaric acids form with strychnia neutral salts, which are very soluble in water, and more or less capable of crystallizing. They crystallize best when they contain an excess of acid. The neutral acetate is very soluble, and crystallizes with difficulty.

Hydrocyanic acid dissolves strychnia, and forms with it a crystallizable salt.

Strychnia neither combines with sulphur nor carbon. When boiled with iodine, a solution takes place, and iodate and hydriodate of strychnia are formed. Chlorine acts upon it precisely in the same way.

Strychnia, when dissolved in alcohol, has the property of precipitating the greater number of metallic oxides from their acid solutions. It is precipitated by the alkalies and alkaline earths; but the effect of the earths proper has not been tried. (See *Ann. de Chim. et de Phys.* x. 142).

2. *Brucia*.—This alkaline substance was obtained by Pelletier and Caventou from the bark of the *brucæa antidysenterica*, and made known to them about the middle of the year 1819. A brief account of its properties was inserted in the *Annals of Philosophy* for April last. It will be requisite to state in this place its most remarkable characters, that we may have it in our power to compare it with the other alkaline bodies contained in this part of our historical sketch.

Brucia was obtained from the bark of the *brucæa antidysenterica* by the following process: The bark was first digested in sulphuric ether, and then in alcohol. The alcoholic solution was evaporated to dryness, and the residue was dissolved in water. The *brucia*, which this solution contained, was saturated with oxalic acid, and the whole evaporated to dryness. This dry residue was digested in alcohol at the temperature of 32°, which dissolved the colouring matter, and left the oxalate of *brucia* in a state of purity. This oxalate being digested with lime or magnesia was decomposed, the oxalate of lime or magnesia remained undissolved, while the pure *brucia* dissolved in the water. *Brucia* thus obtained possesses the following properties:

It crystallizes in oblique prisms, with parallelograms for their bases. It is soluble in 500 times its weight of boiling water, and in 850 times its weight of cold water. This solubility is much increased by the presence of the colouring matter with which *brucia* is united in the alcoholic solution from the bark of the *brucæa antidysenterica*.

Brucia has a very bitter taste, but less so than strychnia; but it is sharper, more acrid, and continues for a long time. It produces exactly the same effects upon the animal economy, but acts only with one-twelfth of the energy that strychnia does. When exposed to the air it undergoes no change. When heated, it melts, and on cooling congeals into a mass, which has the appearance of wax. Its fusing temperature is a little higher than the boiling point of water. When strongly heated, it is decomposed, yielding much empyreumatic oil, a little water, acetic acid, carburetted hydrogen, and a very little carbonic acid. No trace of ammonia can be discovered among the products. When

treated with peroxide of copper, it yields only carbonic acid and water.

Brucia dissolves very well in sulphuric acid, and is capable of neutralizing it. The neutral sulphate of brucia crystallizes in long slender needles, which, when examined with a glass, appear to be four-sided prisms terminated by pyramids of uncertain form. It is very soluble in water, and somewhat soluble in alcohol. Its taste is very bitter. It is decomposed by the alkalis and alkaline earths; but no acid appears capable of separating the brucia from the sulphuric. When an excess of acid is added to the solution of this salt, it crystallizes more readily, and the crystals are larger. These crystals retain the excess of acid after repeated solutions and crystallizations. The sulphate of brucia is composed of

Sulphuric acid	8.84	5.000
Brucia.	91.16	51.561
<hr style="width: 50%; margin: 0 auto;"/>		
100.00		

Muriate of Brucia crystallizes in four-sided prisms terminated by an inclined face. It is not altered by exposure to the air, very soluble in water, and decomposed when exposed to a heat sufficient to destroy the vegetable base. The constituents of this salt are :

Brucia	5.9533
Muriatic acid.	94.0467
<hr style="width: 50%; margin: 0 auto;"/>	
100.0000	

Phosphate of Brucia does not crystallize unless it contain an excess of acid. In that case it forms pretty large rectangular tables with bevelled edges. It is very soluble in water. When exposed to the air, it effloresces somewhat.

Nitrate of Brucia does not crystallize unless it contain an excess of acid. It then forms very distinct acicular crystals consisting of four-sided prisms, terminated by two-sided summits.

When brucia is treated with strong nitric acid, a red colour is immediately produced, as happens when strychnia is exposed to the action of the same reagent, but the shade of colour is quite different. This change is no doubt owing to the same cause as the alteration of strychnia by nitric acid. Pelletier and Caventou are of opinion that the brucia is converted into a new substance, still possessing alkaline qualities, but a good deal weaker than before the action of the nitric acid.

Acetate of Brucia is very soluble, and does not seem capable of crystallizing; but *oxalate of brucia* crystallizes in long needles, especially if it contain an excess of acid.

Brucia is very soluble in alcohol, insoluble in sulphuric ether,

and the fat oils, and very little soluble in the essential oils. (See Ann. de Chim. et de Phys. xii, 113).

3. *Delphia*.—This alkaline substance was detected in July, 1819, by MM. Lassaigne and Feneulle, in the seeds of the delphinium staphysagria, or *stavesacre*, in which it exists united to malic acid. They obtained it from these seeds by the following process :

The seeds were cleaned, and reduced to a pulp : they were then boiled in water, and the liquid was separated by the filter. This liquid was boiled for some minutes with a quantity of pure magnesia. The solid part of the mixture was then separated by the filter, and digested in boiling alcohol. The alcoholic solution being evaporated left the *delphia* in a state of tolerable purity.

Delphia thus obtained is a white powder, having a crystalline texture while moist, but becoming opaque by exposure to the air. Its taste is very bitter and acrid. It is destitute of smell. When heated, it melts, and assumes the appearance of liquid wax ; on cooling, it becomes hard and brittle, like resin. At a still higher temperature, it swells up, blackens, gives out a white smoke with a peculiar odour, which takes fire in the air. It leaves behind it a light charcoal, which burns without leaving any residue.

Cold water has little action on it, yet it must dissolve a small quantity as it acquires an acrid taste. Alcohol and sulphuric ether dissolve it with facility. The alcoholic solution gives a green colour to syrup of violets, and restores the blue colour of litmus reddened by vinegar.

It unites with the acids, and forms neutral salts, which are very soluble in water, and have a very acrid and bitter taste. The alkalis precipitate the *delphia* from these salts in a white jelly like alumina.

Sulphate of Delphia does not crystallize ; but when exposed to spontaneous evaporation, dries into a hard transparent mass similar to gum. It dissolves readily in water and alcohol. Its solution has a bitter and acrid taste, which remains in the mouth for several hours. When a concentrated solution of this salt is acted on by a galvanic battery, the salt is decomposed, the sulphuric acid being deposited at the positive pole, and the *delphia* at the negative pole in white flocks. When sulphate of morphia is treated in the same way, it undergoes a similar decomposition, but the morphia is deposited in distinct white needles.

Nitrate of Delphia made by dissolving *delphia* in very weak nitric acid is colourless. When concentrated, it assumes a yellow colour ; evaporated to dryness, it has the same colour, and a crystalline aspect. When this nitrate is treated with an excess of acid, it is changed into a yellow matter, but little soluble in water. Boiling alcohol dissolves it with difficulty. Potash, ammonia, or lime-water, added to this solution does not occasion

any precipitate. The taste of this body was bitter. It did not appear to contain any nitric acid, and yet it was not possessed of alkaline properties. It was not altered by new doses of nitric acid, and no trace of oxalic acid ever appeared. Morphia, like strychnia and brucia, is rendered at first red by nitric acid; but the only colour induced on delphia by nitric acid is yellow.

Acetate of Delphia, when neutral, does not crystallize, but dries into a hard transparent mass, having a very bitter and acrid taste. It is easily decomposed (as is the case with the other acetates) by strong sulphuric acid.

Oxalate of Delphia has the form of white plates, and possesses the same taste as the other salts of delphia.

When delphia is heated with peroxide of copper, the only gas which it gives out is carbonic acid. Hence it is obvious that azote does not enter into its composition. (See *Ann. de Chim. et de Phys.* xii. 358.)

4. *Picrotoxia*.—M. Boullay has examined the properties of some of the salts of picrotoxia. It will be proper to state here the result of his latest observations.

Sulphate of Picrotoxia must be formed by dissolving picrotoxia in dilute sulphuric acid, for the strong acid chars and destroys it. The solution crystallizes on cooling. The sulphate of picrotoxia dissolves in 120 times its weight of boiling water. The solution gradually lets fall the salt in fine silky filaments disposed in bundles, and possessed of great beauty. When dry, it has a white colour, and feels elastic under the teeth, like plumose alum. It is composed of

Sulphuric acid	9.99	5
Picrotoxia	90.01	45
	<hr style="width: 50%; margin: 0 auto;"/>		
	100.00		

So that the equivalent number for picrotoxia, according to this analysis, is 45. It follows then next after Morphia, if these new alkaline bodies be arranged according to the weight of the atoms; for the respective weights, according to the present state of our knowledge, are as follows:

Morphia	40.250
Picrotoxia	45.000
Strychnia.	47.625
Brucia	51.500

Brucia is the heaviest primary body yet subjected to chemical analysis.

Nitrate of Picrotoxia.—Nitric acid, of the specific gravity 1.38, diluted with twice its weight of water, dissolves, when assisted by heat, the fourth of its weight of picrotoxia. When this solution is evaporated to one half, it becomes viscid, and on cooling is converted into a transparent mass, similar to a solution

of gum arabic. In this state the nitrate of picrotoxia is acid, and exceedingly bitter. If it be still further dried in a temperature not exceeding 140° , it swells up, becomes opaque, and becomes at last perfectly white and light, like calcined alum. If we keep it in this state at a temperature below that of boiling water, adding a little water occasionally, the whole excess of acid exhales, and the taste becomes purely bitter. When this salt is washed in pure water, the acid is totally removed, and the picrotoxia is separated in the state of fine white plates.

Muriate of Picrotoxia.—Muriatic acid, of the specific gravity 1.145, has little action on picrotoxia. It dissolves it when assisted by heat, but does not become entirely saturated. Five parts of this acid diluted with three times its weight of water dissolve about one part of picrotoxia at a strong boiling temperature. The liquor on cooling is converted into a greyish crystalline mass, composed of confused crystals. When these crystals are well washed, they are almost destitute of taste, and feel elastic under the teeth. They dissolve in about 400 times their weight of boiling water; but are almost entirely deposited on cooling. The solubility is much increased by the presence of an excess of acid.

Phosphate of Picrotoxia.—Phosphoric acid diluted with twice its weight of water dissolves but little picrotoxia while cold; but when assisted by heat, it acts pretty much in the same way as sulphuric acid, and the liquid crystallizes in the same manner, only the crystals are smaller. This phosphate, after being well washed, gives a red colour to vegetable blues. It dissolves in 50 times its weight of boiling water, and the solution becomes almost solid on cooling. The crystals are needles, retaining an excess of acid, and having an insupportably bitter taste.

Carbonate of Picrotoxia.—When carbonic acid is passed through water with which picrotoxia is mixed, a portion of the acid is absorbed by the picrotoxia; for it afterwards dissolves in acids with effervescence; but no solution takes place.

Acetate of Picrotoxia.—Acetic acid dissolves picrotoxia very well, and may be nearly saturated with it by the assistance of a boiling heat. On cooling, the acetate precipitates in well-defined prismatic needles. This acetate is soluble in 50 times its weight of boiling water. On cooling, it forms crystals of great beauty, light, without any acid smell, and much less bitter than picrotoxia itself. It is decomposed by nitric acid, which disengages the acetic acid. Dilute sulphuric acid has no marked action on it.

Tartrate of Picrotoxia.—Cold tartaric acid scarcely acts upon picrotoxia, but it dissolves that substance when assisted by heat, and on cooling, the salt gradually separates in triangular needles. This salt has a very bitter taste. It is soluble in 20 times its weight of boiling and in 160 times its weight of cold water. The solution of it, through much diluted with water, is precipitated by lime-water.

Oxalate of Picrotoxia.—Oxalic acid, next to acetic acid, seems to have the property of saturating picrotoxia most completely. The salt crystallizes in triangular plates, having a moderately bitter taste. It dissolves in 10 times its weight of boiling water, and is, therefore, the most soluble of all the salts of picrotoxia examined by Boullay.

Picrotoxia acts upon the living body as a virulent poison, occasioning convulsions, which speedily put an end to the life of the animal. Ten grains of it given to a middle-sized dog put an end to its life in this way in 45 minutes. The same dose of acetate of picrotoxia occasioned some diseased symptoms at first, but in three hours the animal recovered its wonted health and vigour. (See *Jour. de Pharm.* v. 1.)

5. Von Mons has announced that M. Brandes has found a new alkaline substance in the seeds of the following plants :

Datura stramonium,
Hyoscyamus niger,
Aconitum napellus?
Atropa belladonna,
Cicuta virosa.

He proposes to distinguish these five new alkaline bodies by names derived from the generic appellation of each plant. Hence we must call them respectively, *datura*, *hyoscyama*, *aconita*, *atropa*, *cicuta*. Nothing has hitherto transpired respecting the properties of these substances; but that the plants said to yield them actually do contain some poisonous substances, probably of an alkaline nature, there seems no reason to doubt. (See *Jour. de Pharm.* vi. 47.)

IX. SALTS.

1. *Solubility of the Salts in Water.*—A valuable paper on this subject has been published by M. Gay-Lussac, a translation of which has been inserted in the *Annals of Philosophy*, xv. 1. He has given us his method of determining how much of any salt water is capable of taking up. It consists in agitating the water with a greater quantity of the salt than it will dissolve at a given temperature till it ceases to take up any more. The liquid is now poured off, and a given weight of it is put into a balanced Florence flask. This flask is placed inclined on a sand-bath, and allowed to remain till the whole water is driven off. We have only now to weigh the flask again. The increase of weight obviously denotes the weight of salt contained in the weight of liquid subjected to evaporation.

M. Gay-Lussac endeavours to show that the quantity of any salt which water will dissolve at a given temperature, if allowed to act upon it for a sufficient length of time, is precisely the same as the water will retain in solution at that temperature, if it has been previously saturated at a boiling heat. I have no doubt

that the proposition is generally true, though I am not quite certain that it holds so universally as Gay-Lussac seems to think it does. I see no absurdity in supposing that the force of aggregation in certain salts may be too strong to be overcome without the assistance of heat, yet after it is once overcome, and the salt dissolved, the particles of it in the liquid may be at so great a distance from each other as not to unite together, and consequently to remain in solution in cold water. I do not indeed know any instance of this, though I think it very likely that some one or other will occur whenever the solubility of all the salts at different temperatures is investigated.

Gay-Lussac has given us ample details respecting the solubility of nine different salts in water of every temperature from 32° up to the boiling point of the saline solution. These solubilities he has represented in a plate, to which I refer the reader for ample satisfaction on the subject.

Of all these salts the least soluble is sulphate of potash. Water at 32° dissolves about 3.25 per cent. and boiling water dissolves about $25\frac{1}{3}$ per cent.

The most equably soluble salt in water of different temperatures is common salt. Water at 32° dissolves about $25\frac{1}{2}$ per cent.; while boiling water dissolves about 40 per cent.

The most singular salt in point of solubility is anhydrous sulphate of soda. It is most soluble in water of the temperature $91\frac{1}{2}^{\circ}$, as the temperature of the water increases beyond that point, the solubility of the salt slowly diminishes. Water at the temperature of 32° dissolves 5 per cent. of the salt; water at the temperature $91\frac{1}{2}^{\circ}$ dissolves $50\frac{1}{2}$ per cent.; while boiling water dissolves only $42\frac{1}{4}$ per cent.

The nitrate of potash is the salt whose solubility increases at the greatest rate with the temperature. Water at 32° dissolves only 12 per cent. of it; while water at $118\frac{1}{2}^{\circ}$ dissolves 80 per cent. of it.

The rate of the solubility of sulphate of potash, common salt, muriate of potash, muriate of barytes, and sulphate of magnesia, is represented by a straight line inclined at different angles to the axis according to the increase of solubility with the temperature.

The rate of the solubility of nitrate of barytes, chlorate of potash, sulphate of soda, and nitrate of potash, is represented by a curve convex towards the axis; and in sulphate of soda there is a point of contrary flexure corresponding to the temperature of $91\frac{1}{2}^{\circ}$, where the solubility of the salt is a maximum.

Gay-Lussac has given the equation of the line of solubility in several of the salts, and has shown the method of determining that equation. For the details, I refer to the paper with which my readers are doubtless acquainted.

2. I have inserted in the *Annals of Philosophy*, xv. 90, a set of analyses of various salts made by Berzelius, with all the care

of which he was capable, and giving results as near precision as the present state of our apparatus will admit. I have reason to be satisfied with these experiments, as, in every case, the new numbers obtained by Berzelius approach nearer to those which I have fixed upon from theoretical considerations as the true numbers denoting the atomic weights of the respective bodies. To expect an accurate coincidence between my numbers and those obtained by actual experiment would be unreasonable. I consider it as a sufficient indication of at least a very near approach to precision, that in proportion as greater precautions are taken, and greater accuracy attained, in chemical analyses, the numbers obtained approach nearer and nearer to those indicated by theory.

3. *Carbonates of Ammonia.*—When muriate of ammonia and carbonate of lime are exposed to heat, a salt is obtained usually distinguished by the name of subcarbonate of ammonia. This salt has been lately analyzed by Mr. Richard Phillips, and found composed of

3 atoms carbonic acid.	=	8·25	or	55·933
2 atoms ammonia.	=	4·25		28·813
2 atoms water	=	2·25		15·254
		14·75		100·000

It would appear from this, that during the distillation of the mixture of sal ammoniac and carbonate of lime, one atom of water and one atom of ammonia are dissipated; for if nothing were lost, it is obvious that the carbonate obtained would be a compound of

3 atoms carbonic acid,
3 atoms ammonia,
3 atoms water,

which is evidently the same thing as if it were composed of

1 atom carbonic acid	=	2·750
1 atom ammonia.	=	2·125
1 atom water	=	1·125
		6·000

When this subcarbonate is exposed to the air in a state of powder, it is well known that it loses its odour, and that it is converted into a bicarbonate of ammonia. Mr. Phillips has confirmed this by a new analysis of his own, from which it appears that this bicarbonate is composed of

2 atoms carbonic acid	=	5·500
1 atom ammonia.	=	2·125
2 atoms water.	=	2·250
		9·875

It had been hitherto believed that this change was owing to the escape of half the ammonia; but it is obvious from the preceding analysis of Mr. Phillips, that an atom of carbonate of ammonia (one atom carbonic acid + one atom ammonia) must escape.

The first of these salts then is a sesquicarbonate of ammonia, the second a bicarbonate. (See Quarterly Journal, vii. 294.)

4. *Carbonates of Soda*.—Mr. Phillips has shown that carbonic acid and soda are likewise capable of uniting with each other in three proportions; namely,

(1.) Carbonate composed of 1 atom carbonic acid + 1 atom soda + 11 atoms water. This is the common crystallized carbonate of soda of the shops.

(2.) Sesquicarbonate composed of three atoms carbonic acid + two atoms soda + four atoms water. This is the salt which occurs native in Africa, and is known by the name of *natron*. It is formed when a current of carbonic acid is passed through a solution of common carbonate of soda in water, or when such a solution is exposed to the action of an atmosphere of carbonic acid by being placed in a brewer's fermenting tun.

(3.) Bicarbonate composed of two atoms carbonic acid + one atom soda, and formed by forcing carbonic acid into a solution of carbonate of soda by means of a strong pressure. (See Quarterly Journal, vii. 296.)

5. *Hydrocyanate of Ammonia*.—In the *Annals of Philosophy*, xv. 394, I have given the characters of hydrocyanate of ammonia. I do not know whether these characters had been previously known to chemists; but they were new to me, and I have never met with any account of them in any chemical work. It precipitates the greater number of metals white, and the precipitates are often redissolved by agitation, or by nitric acid. I obtained the salt in colourless and transparent crystals, when prussian blue, or rather ferrochazate of iron, was exposed to a red heat in a copper tube.

6. *Carbonate of Potash*.—M. Guibourt has shown that when we wish to procure a pure carbonate of potash by burning a mixture of nitre and cream of tartar, we must take a mixture of two parts by weight of tartar for one part of nitre, otherwise we do not decompose the whole of the saltpetre. If the mixture be thrown in portions into a red-hot hessian crucible, a great deal of hydrocyanic acid, or rather of cyanogen, is formed, and on dissolving the residue in water, we obtain a cyanodide of potash which is not easily decomposed. To obtain the carbonate of potash pure, the best way is to throw the mixture into a crucible scarcely heated to redness. (Jour. de Pharm. v. 58.)

7. *Muriates of Potash and Soda*.—When 50 grammes of muriate of potash are dissolved in 200 of water contained in a vessel capable of holding 320 grammes of that liquid, the thermometer sinks during the solution $20\frac{1}{7}^{\circ}$; the same quantity of

common salt dissolved under like circumstances occasions a diminution of temperature amounting only to 3.4° . A method of determining the relative proportion of these two salts when mixed together has been founded on this property, which, from its extreme simplicity, deserves to be known. The formula will be simpler if we employ the centigrade thermometer: $20\frac{1}{2}^{\circ}$ of Fahrenheit is equal to 11.4° centigrade, and $3.4^{\circ} = 1.9^{\circ}$. The method of proceeding is this: Determine the degree of cold produced during the solution of the salt, or the number of degrees that the thermometer sinks, let this be d . Then the quantity of muriate of potash in the mixture = $\frac{100d - 190}{9.5}$.

The thermometer ought to be delicate; the salt ought to be in the state of a fine powder, and the solution ought to be made as rapidly as possible. The process is this: Weigh out 200 grammes of water, and determine its temperature. Let us suppose it 20.4° . Pour into the water as rapidly as possible 50 grammes of the salt previously reduced to powder. Hold the vessel by the neck (a glass phial, for example), and agitate the water, giving it a circular motion till the salt be dissolved. Observe at what degree the thermometer stands at the end of the solution, suppose it 12.8° . Then we have $d = 20.4^{\circ} - 12.8^{\circ} = 7.6$. Of course the muriate of potash in 100 parts of the mixed salt is $\frac{100 \times 7.6 - 190}{9.7} = 60$ very nearly. (Ann. de Chim. et de Phys. xii. 42.)

8. *Anhydrous Carbonate of Copper.*—In the Philosophical Transactions for 1813, I published the analysis of an anhydrous carbonate of copper from India. MM. Colin and Tallefert have ascertained that when the green or blue carbonate of copper is boiled in a quantity of water, it becomes of a fine brown, in consequence of the escape of its water. Thus they have discovered the curious fact, that the hydrous carbonate of copper may be rendered anhydrous simply by boiling it under water. The brown powder which they thus formed is precisely the same as the native anhydrous carbonate from India which I analyzed. Thus all the three carbonates of copper may be made artificially.

These gentlemen have ascertained likewise that the protoxide of copper is capable of combining with carbonic acid, and forming a yellow-coloured carbonate, which is permanent in close vessels, but in the air gradually imbibes oxygen, and becomes brown. (Ann. de Chim. et de Phys. xii. 62.)

9. *Acetate of Lead.*—In the *Annals of Philosophy*, xiv. 382, I have given the properties and analysis of a new variety of acetate of lead, which was accidentally formed in the sugar of lead manufactory of Charles Macintosh, Esq. of Glasgow. The crystals were white and translucent, and much larger than those of common sugar of lead. They consisted of rhomboidal prisms, the plane angles of which measured 106° and 74° . Each prism

was terminated by a dihedral summit formed by two faces proceeding from the narrow faces of the prism, and meeting at an angle of 130° . I found them composed of

4 atoms acetic acid	= 21.85
5 atoms oxide of lead.	= 60.00
19 atoms water.	= 18.15
	100.00

So that it is a subacetate with the addition of one-fourth of an atom of oxide of lead over and above what exists in common sugar of lead. This salt has been occasionally formed in other manufactories. I have a very fine specimen of it from Mr. Ramsay, of Glasgow.

10. *Salts of Mercury*.—Mr. Donovan's paper has made us much better acquainted with several of the salts of mercury than we were before.

(1.) *Nitrate of Mercury*.—When mercury is dissolved in pure nitric acid, whether with or without the assistance of heat, a number of crystals speedily make their appearance, which Mr. Donovan has shown to be pure protonitrate of mercury. This salt is white and opaque. If exposed to the air, it becomes yellow, by losing a portion of its acid. It does not dissolve in water, unless the liquid be acidulated with nitric acid. When this salt is digested in a sufficient quantity of water, it is converted into a blue-grey matter, which consists of a subnitrate of mercury.

The pernitrate of mercury does not crystallize, and, if evaporated to dryness, is deliquescent. This salt remains behind after the protonitrate of mercury has separated in crystals from the solution of mercury in nitric acid.

(2.) *Sulphate of Mercury*.—When sulphuric acid is heated over mercury so gently as merely to effervesce, a white salt is speedily deposited, which is a protosulphate of mercury. It dissolves very sparingly in sulphuric acid, and is decomposed by water. There is another variety of this salt; namely, the subprotosulphate, which may be obtained by mixing together the solutions of protonitrate of mercury and sulphate of soda. The precipitate which falls is white, and consists of subprotosulphate of mercury. When protoxide of mercury and sulphuric acid (of the specific gravity 1.09) are triturated together, the colour of the oxide changes to grey, but none of it dissolves. This likewise (when welledulcorated) is a subprotosulphate of mercury.

When sulphuric acid is violently boiled on any quantity of mercury, the metal is first brought to the state of black oxide; but by continuing the heat, it is converted into peroxide. If a mixture of three parts strong acid and two parts mercury by weight be boiled together for a sufficient time, the whole is converted into persulphate of mercury. This salt, like the other,

is deprived of a portion of its acid by water, and converted into subsulphate.

Mr. Donovan has not subjected any of the persulphates of mercury to analysis; had he done so, I believe he would have changed his nomenclature a little. The white persulphate is in reality a bipersulphate, while the yellow powder obtained by washing the former salt in hot water is a neutral persulphate.

11. *Nitrate of Silver*.—The method of obtaining a pure nitrate of silver from standard silver, which is an alloy of that metal with copper, given in the *Annals of Philosophy*, xv. 289, deserves the attention of practical chemists. It consists in this: The alloy is dissolved in nitric acid, and the solution evaporated to dryness in a glass vessel. It is then put into an iron spoon, and exposed to a moderate heat, keeping the salt in fusion till all ebullition is at an end. By this heat the nitrate of copper is decomposed, while the nitrate of silver remains unaltered. Dissolve the salt in water, filter and evaporate the colourless solution, and we obtain nitrate of silver in a state of purity.

12. *Muriate of Gold-and-Soda*.—This is a triple salt, which has been prepared by M. Figuier, of Montpellier. It is employed in medicine for the purposes recommended by M. Chretien. The method of preparing it is abundantly simple. Gold is dissolved in aqua regia, and the solution made as neutral as possible by evaporating to the consistence of a syrup. It is then diluted with water, and mixed with a solution of common salt. By the requisite concentration, permanent crystals of the triple salt are obtained. The figure of the crystals is a thin rectangular parallelepiped, which does not attract moisture from the atmosphere. The constituents of this salt, according to the analysis of Berard and Figuier (with the details of which, however, they do not furnish us) are as follows:

Muriatic acid.	19.75
Oxide of gold	50.76
Soda.	8.50
Water.	20.99
	<hr/>
	100.00

(*Journ. de Pharm.* vi. 64.)

13. *Ferrochyzate of Iron*.—My experiments on this salt have so recently made their appearance in the *Annals of Philosophy*, xv. 392, that it seems to be necessary merely to allude to them. It is a neutral salt, when pure, composed of one atom of ferrochyzic acid + one atom peroxide of iron. I am inclined to ascribe the pyrophorous quality which the residue of this salt, after exposure to a red heat in close vessels, is known to possess, to the presence of a little potash, from which it is exceedingly difficult to free the salt. This potash is probably converted into potassium during the process of heating, and the rapidity with which it

absorbs oxygen, when afterwards exposed to the air, doubtless occasions the combustion.

14. *Hyposulphites*.—Mr. Herschell has published an excellent set of experiments on these curious salts, and has made us much better acquainted with them than we were before. He has found that if quicklime, sulphur, and water, be boiled together for some time, and if through the filtered liquid a current of sulphurous acid gas be passed, the colour of the liquid is destroyed, and it becomes a solution of hyposulphite of lime in water. By evaporation, this hyposulphite is obtained in crystals. It was by means of this salt that most of the hyposulphites which he subjected to examination were formed.

The hyposulphites seem to be mostly soluble in water. Their taste is either intensely bitter, or intensely sweet.

(1.) *Hyposulphite of Lime*.—This salt usually crystallizes in irregular six-sided prisms, whose faces are inclined to each other at angles of $141^{\circ} 39'$; $110^{\circ} 45'$, and $107^{\circ} 36'$. They refract doubly. They dissolve very readily in water; that liquid at the temperature of 37° dissolves nearly its own weight of them, and during the solution, the thermometer sinks to 31° . The specific gravity of a saturated solution at 50° is 1.300; and when the specific gravity is 1.114371 at 60° of temperature, the solution contains 0.2081 of its weight. These crystals are not altered by exposure to the air unless it be very dry. From Mr. Herschell's experiments, this salt appears to be composed of

2 atoms hyposulphurous acid.	= 6.000
1 atom lime.	= 3.625
6 atoms water.	= 6.750

16.375

(2.) *Hyposulphite of Potash*.—This salt crystallizes in a confused mass of spiculæ. It has a penetrating taste, like nitre, succeeded by bitterness, and deliquesces speedily when exposed to the air. When heated, it dries down to a white mass, then takes fire, and burns much like a piece of tinder.

(3.) *Hyposulphite of Soda*.—It crystallizes in silky tufts radiating from centres, which at length extend through the whole liquid, and become almost solid. Its taste is intensely bitter and nauseous. When heated, it undergoes the watery fusion, then dries into a white mass, and at length takes fire, burning with a vivid deflagration, and a bright-yellow flame.

(4.) *Hyposulphite of Ammonia*.—This salt does not easily crystallize regularly. Its taste is biting pungent, succeeded by a disgusting bitterness. When heated, it burns with a weak flame, and evaporates entirely.

(5.) *Hyposulphite of Barytes*.—This salt is a white, brilliant, scaly powder, which is soluble in dilute muriatic acid; but which requires more than 2000 times its weight of water to dissolve it. When heated on platinum foil, it is thrown into a

singular agitation, and seems enveloped in a kind of fog, caused by its own dust thrown up in an infinite number of minute explosions. According to Mr. Herschell's analysis, it is a compound of two atoms acid + one atom barytes.

(6.) *Hyposulphite of Strontian.*—This salt crystallizes in flat rhombs, having the plane angles of their more extended surfaces about $64^{\circ} 45'$ and $115^{\circ} 15'$; but whose solid form is that of an oblique parallelepiped, whose sides are inclined to each other at angles of about $76^{\circ} 30'$, $96^{\circ} 45'$, and $97^{\circ} 13'$. It refracts doubly. At the temperature of 45° , it dissolves in four times its weight of water, while it dissolves in $1\frac{3}{4}$ times its weight of boiling water. Its taste is purely bitter. It is insoluble in alcohol. It dissolves chloride of silver, and alcohol precipitates the solution in the state of a sweet syrup.

(7.) *Hyposulphite of Magnesia.*—This salt is easily formed by boiling a solution of sulphite of magnesia with flowers of sulphur. It readily crystallizes, is intensely bitter, dissolves readily in water, but does not deliquesce when exposed to the air. When laid on a hot iron it burns with a weak blue flame, but is incapable of maintaining the combustion per se.

(8.) *Hyposulphite of Copper.*—This salt may be obtained by digesting hyposulphite of lime on carbonate of copper. It is colourless, has an intensely sweet taste without any metallic flavour. It is not decomposed by ammonia, nor turned blue by an excess of that alkali provided the air be excluded. Hence the copper in it is in the state of a protoxide.

(9.) *Hyposulphite of Lead.*—A white mealy powder obtained by pouring nitrate of lead into a neutral hyposulphite. When held long in the mouth, it leaves a sweet impression. For solution it requires 3266 times its weight of water. At a temperature rather under 212° , it becomes black, and when the heat is raised, takes fire, becoming red-hot, and burns with a weak flame. If it be now removed from the fire, the ignition and combustion may be maintained for any length of time by cautiously adding small quantities of the salt. According to the analysis of Mr. Herschell, this salt is composed of two atoms acid + one atom protoxide of lead.

(10.) *Hyposulphite of Silver.*—This salt may be formed by adding nitrate of silver to a diluted solution of a hyposulphite. It has an exceedingly sweet taste. In a short time it is decomposed, and sulphuret of silver is precipitated.

(11) *Hyposulphite of Mercury.*—Mr. Herschell's experiments to procure this salt did not lead to very satisfactory results. It seems to follow from them that hyposulphurous acid is capable of combining with the peroxide, but not with the protoxide of mercury.

Mr. Herschell has shown that the hyposulphite of silver has the property of combining with several of the other hyposulphites,

and forming double salts, which have some permanency. Of these he has described the following:

Hyposulphite of potash-and-silver,
 Hyposulphite of soda-and-silver,
 Hyposulphite of ammonia-and-silver,
 Hyposulphite of lime-and silver,
 Hyposulphite of strontian-and-silver,
 Hyposulphite of lead-and-silver,

Though the existence of this last salt has not been made out in a satisfactory manner. (See Edinburgh Philosophical Journal, i. 8 and 396.)

15. *Muriates*.—The most generally received opinion at present respecting the muriates is, that when dried so as to be totally freed from water, they are in reality compounds of chlorine, and the metallic base of the alkaline ingredient. This opinion is chiefly founded upon the experiments of Gay-Lussac and The-nard, and of Sir H. Davy. According to these gentlemen, when the muriates previously reduced to a state of complete dryness are heated with perfectly dry phosphoric or boracic acid, no decomposition takes place; but if water be present, the muriate is decomposed, and a quantity of muriatic acid is driven off. M. Vogel, of Munich, has lately repeated these experiments apparently with great care. He fused pure phosphoric acid and muriate of barytes in separate platinum crucibles. They were then reduced to powder, introduced into a platinum tube, and subjected to a heat sufficiently strong to produce fusion. A copious evolution of muriatic acid gas was the consequence. The result was the same when fused muriate of tin, or muriate of manganese, was employed. When boracic acid previously fused was heated with dry muriate of potash, or of soda, muriatic acid gas was likewise evolved. (Jour. de Pharm. v. 61.)

The obvious consequence deducible from these experiments is, that we are not able to deprive phosphoric acid or boracic acid of the whole of its water by exposure to a red heat.

16. *Sulphate of Magnesia*.—M. Longchamp has published a most elaborate set of experiments on this salt. He finds its composition to be as follows:

Sulphuric acid	33·751	5·000
Magnesia	...	13·249	1·962
Water	53·000		
				100·000

According to this analysis, the weight of an integrant particle of magnesia would be 2, and an atom of magnesium 1. This differs so materially from preceding analyses, that chemists cannot adopt it till the subject undergo further investigation.

According to M. Longchamp sulphate of magnesia cannot be deprived of its water unless it be exposed to a white heat. Hydrate of magnesia he finds composed of

Magnesia.	52·997
Water	47·003
	<hr/>
	100·000

After being exposed to a red heat, this hydrate still continues composed of

Magnesia.	79·218
Water	20·782
	<hr/>
	100·000

According to M. Longchamp the best way of separating lime from magnesia is to precipitate by carbonate of ammonia, which throws down the lime in the state of a carbonate without throwing down any magnesia. The magnesian solution is then to be evaporated to dryness, and calcined to drive off the ammonia. The magnesian salt is now to be dissolved in water, and the magnesia precipitated by caustic potash.

He finds that the carbonate of magnesia is soluble in sulphate, nitrate, and muriate of potash and soda. (*Ann. de Chim. et de Phys.* xii. 255.)

17. *Salts of Nickel.*—M. Berthier has published some recent analyses of different salts of nickel which it may be worth while to state here.

(1.) *Arseniate of Nickel.*—This salt was obtained by mixing arsenic acid with muriate of nickel, and precipitating by an excess of an alkaline carbonate. This arseniate he found composed of very nearly equal parts of acid and base. This would be nearly a compound of one atom arsenic acid and three atoms protoxide of nickel.

(2.) *Sulphate of Nickel.*—This salt, according to the analysis of Berthier, is composed of

Sulphuric acid	52·2	5·000
Protoxide of nickel.	47·8	4·578
	<hr/>	
	100·0	

(3.) *Carbonate of Nickel.*—This salt is composed of

Carbonic acid	14·0
Protoxide of nickel.	47·5
Water	38·5
	<hr/>
	100·0

This carbonate is of a fine apple-green colour. It is obtained

by precipitating a solution of nickel with an alkaline carbonate. When a bicarbonate is employed to precipitate the nickel, the precipitate is much paler, and appears to be a sesquicarbonate; for Berthier found it composed of

Carbonic acid	21·0
Protoxide of nickel.	48·3
Water	30·7
	100·0

(Ann. de Chim. et de Phys. xiii. 57.)

(*To be continued.*)

ARTICLE II.

On a fibrous Metallic Copper. By James Smithson, Esq. F.R.S.

(To Dr. Thomson.)

SIR,

Paris, March 17, 1820.

THERE occur, in mineral collections, pieces of a copper slag, having fibres of metallic copper in its cavities. I have seen this fibrous copper erroneously placed among native coppers.

I possess samples of this kind from a foundery in the Hartz. The metallic copper in the cavities, or air-holes, is so delicately slender as to be a metallic wool.

From several considerations, it appeared to me to be beyond all doubt that the opinion of these fibres having been produced by crystallization was perfectly inadmissible; and I was for a very long time totally unable to come to any conjecture with respect to the mode in which they had originated.

Looking on one of these specimens this morning, an idea struck me which is, I am convinced, the solution of this knotty problem.

It occurred to me that these fibres had been generated at the instant of consolidation of the fused slag. That by its shrinking at that moment, it had compressed drops of copper, still in a fluid state, dispersed in its substance, and squeezed a portion of it through the minute spaces between its particles, under this fibrous form, into its cavities, or air-holes.

For this operation to take place, the concurrence of several conditions is required. The slag must be so thick and pasty as to retain metallic copper scattered through it. It must have developed bubbles of some gas which have occasioned vacuities in it. It must be less fusible than the copper, but in so very small a degree that the copper consolidates as the fibres of it are formed.

It is evident that on this supposition these fibres of copper are produced by a process entirely the same as that employed

for the manufactory of macaroni and vermicelli; and which are made by forcing paste through small apertures by the pressure of a syringe. It is wire-drawing performed inversely—by propulsion instead of traction.

As soon as this hypothesis had presented itself to me, I became anxious to ascertain whether I could give birth to this fibrous copper at the blow-pipe. I melted a small fragment of the slag; and, on breaking it, I had the gratification of finding its little cavities lined with minute fibres of metallic copper as those of its greater prototype.

I wished now to form the slag itself which was to afford the copper fibres. As I had ascertained the slag of the Hartz to consist of sulphur, copper, and iron, I had recourse to the yellow sulphuret of copper and iron. To produce the required portion of metallic copper, I calcined some small fragments of this yellow ore at the tip of the exterior flame. Finding that I had exceeded the proper point, and rendered them too infusible, I added a little of the raw ore; and after encountering a few difficulties succeeded in producing a little mass of slag, whose internal cavities presented me, on breaking it, with the fibres of copper which were the object of my toil.

A repetition of these experiments in a furnace, on a larger scale, would undoubtedly have yet more successful results.

It deserves to be noticed that the curved form which these fibres of copper generally have is entirely favourable to the foregoing theory of their formation, and equally contrary to the supposition of their being produced by crystallization.

The power to which has been ascribed the phenomenon which forms the subject of these pages has hitherto been overlooked. It has not been considered what the effects might be of the contraction of a melted mass at the moment of its congelation. It is, however, a means of effects which may have acted on many occasions in the earth. Two matters of unequal fusibility, and of no attraction to each other, are not unlikely to have occurred blended in a state of fusion; and then the most fusible to have become pressed out from between the particles of the other when it solidified. If some evolved vapour had opened cavities in the mass, or rents had formed in it, the fluid matter will have escaped from the pressure into these voids, as has happened with the copper. If these receptacles for it have been wanting, it must have flowed to the external surfaces, and may have formed a crust there. The matter which lines or fills the cavities of some lavas has, perhaps, been so introduced into them.

A knowledge of the productions of art, and of its operations, is indispensable to the geologist. Bold is the man who undertakes to assign effects to agents with which he has no acquaintance; which he never has beheld in action; to whose indisputable results he is an utter stranger; who engages in the fabrication of a world alike unskilled in the forces and the materials which he employs.

ARTICLE III.

An Account of a native Combination of Sulphate of Barium and Fluoride of Calcium. By James Smithson, Esq. F.R.S.

(To Dr. Thomson.)

SIR,

Paris, March 24, 1820.

I ACQUIRED this substance in Derbyshire. It is many years since I ascertained its constitution. I have examined several minerals which in appearance bore a resemblance to it, but have not found any of them to be of the same nature. This species would hence appear to be of rare occurrence in the earth.

This substance formed a vein about an inch wide in a coarse shell limestone. Next to this substance was a layer of crystals of sulphuret of lead; and between these and the limestone rock a layer of crystals of carbonate of calcium.

I infer that these matters filled a vertical fissure in the limestone stratum; and from the ideas I entertain of the mode by which such fissures have generally become occupied by their contents, I believe them to have been successively deposited in it by sublimation, either through the intense vehemence of subterranean fire, or by the agency of the vapour of water, or of some other gas.

This compound matter bears in its general appearance so strong a resemblance to fine compact grey limestone that the eye can probably not distinguish between them.

Forty-two grains of it lost 11·2 grs. in rain water at the temperature of 61° Fahr.; consequently its density is 3·750.

These 42 grs. of this stone by laying in the water did not absorb into their substance a quantity of it equal to one-tenth of a grain.

It does not mark glass, and is readily scraped to a powder by a knife. It marked sulphate of barium. Its hardness and that of fluoride of calcium appeared to be the same.

It showed no electricity by heat. By friction it readily became electrified.

In the fire it lost no weight.

At the blow-pipe, it readily melted. The little bead while in fusion was transparent. On evolving, it became opaque. The transparency of the bead in a melted state is best seen with a very minute one. On fusing this matter long, it spreads on the coal, and becomes a refractory mass.

With borax, it dissolved with great effervescence into a brown glass. If much stone was used, the glass appeared quite black, but drawn out to a thread with the tongs, it was found to be of a fine hyacinth colour. These colours depend on the formation of sulphur.

With microcosmic salt it fused with effervescence to a clear

colourless glass, which became opaque, and white on adding more of it.

A particle of this stone which had been *fused* on the charcoal being laid in a drop of water on a plate of silver, immediately made a black spot of sulphuret of silver on it.

This bit of melted stone, transferred to a drop of marine acid, on a piece of glass, partially dissolved with effervescence. The solution let exhale spontaneously, afforded crystals of chloride of barium.

Some of this stone in fine powder, being heated in a drop of sulphuric acid on a bit of glass, the polish of the glass was destroyed.

Water in which this stone in fine powder had been boiled was not affected by solution of nitrate of lead.

A bit of this stone, being heated in dilute marine acid, emitted a few bubbles of carbonic acid, but was not otherwise affected: 5.4 grs. of this mineral in very fine powder were let remain in an excess of marine acid till all action on them had ceased. The undissolved portion washed and gently ignited weighed 5.15 grs. The acid had acquired lime; so that this mineral contains a mechanical admixture of $\frac{4.6}{100.0}$ of carbonate of calcium.

This fine powder, which had been treated with the marine acid, had sulphuric acid evaporated to dryness on it in a platinum crucible. It was then digested in dilute marine acid. On evaporating this solution, a large quantity of sulphate of calcium in crystals was obtained.

From these results, sulphuric acid, fluorine, barytes, and lime, appear to be the elements of this mineral. It is consequently inferable that its proximate principles are sulphate of barium and fluoride of calcium.

The following experiments were made to obtain some idea of the proportions in which these two compound components of this mineral exist in it:

5.6 grs. of this stone in powder were heated in a platinum crucible in so large a quantity of sulphuric acid as to be entirely dissolved. The mixture was then exhaled dry, and ignited. The weight was now 7.85 grs. The increase had, therefore, been as $\frac{40}{100}$.

This augmentation of weight could arise only from the change of the fluoride of calcium into sulphate of calcium.

To know to what quantity of fluoride of calcium it corresponded, two grs. of pure fluoride of calcium in subtile powder were treated with sulphuric acid till the augmentation of weight ceased. The two grains had then become 3.65 grs.; accordingly the augmentation of weight was $= 1.65 = \frac{83}{100}$.

∴ This Derbyshire mineral, therefore, consists of

Sulphate of barium	51·5
Fluoride of calcium	48·5
	100·0

Some error is created by the admixed carbonate of lime; and which had not been removed.

This mineral presents us with a remarkable case of combination; that of a neutral salt with a body which is not a salt, but belongs to an order which is analogous to metallic oxides. I have met with another instance of the same kind. I have examined transparent crystals which were composed of anhydrous sulphate of calcium and chloride of sodium.

These combinations of their compounds may, however, perhaps, appear to some persons to cast doubts on the opinion that chlorine and fluorine are not acids.

These compounds will still be deserving of particular attention from consisting of *four* matters.

ARTICLE IV.

ANALYSES OF BOOKS.

Philosophical Transactions of the Royal Society of London, for 1819, Part II.

(Concluded from vol. xv. p. 447.)

VII. *On some new Methods of investigating the Sums of several Classes of infinite Series.* By Charles Babbage, Esq. A.M. F.R.S.

VIII. *On the Optical and Physical Properties of Tabasheer.* By David Brewster, LL.D. F.R.S. Lond. and Edin.—The tabasheer is a concretion in the bamboo, at first gelatinous, but gradually changed into a white solid. Mr. Smithson first ascertained that it consisted chiefly of silica, and afterwards Fourcroy and Vauquelin analyzed it, and found it a compound of 70 silica and 30 potash and lime. Dr. Brewster found that when the tabasheer is immersed for a short time in water, it becomes opaque; but a long emersion renders it transparent. This curious property he found to depend upon the uncommonly small power which the tabasheer has to refract light. The absolute refractive power of air being 4530, he found that of the tabasheer to be only 976·1. This is on the supposition that the specific gravity of the tabasheer is 2·4. If we were to reckon it only 0·66, as has been suggested by a member of the Royal Society, the anomaly would in a good measure disappear; and this would approach the specific gravity if we were to take into con-

sideration the air which exists in such abundance in the pores of the body.

IX. *An Account of a Membrane in the Eye, now first described.* By Arthur Jacob, M.D. Member of the Royal College of Surgeons in Ireland, Demonstrator of Anatomy, and Lecturer on Diseases of the Eye in the University of Dublin.

It has been the general opinion of anatomists, that the retina consists of two portions or membranes; 1, an expansion of the nerve, next the choroid coat; 2, a vascular layer next to the vitreous humour. It is generally admitted that the medullary expansion of the nerve cannot be separated so as to present the appearance of a distinct membrane, though it may be scraped off, and even portions of it detached, leaving the vascular layer perfect. Dr. Jacob has found that besides these two layers the retina is covered on its external surface by a delicate transparent membrane united to it by cellular substance and vessels. This structure, not hitherto noticed by anatomists, he has had so many opportunities of demonstrating as to leave no doubt whatever of its existence. He considers it as a distinct and perfect membrane, apparently of the same nature as that which lines serous cavities. He describes at full length the method of demonstrating it.

The author terminates his paper by observing that the different coats of the eye are not merely in contact, but are likewise united together by cellular substance and delicate vessels.

X. *A new Method of solving numerical Equations of all Orders by continuous Approximation.* By W. G. Horner, Esq.—I regret that it is inconsistent with the plan of this work to give an analysis of this valuable and ingenious paper here. The reader will find it well deserving his attention.

Philosophical Transactions of the Royal Society of London for the Year 1819, Part III.

This part contains only a single paper; namely,

An Account of Experiments for determining the Variation in the Length of the Pendulum Vibrating Seconds at the principal Stations of the Trigonometrical Survey of Great Britain. By Capt. Henry Kater, F.R.S.—In the year 1816 an address was presented by the House of Commons to the Prince Regent praying him to give directions for ascertaining the length of the pendulum vibrating seconds of time in the latitude of London as compared with the standard measure in the possession of the House of Commons, and for determining the variations in the length of the said pendulum at the principal stations of the trigonometrical survey, extended through Great Britain. In consequence of this address, an application was made by his Majesty's ministers to the Royal Society, requesting the assistance of that body. A committee was appointed for the purpose, and Capt. Kater was selected to make the experiments.

The result of his trials to determine the length of the seconds pendulum in London was given in a preceding volume of the Transactions; but in the present paper he corrects an error in his previous determination from his not having accurately ascertained the specific gravity of his pendulum.

He took the opportunity of his experiments to verify Col. Mudge's determination of the latitudes at Clifton, Arbury Hill, and Dunnose, which Don Joseph Rodriguez, in an ingenious paper published in the Phil. Trans. for 1812, had supposed erroneous to the amount of about five seconds, in order to get rid of a very remarkable anomaly observed by Col. Mudge; namely, the degrees of the meridian instead of *increasing* with the latitude actually decreasing. The result of his observations is a confirmation of the latitudes of those places as already determined by Col. Mudge; so that Don Joseph Rodriguez's explanation turns out insufficient.

Capt. Kater in the present paper gives a minute account of his apparatus, and of the different observations which he made. The following table exhibits the ultimate results of his trials which appear to have been conducted with every possible attention to accuracy:

Places of observation.	Latitude.	Vibrations in a mean solar day.	Length of the pendulum vibrating seconds in parts of Sir George Shuckburgh's scale.
Unst.	60° 45' 28·01''	86096·90	39·17149
Portsoy	57 40 58·65	86086·05	39·16159
Leithfort	55 58 40·80	86079·40	39·15554
Clifton.	53 27 43·12	86068·90	39·14600
Arbury Hill . . .	52 12 55·32	86065·05	39·14250
London.	51 31 8·40	86061·52	39·13929
Shanklin Farm.	50 37 23·94	86058·07	39·13614

ARTICLE V.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

May 18.—A paper by his Excellency Governor Sir Stamford Raffles was read, entitled "Some Account of the Dugong." The general form of this animal resembles that of the other cetaceæ. The skin is smooth, and about three-quarters of an inch thick, with a few scattered hairs, and the head is small in proportion to the size of the animal. There are two thick tusks

projecting from the extremity of the upper jaw. The position of the incisor teeth is occupied by the rough bristly surfaces of the palate and jaws, which enable the animal to browse upon the marine vegetables which constitute its food. There are 12 cylindrical molares with flat crowns. The aperture of the ear is remarkably small. There are no dorsal or ventral fins, and the place of the anterior extremities is supplied by fins incapable of supporting the animal when out of water. There are two appendages opening into the stomach near the junction of the duodenum. The intestines are long. The liver is divided into two large lobes, and there is a smaller tongue-shaped lobe which covers the gall-bladder. The kidneys are large, and the urinary bladder apparently capable of considerable distension. The testicles are situated a little below the kidneys. The urethra opens in a small tubercle between the two lobes of the glans penis. The thymus gland is large, black, and friable. The lungs are not lobulated, and the ventricles of the heart are separated at their points. The head is remarkable for the manner in which the anterior part of the upper jaw bends downward, the lower jaw being proportionally truncated. There are 52 vertebræ. The ribs are 18 on each side. The sternum is bifurcated at the point, and articulated to the cartilages of the upper ribs. There is no pelvis or posterior extremities, but opposite the eighth and tenth lumbar vertebræ are two narrow flat bones lodged in the flesh, one on each side. The scapula is thick, and the humerus, radius and ulna short and strong.

The flesh of this animal is delicate and juicy, and somewhat resembles veal or young beef. It is only found in the shallows and inlets of the sea; and the greatest number is said to be taken during the northern monsoon, near the mouth of the Johore river, in the inlet of the sea between the island of Singapore and the main. They seldom exceed eight or ten feet in length, though the author considered it probable that they grow much larger.

June 1.—A paper, by Sir E. Home, was read, entitled, "Microscopical Observations on the Human Urethra."

For the chief observations in this paper, the author confessed himself indebted to Mr. Bauer. From these it appears that the human urethra is made up of two parts; an internal membrane, and an external muscular covering. The former is very thin, and destitute of fibres. It is thrown into folds in the collapsed state, and upon its surface are numerous orifices of glands. The latter is made up of short interwoven fibres, forming fasciculi, united by an elastic substance of the consistence of mucus. These facts show, in the author's opinion, the fallacy of the common opinion, that the lining of the urethra consists of circular contractile fibres, and thus throws a new light on stricture; a spasmodic stricture being caused by a contraction of a small portion of the longitudinal muscular fibres, while the others are

in a state of relaxation, and a permanent stricture by the exudation of coagulable lymph, in consequence of inflammation, between the fasciculæ of muscular fibres, and upon the internal membrane.

After briefly noticing what is already known respecting the structure of the corpus spongiosum and corpora cavernosa, the author proceeded to relate Mr. Bauer's observations on these parts. The cellular structure of the corpora cavernosa is made up of very thin membranous plates, very elastic, and so connected as to form a trellis work, the edge of which is attached to the elastic ligamentous substance which surrounds them, and which forms the septum between them. The structure of the corpus spongiosum was stated to be similar to that of the corpora cavernosa, except that the parts are formed on a smaller scale, and that there are no muscular fibres in its ligamentous elastic covering. These observations were illustrated by several beautiful drawings made by Mr. Bauer.

June 8.—A paper, by George Fisher, Esq. was read, entitled, "On the Errors in Longitude as determined by Chronometers at Sea arising from the Action of the Iron in the Ships upon the Chronometers." The author commenced by stating that the sudden alteration of the rates of the chronometers when taken on ship-board has been often noticed, and generally ascribed to the ship's motion. But from what he had observed respecting the action of the chronometers on board the *Dorothea* and *Trent* in the late voyage to the North Pole, he had been induced to refer this change of rate to other causes. Thus he found that in all cases the gaining rates were increased, and the losing ones diminished on ship-board. That this alteration took place when the ships were fast in ice, or when at anchor close on shore, and consequently when there was no motion; and lastly, that it was independent of changes in temperature. The author then stated his opinion that it depends upon the iron in the ship, which, by acquiring polarity, converts the whole ship, as it were, into one large magnet, having its south pole in the deck, and its north pole below; and that the outer rim of the balance, being made of steel, is acted upon by this magnetic influence, and is itself also liable to become magnetic. This opinion was illustrated by experiments, in all which it was found that when magnets were made to act upon chronometers, their rate was accelerated, and is further confirmed by the fact that chronometers having balances and balance-springs, made of gold, keep time better than others.

June 15.—A paper, by Sir E. Home, was read, giving an account of a new mode of performing the high operation for the stone. The mode of operation described is what the author considered to be an improvement upon a method practised by Dr. Souberbielle in France, and lately described by Mr. Carpue. The improvement consists chiefly in rendering the incision in

the perinæum unnecessary. In the case described, the subject of which was a boy of about 16 years of age, an incision was made in the direction of the linea alba, beginning at the pubes, and four inches long, down to the tendon. The linea alba was then pierced close to the pubes, and divided to the extent of three inches. A silver catheter was now introduced into the bladder, and its point being felt through the wound, in the fundus of the bladder, a stilette was forced through it, followed by the end of the catheter. The stilette was then withdrawn, and the opening at the fundus of the bladder enlarged by a probe-pointed bistoury, the catheter was then withdrawn, and the stone extracted. A flexible gum catheter was lastly introduced by which the urine passed off.

The wound in the bladder was healed, and the boy passed his urine freely through the urethra in 10 days. The stone extracted was very rough, and above an ounce in weight.

At this meeting also a paper was read, entitled, "Observations on the Dugong, the *Trichechus Dougong* of Linnæus, by MM. Diard and Duvaneil, naturalists, employed under the authority of Sir Stamford Raffles." The description of this animal given by these gentlemen very closely resembled that given above by Sir S. Raffles himself, but was more full in some respects. The head was compared to that of a young elephant having its trunk cut off obliquely from above, a few inches below the forehead. The lips were stated to be muscular, and moveable, and the jaw bones paved with many horny plates. The animal has two stomachs. On the left side of the first is a large gland which appeared to secrete a fluid resembling the pancreatic. This first stomach communicates with a second, half its size, having near its orifice two conical cæca. The lungs are long, and the trachea bifurcated immediately below the larynx. All the bones of the hand as they exist in man were found under the skin in the pectoral oars, although they bore no external appearance to hands. In other particulars, the account given by this gentleman of the structure of this animal coincided with that given above by Sir S. Raffles.

June 22.—The Society met, but immediately adjourned, on account of the lamented death of their President, the Right Hon. Sir Joseph Banks.

GEOLOGICAL SOCIETY.

April 7.—The first part of the observations on the Geology of the Isle of Man, by J. S. Henslow, Esq. M.G.S. was read.

Granite occurs in much greater abundance in the Isle of Man than Dr Berger seems to have suspected. In the neighbourhood of the Dun, it is found chiefly in a decomposed state, and in some places passing into gneiss, but in the higher elevations it is perfectly compact. To the south of Fox-dale, it forms a hill or ridge stretching nearly north and south for about one mile, and

is called by the inhabitants Slicaw-my-clough, which signifies "the strong mountain." The granite here is large grained. Two varieties of mica slate are met with on Slicaw-my-clough, the first occurring in thin strata where the Foxdale river bends to the north, and being traversed by veins of schorl and quartz crystallized in alternate bands; the other appearing in two or three detached places, and consisting of fine grains of quartz and mica intimately blended with small crystals of schorl.

The gneiss near the Dun gradually passes into a chlorite slate very regularly stratified, the strata running north-east and south-west, dipping at a considerable angle to the south-east. A bed occurs here about 12 or 14 feet wide, which is between well-defined strata, but is itself in a state of confusion. It is much decomposed on the surface, and is traversed by a vein composed partly of clay and partly of a stalactitic substance united with a considerable portion of lime and a few specks of galena. This bed may be traced from the road side beyond the ridge, and along the course of the stream, about half way between the former spot and the sea till it is lost there.

The sections of the hill over which the road passes to the north of Douglass Bay, shows the occurrence of veins and beds in the Killas which cannot well be referred to whin or elvan dyke, although they appear to approach the latter on Peel Hill, and between Portle Murray, and the small patch of limestone to the south, and in a quarry at Port Eshee similar appearances are observable.

At Canal Ferrane there is an unstratified bed of a crystalline texture, of a dirty flesh colour, full of fossils, and exceedingly hard, which Dr. Berger has omitted to mention. It presents a rugged appearance along the beach, so nearly resembling that of the amygdaloid with which it is connected, that it might be mistaken for a portion of the same bed.

April 21.—The reading of the "Observations on the Geology of the Isle of Man," by J. S. Henslow, Esq. M.G.S. was concluded.

The sandstone at Peel which is stratified occurs both fine grained, and under the form of a breccia, with spangles of mica dispersed through it, and occasionally mixed with much argillaceous matter. Towards the northern extremity of this formation near Knockshally, it terminates in an unstratified breccia, containing limestone, agate, and ferriferous carbonate of lime. Many of the agates are of a large size, and very beautiful texture, but full of flaws so as to be unfit for the purpose of the lapidary. Veins of crystallized carbonate of lime traverse the bed in every direction, and pass through every substance which they meet with in their course, even the smallest agates.

A portion of the bed of amygdaloid near Castle Town consists of a trap tuff, or conglomerate, occurring in patches, which overlie and fill up the cavities in the wacke of which the remaining

part of the bed is composed, and enclosing angular lumps of black limestone and marine exuvia fossilized by lime. A bed of greenstone in which hornblende is remarkably well crystallized occurs to the south of Port le Murray.

Fibrous actinolite occurs in two veins near the Dun, traversing the decomposed portion of the granite and gneiss, and veins of chlorite are found in different parts of the island traversing the clay slate, and often colouring the quartz.

Curragh is a name given to any tract of peat bog. These curraghs exist both in the low lands and on the tops and sides of mountains, and in most of them are found trees often of a considerable magnitude, and principally oak and fir; hazels also with their nuts are common; ash and black alder are in some instances met with, and in one of the mountain curraghs, holly has been found. These trees are never even partially charred, but the oak is frequently tinged of a deep black colour, and when properly seasoned will admit a polish equal to ebony. Towards the north, the trees lie in general upon a bed of clay; in some of the deeper curraghs they are still erect, but for the most part they are broken about two or three feet from the roots. In the parish of Kirk Balaff there is a bed of marl, in which are found the remains of the gigantic elk. Over it is a bed of sand six feet thick, and upon that lies a kind of peat a foot and a half thick, and composed of rotten leaves and small branches closely matted together, and containing a vast number of the exuvia of beetles, bees, and their nests, crushed together. From these appearances it may be inferred that an extensive forest has been overwhelmed by an irruption of the sea bringing with it a large portion of sand. Between 20 and 30 years ago, the sea, during a violent storm, laid bare a forest about midway between high and low water mark in Poolwash Bay. The trees were of the pine tribe, and lay dispersed in the same direction as those before mentioned, viz. from south-west to north-east.

An appendix to the paper "On the Geological Relations of the Environs of Portrush," by T. Weaver, Esq. containing "Observations on the Organic Remains in Trap," was read.

At Portrush, and in the Skerries Island, the kind of trap containing petrefactions of ammonites, pectinites, and belemnites, is evidently a part of that extensive formation which was deposited subsequently to chalk, and which reposes principally upon it in the north-east quarter of the island. But detached portions of the same formation are also to be found in the country adjacent to the grand deposition, being dispersed over both primary and secondary tracts, and in some cases also covering layers of bituminous wood, and consequently may be considered as belonging to that series which has been denominated the overlying or newest floetz trap formations.

The rock of Portrush and Skerries has been considered by some geologists as being a flinty slate rather than a basalt, but

being compounded, as it would appear, of compact felspar coloured black by an intimate solution of hornblende, and possessing as great a degree of fusibility as basalt itself; it seems referable to the trap family, and to be more nearly allied to compact greenstone than to any other rock: it closely resembles the substances that form the walls of the vein or dyke on the coast, a little to the west of Seaport; intersecting there alternate beds of basalt and amygdaloid. The trap which exhibits vestiges of organized bodies in the north of Ireland is evidently a formation of a much later era than that which contains analogous remains in the transition series, or in the first floetz limestone.

But these formations agree in this respect, that the organic remains found in them correspond with those contained in the transition beds, the first floetz limestone or the chalk with which they are connected; and the inference, therefore, appears legitimate, that trap contained in those transitions and floetz rocks is to be referred to a similar origin.

May 5.—“A Notice on the Geological Structure of a Part of the Island of Madagascar, and on some Specimens from the Interior of New South Wales,” by Prof. Buckland, was read.

From the specimens from Madagascar which were chiefly collected at Port Lougair, at the north-east angle of the island, it would appear that a portion of the island consists of primitive rocks, sandstone, and trap, and presents a similar geological structure to the adjacent continent of Africa. The varieties of granite which are found in the bed of the river Vaulaceen, that runs to the above-mentioned port, are very similar to those which commonly occur in Europe, e. g. fine grained grey granite, large grained granite, containing flesh-coloured crystals of felspar. The secondary rocks are varieties of sandstone, destitute of organic remains, and composed of grains of glassy quartz intermixed with decayed felspar, but possessing no strongly marked character by which they can be identified with any of the known European series of rocks. A bright red sandstone, which is said to form the substratum of a hill called St. George's, in the same district, is more decidedly characterized, and seems to belong to the same class as enormous tracts of a similar formation which occurs in the neighbourhood of the Cape of Good Hope. It resembles in every particular of its colour and composition the newer red sandstone of the English series.

Among the other Madagascar rocks are a clay porphyry resembling that of Newton Glens in the county of Antrim, a fine grained greenstone similar to some of those found at the Giant's Causeway, and a firmly compacted cream-coloured limestone composed of granulated fragments of shells agglutinated by a calcareous cement.

The specimens from New South Wales afford indications

principally of primitive rocks and trap. Among the former are several varieties of granite, and specimens of mica slate, clay slate, and serpentine. Among the latter are some specimens resembling those of the neighbourhood of Edinburgh. A few varieties of sandstone interposed with decomposed felspar are the only secondary rocks of the collection. There is nothing in these specimens indicative of valuable metals, or precious stones, or coal, or any kind of animal or vegetable remains.

May 19.—A letter from E. Hanmer, Esq. M.G.S. describing the Tottenhoe stone, was read.

The quarries which supply this stone are situated about two miles from Dunstable, at the north-west frontier of the chalk, between the Roman camp and Dunstable hill. The stone forms a bed of from 15 to 25 feet in thickness, at the bottom of the chalk resting on a grey marl, and dipping slowly to the south-east. It has been long used in building, as appears from the churches and other old buildings in the country.

One hundred grains of the dried stone were found to contain as follows :

Carbonate of lime	72
Silex (in very fine white sand)	21
Alumina	7
	<hr/>
	100

Part of the paper on the Geology of the Environs of St Petersburg, by the Hon. W.T.H.F. Strangways, M.G.S. was read.

June 2.—A paper "On the Limestone Veins of Cette near Montpellier, by Dr. Daubeny, M.G.S. was read.

The calcareous rock of Cette is of a compact kind of stone, of a blueish colour, and tolerably destitute of organic remains, and seems a continuation of the secondary limestone, which reposes on the primitive rocks, the granite and mica slate of the Vivarais, alternating with a sandstone that resembles in its characters and geological relations the old red sandstone of this country.

It approaches to some varieties of the mountain lime, near Bristol, and, like it, contains insulated masses of a greyish-white flint, which vary in size from the magnitude of a man's foot to that of a filbert.

This rock is also intersected by irregular masses of a limestone quite different in character; although of various degrees of hardness, it is never so compact as the rock which it penetrates; its colour approaches more or less to a yellow ochre; and its texture is often distinctly striated. These masses are associated with some very beautiful and distinct stalactites, which, from the imperceptible gradation between them and the limestone to which they adhere, their exact resemblance in colour and struc-

ture to portions of the stone which are connected with the hardest form of the rock found in the vein, together with the curious intermixture of the compact with the stalactitical varieties, are clearly referable to the same origin, and appear to have been produced by a similar process of argillaceous infiltration.

A paper, by the same author, entitled "Remarks on the Columnar Structure of Trap Rocks," was then read.

In following the track of the streams of lava from their sources, we are struck with the increasing solidity which they assume in the course of their descent, their texture becoming gradually less and less porous, until, as we approach the valley which forms the limit of their progression, they put on almost invariably the character of a compact basalt, but it is only where the lava is cut through by the stream which occupies the lowest part of the valley that it assumes a columnar structure. This arrangement has been ascribed by the late Mr. Faujas St. Fond to the sudden congelation of the liquified mass by falling into water; but in opposition to this opinion, it may be observed, first, that sudden congelation instead of being favourable to a regular arrangement among the particles of matter, proves, as far as we know, almost destructive of it; secondly, that there are numerous instances of columnar trap being met with in situations where no collection of water can be supposed to have existed; as, for instance, the columnar clinkstone porphyry on the summit of the Mozen, the highest mountain of the Vivarais; and thirdly, that it would become necessary if this hypothesis were admitted, to attribute an igneous origin to granite and other rocks which exhibit occasionally the same prismatic arrangement. It appears more probable, that the natural structure of the stone has been developed by the continual action of the stream, which, in the course of ages, has cut itself a passage through the existing materials of the rock, though, perhaps, it would be going too far to assert that the columnar arrangement is always referable to this cause. In confirmation of this conjecture, it may be observed, that the rock, at the foot of which the town of Murat (Dep. of Cantal in Auvergne) is situated exhibits some remarkably regular basaltic columns, which, towards the summit, are vertical; but as they descend, appear to be bent in conformity with the slope of the hill. The curve which they describe still increasing, they become by degrees quite horizontal; so that the extremities only of the prisms jut out from the side of the rock; the interstices between the columns being filled up by a species of trap much looser in its texture than that of which the columns themselves are composed. Again at the village of Prentigarde, about half a league from the Bains de Mont Doi, in the Dep. Puy de Dôme, the trachyte or porphyry formation is surrounded by a basalt of a very compact character, and of both these rocks a good section is exhibited at the cascade of Querceuil, where a small mountain

torrent is precipitated down a perpendicular escarpment to the depth of about 50 feet, and where it may be perceived, that, although the basalt on either side is wholly amorphous, or divided only by irregular fissures into imperfect quadrangular masses, yet that, where it lies in immediate contact with the falling stream, it exhibits some well worked columns which seem clearly to have been developed by the continued action of the water, and the abrasion of the rock by which the natural structure was obscured.

ROYAL ACADEMY OF SCIENCES AT PARIS.

An Analysis of the Labours of the Royal Academy of Sciences of Paris during the Year 1818.

(Concluded from vol. xv. p. 463.)

MEDICINE AND SURGERY.

The membrana pupillaris is that cellular and vascular veil which closes the pupil in the fœtus, and is torn, and usually disappears towards the time of the birth. M. Portal has published some observations on this veil, which he thinks is sometimes, by its not being torn, the occasion of that blindness which occurs in new-born infants, and which can be easily cured by an operation. M. Portal thinks the new-born infant is void of hearing and smell, as well as of sight, because the nostrils and the cavity of the tympanum are filled with mucosity, from which they must be freed before the organs can be of any use. It also sometimes happens that infants are born deaf, and remain so, because the cavity of the tympanum is not emptied.

M. Portal, whose work on the enlargement of the heart without any dilatation of its cavities was analyzed by us last year, has read in the present a paper on the aneurisms of that organ.

In this work he has shown that they are very common, and always consist of an enlargement of one or more of the four cavities of the heart, either because their sides have become thin, or have grown thicker; for, in every case, it is the blood which produces either singly, or in concurrence with other causes, this enlargement in one or more of the cavities of the heart, by distending their sides, which are always too weak relatively to its impulse, or because the blood is in too great quantity throughout the whole system of circulation, or because there being some obstacles to its egress from the heart, it is retained there in too great a quantity, by which means it distends its sides: also that the contractions of the sides of the heart, so far from being stronger when these sides are thicker than usual, are, on the contrary, much weaker, if the sides are disorganized by any disease, as they almost always are in that case; and should it happen that, while the heart is sound, its sides acquire a greater thickness than usual, they would indeed contract more forcibly,

but they would then be in a state very contrary to that in which they are found when an aneurism is formed; for then, by forcing out the blood with too great violence into the pulmonary arteries and the aorta, aneurisms might be produced in those parts, but not in the cavity of the heart, from whence the blood proceeded.

M. Portal, from these observations, concludes in favour of the opinion of many celebrated physicians, that aneurisms are always passive in respect to the force of the sides of the heart; absolute or relative to the action of the blood against the same sides; and that the indications of aneurisms given by these skilful practitioners are quite certain, and their practice, relative to blood letting, is the best established, and the most efficacious.

Baron Percy has communicated to the Academy some curious historical researches upon *mericism*, a kind of disgusting indisposition, consisting in bringing up into the mouth the half digested food, to swallow it a second time. It is a sort of rumination that has caused those physicians who have spoken of it to advance very extravagant opinions, all of which M. Percy reduces to their proper value.

Several diseases of the chest, by altering the proportion between the hollow and occupied places in that cavity, or by diminishing entirely or partially the faculty that the lungs possess of expanding or contracting, produce a change of sound from the sides of the chest when they are struck—a change which yields, in certain cases, some useful indications respecting the causes which produce it.

From this has arisen the art of discovering the affections of the chest by percussion, concerning which, Avenbrugger, a physician of Vienna, published a treatise, which has been translated and extended by Mr. Corvissart. But much more delicate observations may be made, either by bringing the ear nearer, or by using instruments; this kind of observations constitutes the art of exploring the diseases of the thorax by means of auscultation.

M. Laennec, a physician of Paris, has presented to the Academy an essay upon this subject, in which he explains a method of his own. Sometimes he uses a solid cylinder, sometimes a tube with thick sides, and sometimes a tube hollowed out like a funnel: he applies one end of these instruments to different parts of the thorax, and brings his ear near the other end.

The tube with thick sides, or cylinder pierced along its axis with a narrow aperture, being applied to the chest of a person who is speaking or singing, there is heard, if the person is in good health, no more than a sort of trembling noise more or less distinct; but if an ulcer exists in the lungs, a very singular phenomenon happens. The voice of the sick person can no longer be heard by the ear at liberty; the whole of the sound passes along the aperture of the cylinder to the observer. Com-

missioners appointed by the Academy have verified this experiment in many consumptive persons.

The same phenomena take place when the instrument is placed on the trachea, or larynx, of a healthy person. M. Laennec, who has given the name of pectoriloquy to this effect, arising from the pulmonary alterations, distinguishes the varieties of it, and shows the indications which result from it, by reason of ulcers in the lungs, of their size, of their state whether empty or full, and of the consistence of their substance.

This instrument will also show in a distinct manner the motion of respiration, and the beating of the heart, so that we may easily form a judgment of their greater or less regularity; information which cannot fail of affording useful indications in the diseases of those two functions.

The employment of gold in medicine, although long ago praised by the alchemists, appears to have been forgot in modern times; but M. Chrétien, a celebrated physician of Montpellier, announces that he has discovered that this metal, even in a state of purity, has very strongly marked medical properties, and that he has been able to use it with success in scrofulous and syphilitic affections. He has addressed a voluminous work to the Academy, which contains the history of the principal diseases in which he used it, and a detailed account of the precautions with which he employed it. The committee of the company have also made in their turn numerous experiments in the method pointed out, in order to be able to estimate the virtues of gold. By means of rubbing gold, or the triple muriate of gold and soda, upon the tongue, they have been able to cicatrise scrofulous ulcers, to resolve syphilitic swellings, to destroy nodes in part, to stop the progress of caries, to put an end to unsupportable pains in the bones, to dissipate ophthalmias of long standing, obstinate disorders of the throat, and eruptions which had withstood all other remedies.

But there happened to them also other results which were much less fortunate, and their want of success was not limited by the disease being left in its primitive state; for it was in several instances exasperated by the action of the remedy. Indolent tumours have become inflamed, fever, cholic, alarming inflammations of the stomach have taken place; a thickening of the periosteum, which was without any pain, has degenerated into a cancer.

It is, therefore, very certain that gold is very far from being such an inactive medicine as has been said; but it is equally certain that its use must be guided by certain rules and precautions relative to the circumstances in which the patients who are to use them are placed. Rules and precautions which nothing but long experience, and a numerous set of observations well weighed, can supply to the medical art.

The late M. Ravrio, bronze manufacturer, who had acquired cele-

brity from the perfection to which he carried that species of workmanship, left two years ago a sum of money to the Academy, to be given to the person who should discover the means of preserving gilders on bronze from the deleterious effect of the vapours of quicksilver, which causes nearly all of them to die very soon after dreadful sufferings.

This prize has been gained by M. Darcet, who has not only given a complete solution of M. Ravrio's problem, but also inserted in his essay, so many useful schemes for rendering the different operations occurring in the art of gilding, easier, more efficacious, and less unwholesome, that his work is become a complete treatise on that art, which is at present so important to France.

The method invented by M. Darcet consists in an auxiliary furnace, from whence a tube passes into the gilder's chimney, and thus produces such an ascending current of air that no portion of the quicksilver can fail of being drawn up by it; and also in adapting to the chimney another pipe, which is bent into a vessel filled with water, where the greatest part of the quicksilver that was evaporated is collected for future use.

Another important change made by M. Darcet is the substitution of nitrate of quicksilver instead of nitric acid, in the operation of cleansing the work, which operation is very injurious to the lungs of the workmen, when it is made with pure acid.

The processes that M. Darcet introduced a long time ago into the mint have been adopted in several water-gilders' workshops, and the Prefect of Police no longer allows any water-gilder to set up in business, or to remove his workshop, unless he arranges his shop so as to employ these processes.

Cupping glasses are instruments in the form of a bell, which are fastened to the skin by a vacuum being made in them either by heat, or by a piston; the weight of the atmosphere acts on all the surface of the body, except at the place where the cupping glass is applied, which of course produces an elevation of the skin, and a swelling of the blood-vessels and lymphatics at those places, rendering them red and purple, and exciting there a strong sensation of heat. Scarifications made either before or after the application of the cupping glasses draw out part of the blood and lymph which are thus accumulated on the spot. The adjacent parts, and those below them, are thus freed from the fluids that cause the swelling, and retract afterwards, in consequence of the external dilatation.

This mode of cure, much used by the ancients, and still frequently employed in Germany and some other countries, is rather neglected in France.

M. Gondret, whose remarkable observations on the use of fire in medical practice has been already noticed by us, has also occupied himself with cupping glasses. He has found that the effect they produce is frequently far superior to what

might be expected from the small quantity of the fluids extracted by their use. Leeches, although drawing out much more blood, have frequently by no means the same success; and moreover dry cupping, or the mere application of the exhausted glasses, produces in many cases the same effect as when scarification is also used. This remedy has proved successful in many local congestions, accompanied with irritation and fixed pains, and generally in phlegmatic or partial inflammations either acute or chronic. When properly applied, it has allayed the symptoms caused by difficult dentition, has occasioned palpitations of the heart to disappear, and has stopped uterine hæmorrhages.

The most surprising and most honourable operation of surgery is without any contradiction that executed by M. Richerand, by taking away a part of the ribs and of the pleura. The patient was himself a medical man, and not ignorant of the danger he ran in this operation being had recourse to, but he also knew that his disorder was otherwise incurable. He was attacked with a cancer on the internal surface of the ribs and of the pleura, which continually produced enormous fungosities, that had been in vain attempted to be repressed by the actual cautery. M. Richerand was obliged to lay the ribs bare, to saw away two, to detach them from the pleura, and to cut away all the cancerous part of that membrane. As soon as he had made the opening, the air rushing into the chest occasioned the first day great suffering and distressing shortness of breath; the surgeon could touch and see the heart through the pericardium, which was as transparent as glass, and could assure himself of the total insensibility of both. Much serous fluid flowed from the wound, as long as it remained open, but it filled up slowly by means of the adhesion of the lung with the pericardium, and the fleshy granulations that were formed in it. At length the patient got so well that on the twenty-seventh day after the operation he could not resist the desire of going to the Medicinal School to see the fragments of the ribs that had been taken from him, and in three or four days afterwards, he returned home, and went about his ordinary business.

The success of M. Richerand is the more important, because it will authorize in other cases, enterprizes, which, according to received opinions, would appear impossible; and we shall be less afraid of penetrating into the interior of the chest.

M. Richerand even hopes that by opening the pericardium itself, and using proper injections, we may cure a disease that has hitherto been always fatal, the dropsy of that cavity.

The cataract is a species of blindness that arises from the crystalline lens of the eye losing its transparency, and from the most ancient times, the art of curing it has been known, either by extracting the vitiated crystalline through an opening made in the cornea, or by displacing this lens by means of a needle put into the eye, and thus leaving a free entrance to the rays of

light through the pupil. The respective advantages of these methods have been long disputed, and each of them has been alternately more in use than the other: even in the present day oculists are divided as to their respective merits, and prefer either one or the other, according to the ideas they have formed of them, and the habit of using them which they have acquired. The circumstance which has prejudiced some against the operation of depressing the cataract was the uncertainty as to what became of the crystalline, and the fear that it should replace itself, and again obstruct the pupil; but it is now known by the experiments of M. Scarpa, that it is either dissolved, or absorbed, by the humours of the eye in a very short time, and that every trace of it is speedily obliterated.

M. Roux has read to the Academy a memoir on these two methods, and their respective advantages; he prefers the extraction, but he allows that it is not applicable to every case, and it is only in these that he wishes the depression to be employed.

RURAL ECONOMY.

M. Yvart, being invited last year by the Minister of the Interior to assist with his advice the proprietor of an estate in Auvergne in a large agricultural enterprize made on the ruins of extinct volcanoes, immediately went thither, and has seized this opportunity to study the system of rural economy adopted in the neighbourhood of the Mont d'Or, and the Puy de Dôme.

He has furnished the Academy with the several objects which struck his attention in this journey; such as the practice of paring and burning the soil, the inconveniences of *écobuage*, the importance of natural and artificial meadows, the necessity of destroying the prejudices that still exist on many points in respect to the last, the cultivation of corn on steep slopes, that of some economic plants proper to better the condition of the inhabitants of mountainous tracts, the rearing, keeping, and profits of cattle; the means of supplying in many cases the use of common salt in feeding cattle by acidulous mineral waters, the acquisition, preparation, and employment, of manures and dressings, the necessity of plantations, the precautions to be taken in order to insure their success, the advantages and means of establishing watered meadows.

This last object having appeared to M. Yvart to merit the being treated of in detail from the numerous notes which he had collected in his different journeys through nearly the whole of France, Italy, Switzerland, and England, he has endeavoured to demonstrate in another work, which he has also submitted to the Academy, the great importance of this kind of improvement in our rural economy, and how much there remains for us to perform in order to enjoy all the benefits of this practice.

The Minister has recently caused the whole of the researches

and observations of this skilful cultivator to be published, in order to communicate them to the provincial administrations, and the several agricultural societies. By this means the country gentlemen will speedily be able to see them, and will no doubt find in them several ideas which will be useful for the improvement of their estates.

M. Yvart has also communicated several interesting details concerning the agricultural statistics of England, through the medium of giving to the Academy an account of a work on Derbyshire, which the author, Mr. Farey, had addressed to him.

STATISTICS.

M. Moreau de Jonnes has given a memoir on the population of the French West Indian islands, in which he appreciates the causes and extent of the annual augmentation or decrease of each of the classes composing that population.

From official data, he estimates the mortality of the white Creoles, and of the free mulattoes, at four per cent. and that of the native black slaves at only three per cent.; but in regard to new comers, the case is quite different. The English troops lost 21 men in every 100, and the French, 33—a difference which the author attributes to the superior discipline of the former. The black troops raised in Africa by the English, and transported to the West Indies, lose only in the proportion of three and a half per cent.; but the slaves carried over lose as far as $17\frac{1}{2}$ —a mortality, however, which is even then inferior to that of the Europeans.

The reproduction of the whites is three in 100, and that of the mulattoes four; which arises from the numerous cohabitations of the whites with negresses and mulatto women; but among the slaves there is born at Martinique only two infants from 100 persons; of course, this class diminishes every year at the rate of one per cent. The diminution of this class at Grenada is, according to Colquhoun, double.

The Academy judged this memoir worthy of the prize lately founded, by an anonymous person, for the encouragement of statistical inquiries.

ARTICLE VI.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE

I. *Meteoric Stones.*

The number of meteoric substances has been lately increased by a very remarkable body, distinguished in Germany by the

name of *mourning paper* (*trauer papier*). It fell, according to the statement in the ephemeris of the Leopold Academy, near Rauden, in Curland, on Jan. 31, 1686, in great abundance. Chladni mentions it in his Catalogue of old Meteoric Deposites, and by this means brought it into notice, and enabled M. von Grotthuss, of Curland, to recognize it in an enigmatic substance, deposited in his museum of natural curiosities; but which an attached ticket announced to be of meteoric origin. It consists of a mass of black leaves, having the appearance of burnt paper; but it is harder, coheres together, is somewhat brittle; and when examined by chemical reagents is found to be a mineral mass, composed of the same ingredients as meteoric stones; for it consists of silica, magnesia, iron, and some nickel, and exhibits also traces of chromium. It may be compared to the black crust which covers the surface of the meteoric stones. Black substances, like beans, are described as having fallen along with this matter; but these bodies have not yet been recognized.

II. *Carmine.*

According to M. von Grotthuss, carmine may be deprived of its yellow shade by ammonia, and subsequent treatment with acetic acid and alcohol. Since the knowledge of this fact was published, M. Herschell, colour-maker at Halle, has prepared a most beautiful carmine.

III. *Vinegar from Wood.*

Mr. Stotze, apothecary at Halle, has discovered a method of purifying vinegar from wood by treating it with sulphuric acid, manganese, and common salt, and afterwards distilling it over. For this method he has obtained a prize from the Royal Society of Gottingen.

This gentleman has likewise verified the method proposed by Prof. Meineke in 1814 of preserving meat by means of vinegar from wood, and by continued treatment with the same acid has converted bodies into mummies.

IV. *Iron Pyrites.*

Prof. Meineke has described shortly to the Natural History Society of Halle a new variety of pyrites (*blätterkies*) detected by him. It is precipitated in brilliant tables upon the roots of reeds.

V. *New Vegetable Alkalies.*

Messrs. Meissner and Brandes have made themselves conspicuous in Germany by their investigations and discoveries of the alkaline substances which exist in narcotic plants. Dr. Brandes was the first discoverer of the *delphia*, *daturia*, *hyoscyama*, *atropia*. The last of these substances he has found to constitute the ingredient, which gives the atropa belladonna its peculiar properties. It is brilliant white, crystallizes in long needles, is tasteless,

and little soluble in water and alcohol. It is capable of withstanding a moderate heat. It forms with acids regular salts, and is capable of neutralizing a considerable proportion of acid. Sulphate of atropia is composed of

Sulphuric acid	36.52
Atropia	38.93
Water	24.55
	100.00

When atropia and potash are mixed together and exposed to a red heat, the ashes, when mixed with muriate of iron, strike a lively red colour.

Hyoscyama, or the alkali extracted from the *hyoscyamus niger*, is not easily altered in a high temperature even when heated to redness with charcoal. It crystallizes in long prisms, and when saturated with sulphuric acid, and especially with nitric acid, forms very characteristic salts.

The examination of the alkaline constituents of narcotic plants demands great circumspection, because in them the whole poisonous properties of the plant are concentrated. The vapour is particularly prejudicial to the eyes. The smallest morsel put upon the tongue is particularly dangerous.

VI. Analysis of Meionite. By Professor Leopold Gmelin, of Heidelberg.

The specimen of meionite subjected to experiment was from Vesuvius. Though considerable pains were taken to select pure crystals, it was impossible to free them entirely from leucite, carbonate of lime, and a white matter, with which many of them were encrusted. The specific gravity of the specimens examined was 2.650, which approaches nearly to 2.612, the specific gravity found by Mohs. Though Lelievre affirms that this mineral melts easily into a vesicular white glass, and though this statement has been copied into most mineralogical books, Gmelin was unable to fuse it before the blow-pipe, or even to soften the edges of the particles exposed to heat. The constituents of this mineral, as determined by analysis, are as follows :

Silica	40.8
Alumina	30.6
Lime	22.1
Soda with some lithia	2.4
Oxide of iron	1.0
Carbonic acid and loss	3.1
	100.0

The presence of lithia was inferred from the violet colour which the platinum crucible acquired. This analysis shows an

approach in the composition of meionite to scapolite and prehnite. As the specimen subjected to examination was not absolutely pure, it would be unsafe to attempt from the preceding numbers to deduce the constitution of the mineral.—(Schweigger's Journal, xxv. 36.)

VII. *Bucholzite*.

This is the name by which Dr. Brandes has thought proper to distinguish a mineral similar to that formerly described by Werner under the name of *fibrous quartz*. It occurs in alumslate, in Voigtland, near Hartmannsdorf, and at Wiesenbad in a gangue of amethyst. The mineral which Brandes subjected to analysis, however, came from the Tyrol, and though it resembles fibrous quartz in its appearance, we have no evidence that it agrees with it in its constitution. It will be requisite, therefore, to give the description of the bucholzite of Brandes, and not to confound it with the fibrous quartz of Werner till it has been determined by actual analysis that both are composed of the same constituents.

The colour is a mixture of white and black in spots. Lustre glistening, and the kind of lustre, is waxy, pearly, and glassy. The principal fracture is fibrous, and most remarkably so in the black spots. In the grey and white-coloured spots, the texture is often with difficulty recognisable. The cross fracture displays here and there a conchoidal fracture. In some cases there is a tendency to the foliated fracture, and the cleavage indicates in these cases an analogy to felspar. The fragments are wedge-shaped, sometimes sharp-edged, and sometimes not particularly so. When in thin fragments it is weakly translucent, and this is particularly the case in the white spots. It is hard enough to scratch glass; but is itself scratched by quartz. The result of Brandes's analysis of this mineral, which it seems unnecessary to specify, as it differed in nothing from the usual mode of analysis, gives its constituents as follows:

Silica	46·0
Alumina.	50·0
Oxide of iron	2·5
Potash.	1·5
	<hr/>
	100·0

From the variation in the colour of this mineral, it is probable that the oxide of iron is merely accidentally present, and that it does not enter as a chemical constituent of the mineral. It seems, therefore, to be in reality a neutral silicate of alumina mixed, or united, with a very small proportion of silicate of potash.

Though I have mentioned fibrous quartz as similar to *bucholzite*, there seems no reason to doubt that it is in reality a different mineral; for Mr. Zellner subjected the quartz to an analysis (Gilbert's Annalen for 1818, p. 182), and found it composed of

Silica.....	98·75
Oxide of iron.....	0·75
Water.....	0·25
	<hr/>
	99·75

The bucholzite in its chemical composition approaches very nearly to nepheline, which, according to the analysis of Vauquelin is composed of

Silica	46
Alumina	49
Lime.....	2
Oxide of iron	1
	<hr/>
	98

But as the properties of these two minerals seem to differ materially from each other, it would be premature to unite them into one species. Indeed further researches appear to be wanting before confidence can be put in the composition of nepheline.

VIII. *Andalusite.*

Dr. Brandes has likewise subjected the andalusite of the Tyrol to a chemical analysis. The characters of the specimen analyzed were as follows :

The principal colours were ash-grey and greyish-white, but in some places there were spots of dark-grey and reddish-grey. Fracture uneven, varying from fine granular to splintery. The fragments were indeterminate angular, and pretty sharp-edged. Slightly translucent on the edges. Hard enough to scratch glass. Exceedingly easily frangible. Not altered when heated alone before the blow-pipe. With calcined borax, it melts readily, and, when cold, assumes the form of a greenish bead. It occurs crystallized in four-sided prisms almost rectangular. The angles could not be determined accurately; but the measurements gave 93° and $87\frac{1}{2}^\circ$. The constituents of this mineral, as found by Dr. Brandes, are as follows :

Silica.....	34·000
Alumina.....	55·750
Oxide of iron.....	3·375
Oxide of manganese.....	0·625
Potash	2·000
Lime	2·125
Magnesia	0·375
Water.....	1·000
	<hr/>
	99·250

Were we to disregard all the constituents of this mineral

except the two first, it would follow from the preceding analysis that andalusite is a subsesquisilicate of alumina, or a compound of 1 atom of silica and $1\frac{1}{2}$ atom of alumina.

IX. *Karpholite.*

This is a name which Werner gave to a mineral found at Schlackenwald in Bohemia. It is distinguished by the following characters :

Its colour is usually of an intense straw yellow ; sometimes, though seldom, wax yellow. Always amorphous. The lustre is strongly glimmering and pearly. Fracture fibrous. It is opaque, and its hardness, in consequence of the uncommon facility with which it may be broken, cannot easily be determined. Specific gravity, 2.935. The constituents of this mineral, according to an elaborate analysis of Professor Steinmann, are as follows :

Silica.	37.53
Alumina.	26.47
Oxide of manganese.	18.33
Peroxide of iron	6.27
Water.	11.36
	99.96

It seems to follow from this analysis that karpholite is a compound of 10 atoms of silicate of alumina, three atoms of silicate of manganese, one atom of silicate of iron, and eight atoms of water ; so that its symbol will be $10 A S + 3 m S + f S + 8 A q$.—(Schweigger's Journal, xxv. 413.)

X. *Boracic Acid.*

From a set of experiments made upon crystallized boracic acid by Dr. Pleischl, of Prague, it appears to be a compound of

Pure anhydrous acid	54
Water	45
	99

Dr. Pleischl made a set of careful and elaborate experiments to determine whether anhydrous boracic acid be capable of decomposing fused chloride of barium ; but the results corresponded with those of Gay-Lussac and Thenard, no decomposition nor chemical combination was obtained.—(Schweigger's Journal, xxv. 438.)

XI. *Peliom.*

This is a name given by Werner to a mineral from Bodenmais, which he constituted into a new species. It seems to be intimately connected with the iolite. Its specific gravity is 2.714. Its fracture is small conchoidal. Its lustre shining and glassy.

It is very difficult to reduce to powder, and it is hard enough to scratch glass. This mineral has been lately subjected to a chemical analysis by Dr. Brandes, who found its constituents as follows :

Silica.	54.00
Alumina.	28.50
Protoxide of iron.	16.18
Magnesia.	0.50
Oxide of manganese.	0.25
Water.	0.25
	<hr/>
	99.68

The constituents of iolite, as determined by Gmelin, are the following :

Silica.	42.6
Alumina.	34.4
Magnesia.	5.8
Lime.	1.7
Protoxide of iron.	15.0
Oxide of manganese.	1.7
	<hr/>
	101.2

(Schweigger's Jour. xxvi. 90.)

XII. *Death of Sir Joseph Banks, Bart. G.C.B. &c.*

We have this month to announce, with no common feelings of regret, the death of the Right Hon. Sir Joseph Banks, Bart. President of the Royal Society, &c. &c. He died at Spring Grove in the morning of Monday, June 19, at the age, we believe, of about 77 years. In him science loses all that it can lose in a single individual. The whole of his time and the whole of his fortune, which was very considerable, were devoted to the purposes of science. His liberality was universally known. It was no uncommon thing for travellers in foreign countries, when reduced to pecuniary difficulties, to draw upon him for money, and he was generally accustomed to honour such draughts by ordering immediate payment. His library, which was kept up at a very great annual expense, was rich in almost every department of science; and in some of the most expensive departments almost complete. This library was open to every man of science, who was not merely at liberty to consult the books in the library, but to carry them home to his own lodgings, and peruse them at his leisure. As a President of the Royal Society he stands unrivalled. No former President ever filled the chair with such advantages to that learned body, nor is it at all likely that the same rare union of qualities for that most important situation will again occur.

ARTICLE VII.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

Dr. Prout is preparing for the Press an Inquiry into the Nature and Medical Treatment of those Diseases connected with a deranged Action of the Urinary Organs, especially Gravel and Calculus. It is the Author's Object to take a more comprehensive View of this important Class of Diseases than has hitherto been attempted.

Dr. Leach has nearly completed his Synopsis of British Mollusca, being an Arrangement of Bivalve and Univalve Shells according to the Animals inhabiting them.

Dr. Thomson is printing a new Edition of his System of Chemistry: he also announces his Intention to prepare a Work on the Practice of Chemistry.

Dr. J. Gordon Smith, Lecturer on Medical Jurisprudence, is preparing for the Press, a Manual on that Subject, as a Guide in the Examination of Medical Practitioners on Questions that require their Evidence in the British Courts, as well as a Text-book to future Lectures.

Dr. Hartz is about to publish an Historic Sketch of the Causes, Progress, Extent, and Mortality, of the Contagious Fever epidemic in Ireland in the Years 1817, 1818, 1819.

JUST PUBLISHED,

A Sketch of the Economy of Man. By W. Nicoll, M.D. 8vo. 10s. 6d.

Medical Notes on Climate, Diseases, Hospitals, and Medical Schools in France, Italy, and Switzerland. By I. Clarke, M.D. 8vo. 7s.

An Inquiry into certain Errors relative to Insanity and their Consequences, Physical, Moral, and Civil. By G. M. Burrows, M.D. F.L.S. 8vo. 8s.

A Commentary on the Systems which have been advanced for explaining the Planetary Motions. By R. Burney, R.N. 8vo. 8s.

A Geographical, Statistical, and Historical Description of Hindostan and the adjacent Country, composed from the most authentic Documents, and from Manuscript Records, deposited at the Board of Control. By William Hamilton, Esq. With Maps. 2 Vols. 4to. 4l. 14s. 6d.

Exposition of Elementary Principles specially concerned in the Preservation of Healthiness and Production of Distempers among Mariners, Travellers, &c. in tropical and variable Climates. By Andrew Simpson. 8vo. 18s.

A Treatise on Inflammation of the Mucous Membrane of the Lungs. To which is prefixed an Experimental Inquiry respecting the contractile Power of the Blood Vessels, and the Nature of Inflammation. By Charles Hastings, M.D. 8vo. 10s. 6d.

The Journal of a Tour in Greece, Egypt, and the Holy Land; with Excursions to the River Jordan, and along the Banks of the Red Sea to Mount Sinai. By Wm. Turner, Esq. With Plates. 3 Vols. 8vo. 3l. 3s.

Journals of Two Expeditions beyond the Blue Mountains, and into the Interior of New South Wales, undertaken by Order of Government in the Years 1817, 1818. By John Oxley, Esq. With Maps and Plates. 4to. 2l. 10s.

A Geological Map of England coloured, accompanied by a Memoir. By G. B. Greenough, Esq. F.R.S. F.L.S. President of the Geological Society. On six Sheets. 6l. 6s.

Journal of a Tour through part of the snowy Range of the Himala Mountains, and to the Sources of the Rivers Jamna and Ganges. By James Baillie Fraser, Esq. 4to. 3l. 3s.

A New Method of solving Equations with Ease and Expedition, by which

the true Value of the unknown Quantity is found without previous Reduction. With a Supplement containing two other Methods of solving Equations, derived from the same Principle. By Theophilus Holdred. 4to. 7s.

ARTICLE VIII.

NEW PATENTS.

Major Rohde, of Leman-street, Goodman's-fields, Sugar Refiner, for a method of separating or extracting the molasses, or syrup, from muscovado or other sugar.

George Lilley, of Brigg, Lincolnshire, and James Bristow Fraser, of Blackburn House, Linlithgow, for improvements in the application of machinery for propelling boats or other vessels floating in or upon water, and for attaining other useful purposes, by means of an hydropneumatic apparatus, acted upon by a steam-engine, or other adequate power.

Thomas Hancock, of Little Pulteney-street, Golden-square, Coach Maker, for the application of a certain material to various articles of dress, and other articles, by which the same may be rendered more elastic.

Thomas Cook, of Brighton, for an improved apparatus for the purpose of cooking, called by him "A Philosophical Cookery."

John Hague, of Great Pearl-street, Spitalfields, Engineer, for certain improvements in the method of heating hot-houses, manufactories, and other buildings, and of boiling liquids.

John Ambrose Tickell, of West Bromwich, Staffordshire, for a cement to be used in aquatic and other buildings, and stucco-work: it is produced by the use and application of a mineral substance, not before employed in the manufacture thereof.

Josiah Parkes, of Warwick, Worsted Manufacturer, for a new and improved method of lessening the consumption of fuel in steam-engines, and furnaces in general, and for consuming smoke.

James Jacks, of Camberwell, and Arthur Aikin, of the Adelphi, for a new and improved method or methods of preventing mildew in sail-cloth and other canvas, and in other manufactures, made of vegetable fibre.

James Scott, of Grafton-street, Dublin, Watchmaker, for a new method of combining, adjusting, and applying, by machinery, certain of the well-known mechanic powers and modification thereof, where power and velocity are required.

John Malam, of Romney Terrace, Westminster, for improvements on gasometers.

Samuel Kenrick, of West Bromwich, Staffordshire, for an improved method of tinning cast-iron vessels of capacity.

Robert Wornum, of Wigmore-street, London, for an improvement on piano-fortes, and certain other stringed instruments.

Robert Bill, of Newman-street, for an improved mode of constructing beams, masts, yards, bowsprits, and other parts of ships, &c. used for navigation, and of other parts of their rigging.

John Barton, of Falcon-square, for improvements in propelling, and in the construction of engines and boilers applicable to propelling.

Richard Watts, of Crown-court, Fleet-street, for improvements in inking printing types with rollers, and in placing and conveying paper on types, and in inking with a cylinder.

Robert Winch, of Shoe-lane, for improvements in machines or presses, chiefly applicable to printing.

Edward Massey, of Eccleston, Lancashire, for improvements in the construction of chronometers and pocket watches.

ARTICLE IX.

Magnetical and Meteorological Observations.

By Col. Beaufoy, F.R.S.

*Bushey Heath, near Stanmore.*Latitude $51^{\circ} 37' 44.27''$ North. Longitude West in time $1^{\circ} 20' 03''$.*Magnetical Observations, 1820. — Variation West.*

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
May 1	8h 35'	24° 33' 20''	1h 20'	24° 41' 46''	7h 10'	24° 32' 49''
2	8 35	24 32 45	1 25	24 40 10	7 10	24 31 29
3	8 35	24 31 02	1 15	24 38 59	7 10	24 30 42
4	8 35	24 30 54	1 40	24 39 09	7 10	24 32 11
5	8 40	24 30 06	1 20	24 40 43	7 15	24 31 16
6	8 40	24 30 05	1 20	24 38 41	—	—
7	8 35	24 29 34	—	—	7 15	24 33 38
8	8 35	24 33 06	1 25	24 40 47	7 15	24 33 04
9	8 35	24 31 30	1 25	24 39 28	—	—
10	8 35	24 30 18	1 25	24 38 32	7 15	24 33 10
11	8 35	24 29 12	1 25	24 39 31	7 20	24 32 50
12	—	—	—	—	—	—
13	8 35	24 31 15	1 25	24 42 15	7 15	24 34 42
14	8 40	24 30 25	1 40	24 41 34	7 10	24 33 54
15	8 35	24 29 57	1 20	24 39 41	7 20	24 34 18
16	8 40	24 29 29	1 20	24 39 00	7 20	24 34 28
17	8 40	24 29 39	1 15	24 39 49	7 30	24 33 36
18	—	—	1 55	24 38 01	7 30	24 34 08
19	8 05	24 32 49	—	—	—	—
20	8 40	24 28 40	1 15	24 40 09	7 30	24 34 07
21	8 45	24 32 44	1 20	24 40 06	7 35	24 33 34
22	8 35	24 28 25	1 25	24 37 53	7 35	24 33 26
23	8 40	24 31 47	1 25	24 40 46	7 40	24 33 42
24	8 35	24 34 56	1 30	24 39 37	7 40	24 34 08
25	8 45	24 31 12	1 45	24 38 29	7 35	24 33 00
26	8 05	24 28 00	—	—	—	—
27	—	—	1 20	24 41 44	7 35	24 32 56
28	8 40	24 27 20	1 25	24 46 10	7 25	24 30 41
29	8 35	24 30 45	1 15	24 40 58	7 35	24 30 10
30	8 40	24 30 02	1 20	24 38 52	7 35	24 32 48
31	8 40	24 30 26	1 30	24 40 38	7 35	24 33 13
Mean for Month.	8 35	24 30 42	1 25	24 40 08	7 24	24 33 00

Meteorological Observations.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
		Inches.				Feet.		
May	Morn....	29.848	48°	57°	W		Fine	41
	1 { Noon....	29.848	57	49	NW		Fine	58
	Even....	29.834	50	53	ENE		Fine	} 42
2 { Morn....	29.800	48	64	W by S		Cloudy	} 61	
Even....	29.745	57	52	NNW		Cloudy		} 42
3 { Morn....	29.682	44	70	Var.		Cloudy	} 53	
Even....	29.678	53	62	S		Cloudy		} 40
4 { Morn....	29.605	47	65	ESE		Cloudy	} 55	
Even....	29.500	45	60	E		Cloudy		} 34
5 { Morn....	29.453	53	52	ENE		Fine	} 54	
Even....	29.400	47	54	E		Cloudy		} 36½
6 { Morn....	29.378	45	57	NE		Fine	} 56	
Even....	29.379	54	50	NE		Fine		} 43½
7 { Morn....	29.385	47	52	NE		Very fine	} 60	
Even....	29.412	46	58	WSW		Clear		} 49½
8 { Morn....	29.369	56	48	WSW		Cloudy	} 60½	
Even....	29.300	—	66	SSW		Rain		} 51
9 { Morn....	29.142	49	64	WNW		Cloudy	} 63½	
Even....	—	—	—	—		—		} 47
10 { Morn....	29.215	52	51	W		Cloudy	} 63½	
Even....	29.236	56	76	S		Rain		} 50
11 { Morn....	29.238	58	63	SSE		Showery	} 62½	
Even....	29.179	55	74	SE		Showery		} 47
12 { Morn....	29.160	56	70	SW		Cloudy	} 65½	
Even....	29.182	62	58	SW		Very fine		} 47
13 { Morn....	—	—	—	—		—	} 64½	
14 { Morn....	29.343	57	65	SW by S		Cloudy		} 50
Even....	29.356	61	54	SW		Cloudy	} 44	
15 { Morn....	29.400	55	65	SW		Cloudy		} 63
Even....	29.466	55	68	SW		Cloudy	} 48	
16 { Morn....	29.479	61	63	SSW		Showery		} 59
Even....	29.500	55	59	SW		Fine	} 46	
17 { Morn....	29.546	—	—	SW		Cloudy		} 60½
Even....	—	—	—	—		—	} 47	
18 { Morn....	29.522	56	65	ENE		Cloudy		} 62
Even....	29.467	59	63	ESE		Cloudy	} 50	
19 { Morn....	29.303	56	65	SE		Cloudy		} 64½
Even....	29.404	57	67	SSW		Fine	} 44	
20 { Morn....	29.379	64	55	S by E		Cloudy		} 63
Even....	29.355	55	69	S		Showery	} 48	
21 { Morn....	29.343	57	66	SW		Cloudy		} 59
Even....	29.343	61	52	SW		Fine	} 46	
22 { Morn....	29.343	56	57	S		Fine		} 60½
Even....	29.236	51	81	W by S		Rain	} 47	
23 { Morn....	29.240	58	63	SW		Showery		} 55
Even....	29.248	52	65	SSW		Cloudy	} 46	
24 { Morn....	29.300	53	70	SW by S		Cloudy		} 60½
Even....	29.288	56	58	SW by S		Showery	} 47	
25 { Morn....	29.242	54	63	S		Fine		} 47
Even....	28.877	48	87	SW		Hardrain	} 55	
26 { Morn....	28.783	53	75	SSE		Stormy		} 55
Even....	28.873	47	77	SW		Stormy		

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
May		Inches.				Fect.		
19	Morn....	29·148	52°	70°	W by S		Very fine	46
	Noon....	—	—	—	—		—	60½
	Even....	—	—	—	—		—	—
20	Morn....	29·685	56	69	WSW		Cloudy	46½
	Noon....	29·742	61	60	WSW		Showery	63
	Even....	29·817	54	64	SW		Fine	46
21	Morn....	29·865	56	65	WSW		Fine	46
	Noon....	29·863	65	57	SSW		Fine	67
	Even....	29·836	58	60	S		Fine	49½
22	Morn....	29·746	61	62	SSW		Very fine	71½
	Noon....	29·719	70	53	S		Very fine	52½
	Even....	29·669	62	59	E by S		Clear	73
23	Morn....	29·554	65	60	ESE		Clear	73
	Noon....	29·500	72	53	ESE		Clear	55
	Even....	29·451	62	63	ESE		Clear	68
24	Morn....	29·284	66	68	S by W		Fine	49
	Noon....	29·312	65	58	W by S		Cloudy	60
	Even....	29·339	59	59	SW		Fine	44
25	Morn....	29·298	55	76	SW		Showery	60
	Noon....	29·289	55	72	SSW		Stormy	44
	Even....	29·266	52	75	SW		Showery	60½
26	Morn....	29·353	50	77	SW by S		Fine	50
	Noon....	—	—	—	—		—	60
	Even....	—	—	—	—		—	—
27	Morn....	29·208	—	82	Calm		Rain	60
	Noon....	29·183	57	67	Var.		Showery	49
	Even....	29·162	54	64	W by S		Fine	57
28	Morn....	29·123	54	66	W by S		Fine	45½
	Noon....	29·116	54	73	W by S		Rain	57
	Even....	29·044	51	80	SW		Rain	57
29	Morn....	28·915	53	64	W		Fine	41
	Noon....	28·871	56	59	W by S		Hail	56
	Even....	28·863	47	72	SW		Th.show.	44½
30	Morn....	28·859	49	62	W		Fine	57
	Noon....	28·862	56	54	W		Fine	41
	Even....	28·870	50	62	W by S		Rain	56
31	Morn....	28·900	52	62	W by S		Fine	44½
	Noon....	28·913	54	58	W		Showery	57
	Even....	28·930	48	65	W		Stomy	—

Rain, by the pluviometer, between noon the 1st of May, and noon the 1st of June, 3·383 inches. Evaporation, during the same period, 4·270 inches.

ARTICLE X.

METEOROLOGICAL TABLE.

1820.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.	Hygr. at 9 a. m.
		Max.	Min.	Max.	Min.			
5th Mo.								
May 1	Var.	30·36	30·33	61	35	—		66
2	Var.	30·33	30·21	62	41	—		69
3	E	30·21	30·06	67	41	—		76
4	E	30·06	29·90	60	31	35		67
5	N E	29·97	29·95	57	26	—		65
6	S W	29·95	29·70	65	44	—	—	67
7	N W	29·75	29·70	67	50	—	—	69
8	S E	29·71	29·70	65	50	48	14	73
9	S W	29·84	29·71	66	46	—		72
10	S W	29·97	29·84	67	51	—		68
11	S W	30·08	29·97	65	44	54		73
12	S W	30·06	30·03	70	36	—		71
13	N E	30·03	29·94	66	46	—		76
14	S W	29·94	29·90	67	47	30		74
15	S W	29·90	29·77	68	48	—	55	70
16	S W	29·81	29·76	63	46	—	12	92
17	W	29·81	29·33	63	49	—	35	74
18	S W	29·70	29·21	60	46	47	17	83
19	S W	30·19	29·70	68	46	—	10	73
20	S W	30·38	30·19	70	42	—	—	76
21	W	30·38	30·24	72	37	40		71
22	S E	30·24	30·04	78	42	—		72
23	E	30·04	29·80	76	46	34		73
24	S W	29·82	29·80	75	49	—	52	72
25	S W	29·85	29·82	60	45	—	09	72
26	S W	29·82	29·74	65	51	—	41	71
27	N W	29·74	29·68	63	49	44	08	82
28	W	29·68	29·36	62	46	—	08	73
29	W	29·36	29·33	60	41	—	21	72
30	W	29·36	29·33	62	46	—	—	71
31	W	29·60	29·36	62	42	45	03	71
		30·38	29·21	78	26	3·77	2·85	92—65

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Fifth Month.—1. *Cirrus*: *Cirrostratus*. 2. *Cirrocumulus*: cloudy. 3. Cloudy morning: day overcast and cold. 4. Cloudy: fine: cold wind. 5. Hoar frost: fine: windy. 6. *Cumulus*: a gentle shower in the evening. 7. *Cirrus*: *Cirrocumulus*. 8. Cloudy. 9. Cloudy: fine. 10. Cloudy: fine. 11. Cloudy. 12. Cloudy: *Cirrus*: *Cirrostratus*: and *Cirrocumulus*. 13. Fine. 14. Fine. 15. Cloudy: fine. 16. Showery. 17. Cloudy. 18. Showers. 19. Cloudy: fine. 20. Cloudy: fine. 21. *Cirrus*: *Cirrocumulus* and *Cirrostratus*; clear: a lunar halo in the evening. 22. A *Stratus* in the marshes early this morning: clear: *Cirrus*. 23. Clear. 24. Fine: windy. 25. Showery. 26. Showery. 27. Showery. 28. Showery. 29. A little hail at half-past eight, a.m.: another shower of hail about one o'clock, p.m. accompanied by thunder. 30. Overcast: *Cirrus*. 31. Showery: cold wind.

RESULTS.

Winds: NE, 2; NW, 2; W, 6; SW, 14; SE, 2; E, 3; Var. 2.

Barometer: Mean height

For the month.	29·860 inches.
For the lunar period, ending the 4th.	30·000
For 14 days, ending the 8th (moon south)	30·047
For 14 days, ending the 22d (moon north)	29·927

Thermometer: Mean height

For the month.	54·693°
For the lunar period, ending the 4th.	48·724
For 31 days, the sun in Taurus.	51·935

Hygrometer: Mean for the month 72·7

Evaporation. 3·77 inch.

Rain. 2·85

ANNALS

OF

PHILOSOPHY.

AUGUST, 1820.

ARTICLE I.

HISTORICAL SKETCH OF IMPROVEMENTS IN PHYSICAL
SCIENCE DURING THE YEAR 1819.

1. *The Chemical Sciences.* By Thomas Thomson, M.D. F.R.S.

(Concluded from p. 46.)

X. COMPOUND COMBUSTIBLES.

1. *Sulphuric Ether.*—Mr. Dalton has published an interesting paper on sulphuric ether, which I have inserted in the *Annals of Philosophy*, xv. 117. He gives us some information respecting its manufacture, and makes some inferences respecting its purity. Had he taken the trouble to consult the section on sulphuric ether in my *System of Chemistry*, vol. ii. p. 331, he would have seen that the specific gravity of pure sulphuric ether is only 0·632 instead of 0·720, as he supposes it to be. He would have seen too in p. 332 of the same volume that the specific gravity of the vapour of sulphuric ether is not 3·2, as he has made it by a set of experiments not susceptible of much precision, though exceedingly ingenious, but 2·586—determined by Gay-Lussac by a much more perfect apparatus.

The experiments which Mr. Dalton has made on the analysis of ether show in a very satisfactory manner that the notion which I threw out in my *System of Chemistry*, that sulphuric ether is a compound of two atoms olefiant gas and one atom vapour of water condensed into one volume, is the true one; for the proportion of oxygen necessary to consume a given volume of

etherial vapour together with the carbonic acid and water resulting are precisely what that supposition would render necessary, though Mr. Dalton does not appear himself to have seen the obvious consequences of his own experiments. The specific gravity of olefiant gas is 0.9708, and that of vapour of water 0.625. Hence

2 volumes olefiant gas weigh. 1.9416

1 volume vapour of water 0.6250

Total. = 2.5666

Specific gravity of ether vapour .. = 2.5860 according to the determination of Gay-Lussac. Now these two numbers are sufficiently near to warrant the conclusion that the specific gravities of the two are in reality the same. According to this view of the subject, the constituents of sulphuric ether are as follows :

Hydrogen. 13.513

Carbon. 64.865

Oxygen 21.622

100.000

Or, which comes to the same thing,

5 atoms hydrogen = 0.625

4 atoms carbon = 3.000

1 atom oxygen = 1.000

4.625

These conclusions differ very materially from those drawn by Dalton ; yet whoever will take the trouble to examine his experiments will find that they in reality lead to the above conclusion.

2. *Nitrous Ether*.—When crystallized nitrate of lead is distilled, a yellow-coloured liquid is obtained, which is *nitrous acid*, nearly, if not entirely, free from water. When absolute alcohol and this acid are mixed together in small quantities at a time to obviate as much as possible the violent heat which is evolved, nitrous ether is formed at once ; and this is probably the best way of forming that ether. (See *Ann. de Chim. et de Phys.* xii. 160.) Bouillon-Lagrange has proposed the following formula for preparing this ether :

Mix together equal parts of alcohol and nitric acid, and put the mixture into a matrass. Saturate this mixture with nitrous gas produced in the usual way. To the matrass are luted a set of Woulfe's bottles, each half way filled with a saturated solution of common salt. The mixture gradually becomes hot, and in about an hour and a half begins to boil. The Woulfe's bottles must now be surrounded with a refrigerating mixture. The boiling continues for half an hour. The whole nitric ether will

be found in the second phial. Eight ounces of alcohol will yield three ounces of nitric ether. (Jour. de Pharm. v. 433.)

3. *Naphtha*.—From the experiments which I have related in the *Annals of Philosophy*, xv. 307, compared with those of De Saussure, on the naphtha of Amiano, there is reason I think to conclude that the naphtha obtained artificially from coal by repeated distillations is precisely similar in its properties to the naphtha which rises spontaneously from the earth in Persia and other parts of the world, supposing both to be brought to the greatest possible state of purity. The following table exhibits the specific gravity of the different specimens of naphtha both artificial and natural which I have had an opportunity of examining.

	Sp. Gr. at 60°.
Pit coal naphtha (rectified 13 times)	0·850
Ditto perfectly colourless.	0·817
Persian naphtha not rectified	0·753
Naphtha of Amiano rectified	0·758

Naphtha, according to my trials, begins to boil at 320°, and it may be heated up to the temperature of 352°; so that its boiling point is about five degrees higher than that of oil of turpentine.

I found the specific gravity of the vapour of naphtha 2·263 at the temperature of about 55°. Saussure, who made his experiments at the temperature of 72·5°, found the specific gravity 2·833. I do not know whether to ascribe this difference to errors in our experiments, or whether it is not rather owing to the specific gravity increasing with the temperature, as happens to a great extent with the vapour of water and of alcohol, supposing these vapours in contact with the liquid from which they were evolved.

By analyzing naphtha by means of peroxide of copper, I found it composed of

13 atoms carbon	= 9·75
14 atoms hydrogen.	= 1·75
	11·50

So that an integrant particle of it weighs 11·5.

3. *Olive Oil*.—Dr. Clarke has related an interesting observation respecting the regular crystallization of olive oil. A phial of this oil having been left in a temperature of 35° exhibited a number of white opaque prismatic radii rising from the bottom of the vessel, and beautifully diverging in the transparent fluid. When examined with a glass, these crystals were found to have the form of mesotype; that is to say, they were rectangular prisms with square bases. (See *Annals of Philosophy*, xv. 329.)

4. *Pit Coal*.—The different varieties of pit coal known and employed for Great Britain for various purposes have hitherto been very much overlooked by chemists; I conceive, therefore,

that the arrangement of them which I have given (see *Annals of Philosophy*, xiv. 81, and xv. 394), founded upon the chemical constitution of each, but recognized likewise by the consumers, possesses some value, and will considerably facilitate future inquiries into the nature and properties of this most important combustible. I have divided pit coal into five different species; namely:

(1.) Kilkenny coal, which burns without flame, and cannot be coked.

(2.) Caking coal, which melts when heated, and then sticks together in large solid masses. It abounds in the counties of Durham and Newcastle, from which places it is carried to every part of the east and south coast of Great Britain.

(3.) Splint coal distinguished by the difficulty with which it breaks, and by the higher temperature necessary to set it on fire, but burning well when once kindled, and answering well for the manufacture of coke.

(4.) Cherry coal which has much of the aspect and friability of the caking coal; but possesses more beauty, and is easily distinguished by its not melting when heated. It abounds in Staffordshire, and in various parts of Scotland.

(5.) Cannel coal easily distinguished from the other kinds by its not soiling the fingers, by its being susceptible of polish, and of being cut into boxes, inkhorns, and other similar trinkets. It catches fire like a piece of wood, and burns like a candle.

The composition of these different kinds of coal is as follows:

(1.) Kilkenny coal contains only carbon and oxygen in the proportion of 35 atoms of the former to two of the latter; so that its composition may be represented thus:

$$\begin{array}{r} 35 \text{ atoms carbon.} \dots\dots\dots = 26\cdot25 \\ 2 \text{ atoms oxygen.} \dots\dots\dots = 2\cdot00 \\ \hline 28\cdot25 \end{array}$$

The three next kinds of coal contain carbon, hydrogen, azote, and oxygen; but cannel coal appears to contain no oxygen, but to be a compound of carbon, hydrogen, and azote. Caking coal contains the least hydrogen compared to the quantity of carbon.

In caking coal there are 3 atoms carbon for 1 atom hydrogen
splint coal there are 2 _____ 1
cherry coal there are 1 _____ 1
cannel coal there are 1 _____ 2

The following table exhibits the number of atoms of each constituent in each of these four kinds of coal:

	Atoms.	Atoms.	Atoms.	Atoms.
Caking coal.	33 carbon	+ 11 hydrogen	+ 3 azote	+ 1·5 oxygen
Splint coal ..	28	+ 14	+ 1	+ 3·5
Cherry coal .	34	+ 34	+ 2	+ 1·0
Cannel coal .	11	+ 22	+ 1	+ 0·0

5. Mr. Garden has pointed out a very singular substance from coal tar, which has at first sight some resemblance to spermaceti. It is white, has an aromatic smell, and, when heated, melts and sublimes in beautiful silky plates. It dissolves in alcohol, and in fixed and volatile oils, and acetic acid; but is insoluble in water (*Annals of Philosophy*, xv. 74). From a few trials which I was enabled to make upon this substance by the kindness of a friend, I consider it as a compound of three atoms carbon + two atoms hydrogen. It is, therefore, the same thing as an atom of olefiant gas united to an atom of carbon.

XI. MINERAL WATERS.

1. *Boiling Spring in the Harbour of Milto.*—I had an opportunity of analyzing a specimen of this water about a year and a half ago. It has a strong saline taste, and its specific gravity is 1.0331. The saline constituents in 500 grs. of this water, determined in the way described in the *Annals of Philosophy*, xiv. 27, are as follows:

Common salt	20.924
Muriate of lime	3.505
Sulphate of soda.	0.684
	<hr/>
	25.113

So that it contains about half a per cent. more of salt than sea water does.

2. *Aluminous Chalybeate on the Coast of Sussex.*—Mr. Cooper examined a specimen of mineral water from the coast of Sussex between Newhaven and Rottingdean. It flows from chalk, and its temperature is uniformly 65° as it issues from the earth. Its specific gravity is 1.076; so that it is greatly heavier than the boiling spring in the island of Milto. It is slightly acidulous, and has the taste of iron. Mr. Cooper detected the following substance in it:

Oxide of iron,	Lime,
Alumina,	Carbonic acid,
Muriatic acid,	Soda.
Sulphuric acid,	

(See *Annals of Philosophy*, xiv. 148.)

3. *Sea Water.*—We are indebted to Dr. Marcet for an extensive set of experiments on sea water collected from different parts of the ocean, and from various seas. Of this a full account has been given so lately in the *Annals of Philosophy*, xv. p. 439, that it seems only necessary to refer the reader thither for a view of the details. I may mention, however, Dr. Wollaston's discovery of the presence of potash in sea water to a very small amount.

4. *Lake Ourmia.*—In the same paper, Dr. Marcet has given us the analysis of the water of the Lake Ourmia, in Persia, situated

not far from the region of Mount Ararat. A small quantity of the water from this Lake was sent by the late unfortunate traveller Brown to Mr. Smithson Tennant. This portion was subjected to analysis by Dr. Marcet. Its specific gravity was 1·16507 : 500 grs. of it being subjected to analysis yielded the following saline contents :

Common salt.	85·00
Muriate of magnesia.	10·08
Sulphate of soda	40·26
	135·34

The waters of this lake then contain more salt than any other mineral water hitherto examined, except the waters of the Dead Sea. (See *Annals of Philosophy*, xiv. 150.)

5. *Dead Sea*.—The waters which constitute this sea have a greater specific gravity, and contain more salt than any other mineral water hitherto examined. The specific gravity, as found by different experimenters, varies a little. Thus

Klaproth found it.	1·245
Gay-Lussac.	1·2283
Dr. Marcet.	1·211

I have lately drawn the attention of chemists to the discordant analyses of this water by some of our most accurate chemists. It affords a humiliating proof of the little progress that the analysis of waters has yet made, and the little confidence which can be put in the most accurate of our experimenters. It is, therefore, of the utmost consequence that every one who publishes an analysis of a mineral water should record accurately the successive steps of his analysis, and describe the mode which he employed in calculating the respective proportions of each constituent. Attention to this will render accurate experiments always of value ; while those that merely give the results may rest assured that their conclusions will be of no value hereafter when the mode of analysis has become much more perfect than it is at present. It will be worth while to give here again the saline contents extracted from 100 grs. of Dead Sea water by Klaproth, Marcet, and Gay-Lussac.

	Marcet.		Klaproth.		Gay-Lussac.
Common salt,	10·676	7·80	6·95
Muriate of lime.	3·792	10·60	3·98
Muriate of magnesia.	10·100	24·20	15·31
Sulphate of lime	0·054	—	—
	24·622		42·60		26·24

Klaproth's salts were only dried at the temperature of boiling water. This accounts for the much greater weight of his salts.

But how can we reconcile the great discordance between Marcet and Gay-Lussac in the weights of common salt and of muriate of magnesia?

XII. VEGETABLE SUBSTANCES.

1. *Starch*.—It has been long known to chemists that when starch is exposed to heat till its colour begins to change and to become yellow, its properties are so far altered that it is now soluble in cold water, and forms a viscid, brown-coloured solution, which may be substituted for an aqueous solution of starch. M. Lassaigne has made some experiments to determine whether starch by this roasting process is converted into gum.

When the aqueous solution of roasted starch is evaporated to dryness, it leaves a brownish-yellow viscid matter. Boiling alcohol, when digested on this matter, acquires a fawn colour, and when evaporated to dryness leaves a brownish-yellow matter, having a bitter taste similar to that which several vegetable bodies acquire when burnt; but the greater part of the residue was not acted upon by the alcohol. This portion being dissolved in water, and the solution evaporated to dryness in a moderate heat, left thin transparent plates, of a reddish-yellow colour, very similar to the gum which flows from certain trees at the period that they ripen their fruit; but when this apparently gummy matter was treated with nitric acid, it yielded oxalic acid without any trace of saclactic acid. Hence it is obvious that it was not in reality gum. (*Jour. de Pharm.* v. 300.)

In the Philosophical Transactions for 1819, there is inserted a paper by M. Theodore de Saussure giving the result of a set of experiments upon the spontaneous decomposition of a mixture of starch and water, when left to itself, either in vacuo, or with the access of air. A portion of the starch disappears amounting to little less than one-fourth, and the remaining three-fourths were converted into the following substances:

Sugar,
Gum,
Amylin,
Starchy lignin,
Lignin mixed with charcoal,
Starch undecomposed.

The sugar possessed the characters of starch sugar, and amounted to nearly half the weight of the starch.

The gum possessed nearly the characters of the matter into which starch is changed by roasting. We see from Lassaigne's experiments, given above, that it differs essentially from gum in its properties.

The *amyline* (called *amidine* by Saussure) is a substance intermediate between gum and starch. It is obtained from the

residue left by the spontaneous decomposition of starch after it has been treated with a sufficient quantity of cold water to take up every thing soluble in that liquid. Boiling water dissolves the amylin, and it may be obtained by evaporating the solution to dryness. It is a pale semitransparent brittle substance, insoluble in alcohol, soluble in 10 times its weight of cold water. Water, of the temperature 144° , dissolves it in any quantity, and retains in solution after cooling a much greater proportion than can be dissolved in cold water. When the liquid is more concentrated, the amylin precipitates in part on cooling in the state of a white opaque matter; but it is redissolved on heating the water to 144° . The aqueous solution assumes a blue colour when treated with iodine. It is coagulated into a white paste by the subtriacetate of lead, but not by the acetate. It is copiously precipitated by barytes water; but neither by lime water, nor infusion of nutgalls. It dissolves in the aqueous solution of potash, and the solution has no viscosity. From this solution, it is thrown down by acids and by alcohol.

The *starchy lignin* was obtained from the residue of the spontaneous decomposition of starch after that residue had been deprived of every thing soluble in cold water, hot water, alcohol, and very dilute sulphuric acid, by digesting it in 10 times its weight of an alkaline ley containing one-twelfth of potash. A brown solution is obtained, from which dilute sulphuric acid precipitates the starchy lignin under the form of a light-brown combustible substance having the lustre of jet. It gives a blue colour to the aqueous solution of iodine. This last property, together with its solubility in a weaker alkaline ley, distinguishes starchy lignin from common lignin.

2. *Gluten*.—I noticed in a late number of the *Annals of Philosophy* (xv. 390), that M. Taddey, an Italian chemist, has recently discovered that the gluten of wheat may be decomposed into two principles, which he has distinguished by the names of *gliadine* and *zimome*. These two substances are obtained by kneading gluten with repeated portions of alcohol as long as that liquid becomes milky when diluted with water. The alcohol dissolves the gliadine, and leaves the zimome.

Gliadine, obtained by evaporating the alcoholic solution, is a brittle, straw-yellow, slightly transparent, substance, having a weak smell, similar to that of honeycomb, and, when gently heated, giving out an odour similar to that of boiled apples. In the mouth, it becomes adhesive, and has a sweetish and balsamic taste. It is pretty soluble in boiling alcohol, but the greater part precipitates as the solution cools. It softens, but does not dissolve in cold water. The alcoholic solution of gliadine becomes milky, when mixed with water; it is precipitated in white flocks by the alkaline carbonates. Dry gliadine dissolves in caustic alkalies and acids. It swells on burning coals, and then contracts in the manner of animal bodies. It burns with a

pretty lively flame, and leaves behind it a light spongy charcoal, difficult to incinerate.

Zimome is obtained pure by boiling gluten in alcohol, or by digesting it in that liquid till it ceases to give out gliadine. Thus obtained, it constitutes a shapeless mass, which is hard, tough, destitute of cohesion, and of an ash-white colour. When washed in water, it recovers part of its viscosity, and becomes quickly brown when left in contact with the air. It is specifically heavier than water. Its mode of fermenting is no longer that of gluten, for when it putrifies, it exhales a fetid urinous odour. It dissolves completely in vinegar, and in the mineral acids at a boiling temperature. With caustic potash, it combines, and forms a kind of soap. When put into lime water, or into solutions of the alkaline carbonates, it becomes harder, and assumes a new appearance. When thrown upon red-hot coals, it exhales an odour similar to that of burning hair or hoofs, and burns with flame.

3. *Camphor*.—I have given in a late number of the *Annals of Philosophy*, the result of my experiments to ascertain the constituents of camphor. This substance I find melts when heated to 288°, and boils at the temperature of 400°. Its constituents are:

8½ atoms carbon.....	=	6·375 or 73·91
10 atoms hydrogen....	=	1·250 14·49
1 atom oxygen.....	=	1·000 11·60
		8·625 100·00

(*Annals of Philosophy*, xv. 392.)

4. *Galbanum*.—M. Meisner has found galbanum composed of the following constituents:

Resin	65·8
Gum.	22·6
Cerasin.	1·8
Malic acid	0·2
Volatile oil.....	3·4
Vegetable debris.	2·8
Loss.	3·4
	100·0

(*Ibid.* xiv. 385.)

5. *Root of Jesuit's Bark*.—It has been ascertained that the root possesses the febrifuge properties in fully as great perfection as the bark of the cinchona. It is obvious, however, that it could not be employed, except at the hazard of destroying entirely the best trees. (*Jour. de Pharm.* v. 44.)

6. *Gentian*.—The root of the gentiana lutea has been subjected to analysis by M. Henry, who extracted from it the following substances:

(1.) A substance possessing very nearly the properties of bird lime.

(2.) A resin united to a little oil, which gives its odour to gentian.

(3.) An extractive, similar to the extract of cinchona, which constitutes the greatest part of gentian, and which possesses the bitter taste and the other characters which distinguish this plant. It possesses no alkaline characters; but agrees with those already recognized in the *bitter principle*.

(4.) Gum united to a colouring matter.

(5.) A salt with a calcareous base possessing the characters of a phosphate.

(6.) It contains neither starch nor inulin, nor any alkaline substance. (Jour. de Pharm. v. 97.)

7. *Flowers of the Arnica Montana*.—These flowers have been subjected to a chemical examination by MM. Chevallier and Lassaigne, who have obtained from them the following substances:

(1.) A resin having the odour of arnica.

(2.) A bitter nauseous substance, resembling the emetic substance from the *Cytisus laburnum* (*Cytisine*).

(3.) Gallic acid.

(4.) A yellow colouring matter.

(5.) Albumen.

(6.) Gum.

(7.) Muriate and phosphate of potash.

(8.) Traces of sulphates.

(9.) Carbonate of lime.

(10.) Silica.—(Ibid. v. 248.)

8. *Seeds of Mustard (Sinapis Nigra)*.—M. Thibierge has examined mustard, and found it to contain the following constituents:

(1.) A fixed tasteless oil which may be obtained by expression.

(2.) A volatile oil which may be obtained by distilling a mixture of mustard and water, and dropping a little soda into the milky liquid which comes over. This oil is the substance to which mustard owes its acrid properties. It is soluble in water; but not in alcohol or ether, and when kept for some time it allows a quantity of sulphur to precipitate. It acts as a vesicatory.

(3.) Albumen.

(4.) Gum.

(5.) Sulphate and phosphate of lime.

(6.) Silica.—(Ibid. v. 439.)

It deserves to be again noticed, that M. Drapiez has found that the fruit of the *fewillea cordifolia* is a powerful antidote against vegetable poisons. (*Annals of Philosophy*, xv. 389.)

9. *Lignin*.—One of the most remarkable circumstances connected with the vegetable principles is the property which

several of them have of being converted into another principle possessed of quite different properties. Thus starch, when treated with sulphuric acid, is changed into sugar. Gum, when treated with nitric acid, is converted into saccharic acid. This property will doubtless hereafter throw much light both upon the constitution of vegetable substances, and upon the phenomena of vegetation, which are at present wrapped up in great obscurity. A set of experiments recently made by M. Braconnot has added several unexpected facts to the stock of our knowledge respecting the changes of vegetable principles still so imperfect.

When the sawings of the hornbeam well dried are mixed and agitated with sulphuric acid, considerable heat is evolved, sulphurous acid is disengaged, and the whole becomes black. When mixed with water, a black powder falls, which, when dried, burns with flame, and possesses nearly the characters of sawings of hornbeam exposed to the air and moisture for several years. The sulphurous liquor was nearly colourless, and being saturated by carbonate of lime, and filtered, yielded, when evaporated, a gum possessing many characters similar to those of gum arabic.

Hemp cloth, being treated in the same way, dissolved in sulphuric acid, but no sulphurous acid was disengaged. The solution, being mixed with water, and treated as that of the sawings of hornbeam, yielded a quantity of gum quite similar in its characters to the gum from the hornbeam: 21.5 parts of hemp furnished 26.2 parts of this gum, which Braconnot found composed as follows:

Matter from the hemp	21.50
Elements of sulphuric acid fixed in an unknown manner	2.83
Elements of water, ditto	0.40
Lime	1.47
	<hr/>
	26.20

The gum is transparent, yellowish, without smell or taste, and breaks with a vitreous fracture. However, it reddens vegetable blues. It adheres strongly to bodies, and forms a very shining varnish. Its solution in water has less tenacity than that of gum arabic. When heated sufficiently, it burns, giving out a strong odour of sulphurous acid. There remains a charcoal, which, after incineration, leaves some traces of sulphate of lime. Its solution in water is not precipitated by nitrate of barytes, or acetate of lead; but subtriacetate throws down a copious precipitate which is totally dissolved by acetic acid.

If this gum be boiled for some time with dilute sulphuric acid, it is converted into two new substances; namely, *sugar*, and an acid which Braconnot has called *vegeto-sulphuric acid*. Twenty-

four parts of old linen rags were reduced into a mucilage with 34 parts of strong sulphuric acid. The acid solution being diluted with water, 3.6 parts of undecomposed lignin fell. The acid liquid thus diluted was boiled for 10 hours, and then saturated with carbonate of lime. The filtered liquid being evaporated to dryness, left a sweet-tasted matter, which weighed 23.3 parts. This matter gradually crystallized, and exhibited all the characters of sugar of starch.

The vegeto-sulphuric acid was separated from the sugar by means of alcohol; but the alcohol dissolved not merely the acid, but likewise a portion of the sugar. The solution being evaporated to dryness was treated with sulphuric ether, which dissolved the acid, but left the sugar. This acid is deliquescent, and cannot be crystallized; it is nearly colourless, has an exceedingly sour taste, and sets the teeth on edge. When heated, it becomes black, and if it be diluted with water, black flocks of charcoal separate. When heated above the temperature of boiling water, it is decomposed, and sulphurous acid exhales. It does not precipitate a single metallic solution, and all the salts which it forms are soluble in water. None of its salts seems capable of crystallizing. The nature of this acid has not hitherto been determined. It possesses some characters similar to those of hyposulphuric acid, but some of its other properties differ entirely from that acid.

When equal weights of caustic potash and sawings of wood are heated in a silver or iron crucible, stirring the mixture constantly at a certain temperature, it softens and dissolves completely, swelling up greatly at the same time. If it be now taken from the fire, and water poured upon it, the whole dissolves with the greatest facility, and we obtain a dark-brown liquid, consisting of a combination of potash and *ulmin* dissolved in water. If an acid be dropped into this liquid, the *ulmin* falls down in great abundance, and requires only to be washed and dried.

Artificial *ulmin* thus obtained has a brilliant black colour, like jet. It is very brittle, and easily broken into angular fragments. It has no smell, and little taste. In this state it is insoluble in water; but when just precipitated, and still moist, water dissolves $\frac{1}{200}$ of its weight of it. The solution is coffee-coloured. Nitrate of silver, persulphate of iron, nitrate of barytes, acetate of alumina, muriate of lime, muriate of soda, throw down brown precipitates from it, but they do not appear till some time after the reagents have been added. Lime and litharge deprive the water entirely of its colour. It is soluble in concentrated sulphuric acid, and precipitated by water. Alcohol dissolves it with facility, and forms a dark-brown liquid which is precipitated by water. (Ann. de Chim. et de Phys. xii. 172.)

XIII. ANIMAL SUBSTANCES.

1. *Picromel*.—This substance though it constitutes the charac-

teristic ingredient in bile has more relation to vegetable than to animal substances in its properties ; and indeed approaches very nearly to sarcocol or liquorice sugar. The result of a set of experiments which I made to determine the composition of this substance gave me the following as its constituents :

5 atoms carbon	=	3·750	or	54·53
1 atom hydrogen	=	0·125		1·82
3 atoms oxygen	=	3·000		43·65
		6·875		100·00

(*Annals of Philosophy*, xiv. 70.)

2. *Honey*.—A quantity of honey which had been kept for two years was observed to undergo a kind of fermentation, after which grains of sugar separated from it in abundance. These being examined by M. Chevallier were found to possess the characters of sugar of grapes. He supposes that the honey consisted originally of two kinds of sugar ; one essentially liquid, and another capable of crystallizing. The fermentation having destroyed the former, the second was at liberty to assume the crystalline form. (*Jour. de Pharm.* v. 253.)

3. *Ambergris*.—It is well known that this substance is found floating on the sea chiefly within the tropics. Naturalists are not yet agreed about its origin. Swediaur, in a paper published long ago in the *Philosophical Transactions*, endeavoured to prove that it was the indurated excrements of the phlyseter macrocephalus ; and this opinion has been pretty generally adopted. Two new opinions respecting the origin of this substance have been lately started founded upon its chemical nature. Bouillon Lagrange, who analyzed ambergris some years ago, found a substance in it which he considered as similar, if not the same, with the adipocire of Fourcroy. This circumstance has induced M. Virey to advance the opinion, that ambergris is formed by the putrefaction of animal bodies at the bottom of the sea. (*Jour. de Pharm.* v. 386.) But he has not produced any evidence whatever in support of his opinion better than conjecture.

MM. Pelletier and Caventou have lately subjected the adipocircous matter of ambergris to a chemical examination, and found it exceedingly similar to a substance found in biliary calculi, to which Chevreul, who subjected it to an accurate examination, gave the name of *cholesterine*. This circumstance has led these gentlemen to conclude, that ambergris is probably a biliary concretion of the species of whale in whose intestines Swediaur produced evidence that it had been found. This opinion appears to me by far the most probable of any hitherto advanced, unless the great size of the masses of ambergris occasionally found (amounting to several hundred weights) be not considered as inconsistent with such a notion.

Pelletier and Caventou have distinguished the adipocircous

matter of ambergris by the name of *ambreine*. It may be obtained by digesting ambergris in hot alcohol of the specific gravity 0.827. The alcohol on cooling deposits the ambreine in very bulky and irregular crystals, which still retain a very considerable portion of alcohol. Thus obtained, it possesses the following properties :

It is of a brilliant white colour, has an agreeable odour, of which it is deprived by repeated solutions and crystallizations. It is destitute of taste, and does not act upon vegetable blues. It is insoluble in water, but dissolves readily in alcohol and ether, and in much greater quantity in these liquids when hot than when cold. It becomes soft when heated to the temperature of 77°, and melts at the temperature of 86°. When exposed to a temperature exceeding 212°, it flies off in the state of a white smoke, while a portion of it is decomposed.

It does not seem capable of combining with an alkali, or of being converted into a soap. When heated with nitric acid, it becomes green and then yellow, while nitrous gas is exhaled. By this absorption of oxygen, it is converted into an acid to which Pelletier and Caventou have given the name of *ambreic acid*.

Pure ambreic acid is yellow while in a considerable mass, but when spread thin, it is nearly white. It has a peculiar smell. It reddens vegetable blues. When heated, it does not melt even at the temperature of 212°, and when heated till it undergoes decomposition no ammonia is given off. It dissolves readily in alcohol and ether. It is likewise soluble, but only in a very small degree in water, though hot water dissolves more of it than cold water. The solution of this acid may be saturated with potash. When the alkali is added rather in excess, no precipitate appears, even when the liquid is concentrated by evaporation; but if the alkali be added only in sufficient quantity to saturate the acid, white flocky precipitate falls, which is a super-ambreate of potash. Ambreate of potash forms a yellow precipitate when mixed with solutions of the following salts :

Muriate of lime,	Acetate of lead,
Muriate of barytes,	Corrosive sublimate,
Sulphate of copper,	Muriate of tin,
Sulphate of iron,	Muriate of gold.
Nitrate of silver,	

The gold in the last precipitate is not reduced till after an interval of some hours. (Jour. de Pharm. v. 49.)

4. *Animal Charcoal*.—This substance appears to be a compound of carbon and azote. We are at present acquainted with three compounds of these two bodies.

(1.) *Cyanogen* composed of two atoms carbon and one atom azote.

(2.) *Azoturet of carbon*, observed by Gay-Lussac under the

form of charcoal, and composed of three atoms carbon and one atom azote.

(3.) Common animal charcoal, which according to the analysis of Dobereiner, is composed of six atoms carbon and one atom azote. (*Annals of Philosophy*, xv. 388.)

5. *Bile of the Yellow Coaita (Ateles Arachnoides)*.—MM. Chevallier and Lassaigne have analyzed the bile of this animal, and found the following constituents :

Albumen,
A yellow-colouring matter,
Green resin,
Picromel,
Muriates of potash and soda,
Phosphate of soda,
A trace of phosphate of lime.

(Ann. de Chim. et de Phys. xi. 104.)

6. *Bile of the Cougar (Felix Discolor)*.—The same chemists have found in the bile of this animal the following constituents :

Albumen,
Yellow matter,
Green resin,
Picromel.—(Ibid. p. 106.)

7. *Pus of Venereal Sores*.—Chevallier, from the pus of an ill-conditioned venereal ulcer, extracted the following substances :

Water,
Ammonia,
Albumen,
Fatty matter,
Muriates of potash, soda, and ammonia,
A trace of sulphate,
Osmazome,
Gelatine.

From the pus of a well-conditioned venereal ulcer, he extracted the following substances :

Water,
Albumen,
Fatty matter,
Muriates of potash and soda, and a trace of muriate of ammonia,
A trace of sulphate,
Gelatine.—(*Annals of Philosophy*, xiv. 232.)

8. *Earthy Mass from a Wen*.—This substance was subjected to analysis by Dr. Prout, who found it composed of

Animal matter	35
Phosphate of lime	61
Carbonate of lime with traces of phosphate and carbonate of magnesia	4
	100

So that it contained more phosphate of lime and less carbonate of lime than bone.—(Ibid. p. 233.)

9. *Ptyalism*.—There is a curious case of spontaneous ptyalism with diminished secretion of urine related by Dr. Prout (*Annals of Philosophy*, xiv. 474). The saliva was said to have a urinous taste: 1000 grs. of it, when evaporated to dryness, left 8.65 grs. of residuum, which consisted of

Animal matter peculiar to saliva	3.33
Ditto soluble in alcohol, and similar to those found in blood	1.06
Sulphuric acid.	0.90
Muriatic acid	0.75
Phosphoric acid.	0.06
Alkali, partly soda, partly potash.	2.55
	8.65

The specific gravity of the urine of this person was 1.0131. It was amber coloured, and contained more acid and less urea than usual. Diuretics increased the flow of urine, and diminished that of the saliva.

10. *Urine of the Sow*.—M. Lassaigne subjected the urine of this animal to chemical analysis, and found in it the following substances:

Urea,
Muriate of ammonia,
Muriate of potash,
Muriate of soda,
Sulphate of potash,
Sulphate of soda,
Trace of sulphate and carbonate of lime.

(*Annals of Philosophy*, xiv. 146.)

11. *Urinary Calculus*.—I subjected to analysis a fragment of a calculus taken after death from the bladder of a person who had been very much afflicted with gout. I found it composed of

Uric acid,
Phosphate of lime,
Lime.

Probably the lime was in combination with the uric acid (Ibid. xiv. 468.)

12. *Pink Sediment in Urine.*—It appears from an analysis of this substance by Dr. Prout that it consisted chiefly of urate of ammonia. What is called the lateritious sediment in urine is usually a mixture of phosphate of lime and of magnesia with urate of soda. The red colour is usually owing to the presence of purpurate of ammonia, or soda. Dr. Prout informs us that he has detected nitric acid in such sediments. The following was the method he adopted: The sediment was digested with a little pure barytes. The nitrate of barytes thus formed was of course soluble, while the other compounds were insoluble. Sulphate of potash was now added to the solution of nitrate of barytes, and the nitrate of potash thus formed was obtained on evaporating the solution in one instance in crystals, and in several others its presence was rendered distinctly perceptible by its mode of combustion with paper and other substances containing carbon. Some specimens of these sediments contained very little or no nitric acid. The specimen yielding the most was deposited from the urine of a gouty patient labouring under a febrile attack. (See *Med. Chirurg. Trans.* vol. ix. p. 481.)

13. *Urinary Calculi.*—It seems sufficient to refer the reader to Dr. Henry's excellent paper on urinary calculi which has been inserted in the *Annals of Philosophy*, xv. 107.

14. The same reference may suffice respecting the extraordinary statement of calculi from a dog possessing the chemical properties of pearl. (See *ibid.* xv. 306.)

XIV. MINERALS.

1. *Fibrous Prehnite.*—This is the variety of prehnite which occurs in the neighbourhood of Glasgow. Its specific gravity, hardness, and chemical composition, differ from those of foliated prehnite. Hence I think it ought to be ranked as a species apart. I found the constituents of a specimen from the neighbourhood of Paisley as follows:

Silica.....	43·60
Alumina.....	23·00
Lime.....	22·33
Oxide of iron.....	2·00
Water.....	6·40
Loss.....	2·67

100·00

(*Annals of Philosophy*, xiv. 67.)

2. *Necronite.*—This is a name given by Dr. Hayden to a mineral found by him in primitive limestone about 21 miles from Baltimore. The name was imposed in consequence of the very fetid smell by which the mineral is distinguished (from νεκρον). The description is too imperfect to enable us to form any accurate idea of the mineral, or to be able to determine whether it be

of a peculiar nature. Its colour is white or bluish-white, its fracture foliated, and the shape of the crystal is described as sometimes the same with that of felspar, and sometimes a six-sided prism (two figures scarcely compatible with each other). Its hardness is equal to that of felspar, and it is infusible *per se*. Had the specific gravity and the chemical composition of the mineral been stated, it would have enabled us to judge with greater accuracy whether the necronite be not a variety of some mineral already known. Were it not for the shape of its crystals, I should be disposed to consider it as a variety of spinell. (See *Annals of Philosophy*, xiv. 68.)

3. *Carbonate of Magnesia*.—This mineral in the state of fine acicular crystals has been discovered by Mr. James Peirce constituting veins in a serpentine rock at Hoboken, in Staten Island, on the New Jersey side of the Hudson. (Ibid. p. 69.)

4. *Zircon*.—I have mentioned in the *Annals of Philosophy*, xiv. 147, that this mineral is not a simple combination of silica and zirconia, as has been hitherto supposed, but that it contains likewise a quantity of alumina.

5. *Gehlenite*.—In consequence of Dr. Clarke's discovery of potash in this mineral, I subjected it to two different analyses without being able to extract any of that alkali. I consider myself, therefore, entitled to affirm, that the quantity of alkali which gehlenite contains, if any, must be extremely small.

During my experiments on gehlenite, a phenomenon occurred, which (though I might have anticipated it) occasioned me some trouble at first. The gehlenite reduced to a fine powder was fused with four times its weight of nitrate of barytes. After softening with water, the whole was dissolved in muriatic acid, and the barytes thrown down by means of sulphuric acid. After separating the sulphate of barytes, the whole of the earthy bodies in the solution were precipitated by means of carbonate of ammonia. The liquid after filtration was evaporated to dryness, and the dry residue was exposed to a red heat in a platinum crucible. A saline substance remained, which deliquesced on exposure to the air, and possessed the taste and all the characters of muriate of lime. I obtained the same residual salt in two different experiments before I turned my attention to the circumstance. The cause of this residue I suppose to be that a little of the lime is kept in solution by means of the excess of carbonic acid (for the carbonate of ammonia of commerce is always a sesquicarbonate at least). This portion at a red heat decomposes a portion of the sal-ammoniac, and forms muriate of lime. I find the existence of this residual salt to be uniform whenever muriate of lime is precipitated by the carbonate of ammonia of commerce.

6. *Egeran*.—This mineral, considered as a variety of idocrase, has been analyzed by Count Dunin Borkowski, and found to be composed as follows:

Silica	41
Alumina	22
Lime	22
Magnesia	3
Iron.	6
Manganese	2
Potash.	1
	<hr/>
	97

(Annals of Philosophy, xv. 146.)

7. *Celestine*.—A variety of this mineral has been found at Nörten, near Hanover, composed according to M. Gruner, of

Sulphate of strontian	73·000
Sulphate of barytes.	26·166
Ferruginous alumina	0·213
Loss	0·621
	<hr/>
	100·000

(Ibid. xv. 283.)

8. *Euclase*.—This rare mineral has been lately analyzed by Berzelius, and found to be composed of

Silica	43·22
Alumina.	30·55
Glucina	21·78
Oxide of iron	2·22
Oxide of tin	0·70
	<hr/>
	98·47

Berzelius considers it as $G S + 2 A S$. (Ann. de Chim. et de Phys. xi. 216.)

9. *Wavellite*.—This mineral has been analyzed by Prof. Fuchs and Prof. Berzelius. They both concur in finding it to be a hydrous phosphate of alumina. The following table exhibits the results which they obtained :

	Fuchs.	Berzelius.
Alumina.	37·20	35·35
Phosphoric acid	35·12	33·40
Fluoric acid	—	2·06
Lime	—	0·50
Oxides of iron and manganese	—	1·25
Water	28·00	26·80
	<hr/>	<hr/>
	100·32	99·36

(See *Annals of Philosophy*, xiv. 278, and Ann. de Chim. et de Phys. xii. 16.)

Professor Fuchs has shown that the mineral called *lasionite* agrees exactly in its composition with wavellite.

10.—The green variety of fluor spar discovered lately in the lead mine of Middlehope Shields, near the village of Westgate, in the county of Durham, has been described by Dr. Clarke (*Annals of Philosophy*, xiv. 34). I have received several specimens of it, and can attest its great beauty.

11. *Ore of Tellurium*.—I have not yet had an opportunity of seeing any specimens of the ore of tellurium said to have been discovered in Connecticut, at New Stratford, and cannot, therefore, add any thing to the account which I transcribed from the *American Journal*. (See *Annals of Philosophy*, xiv. 66.)

12. *Compound of Sulphuret of Lead and Arsenic*.—This is a new mineral species discovered by Mr. Smithson, and described by him in the *Annals of Philosophy*, xiv. 96. It was found in a magnesian lime rock in the Upper Valais. It has a metallic aspect, a grey colour, and a fracture in some directions vitreous, in others foliated. When triturated, yields a red powder. Mr. Smithson, by a set of very minute but satisfactory experiments, demonstrated that its constituents were sulphur, arsenic, and lead.

13. *Plomb Gomme*.—Mr. Smithson has given us some interesting details respecting the history and properties of this mineral, which is a *hydrous aluminate of lead*. It has a yellow colour, and is exceedingly similar in appearance to Mullen glass. When heated, it decrepitates violently; and if it be heated by the blow-pipe, in contact with an alkali, lead is reduced. Its nature was first ascertained by Mr. Tennant. Berzelius has lately analyzed it. The result of his analysis will be found in the *Annals of Philosophy*, xiii. 381. (See *Annals of Philosophy*, xiv. 31.)

14. *Octahedral Iron Ore*.—M. Robiquet has shown that this ore usually contains titanium. (*Ibid.* p. 384.)

15. *Arsenical Nickel (Cupfer Nickel)*.—This mineral from Allemont has been lately subjected to an elaborate analysis by M. Berthier, who found it composed of

Nickel	39·94
Cobalt	00·16
Arsenic	48·80
Antimony	8·00
Sulphur	2·00
Iron and manganese	Trace
	<hr/>
	98·90

Or its constituents may be thus stated:

Arseniuret of nickel	88·55
Arseniuret of cobalt	0·35
Sulphate of antimony	10·00
	<hr/>
	98·90

(*Ann. de Chim. et de Phys.* xiii. 52.)

16. *New Nickel Ore.*—This is an ore mentioned by Cronstedt as existing in Sweden. It has been recently described and analyzed by Pfaff.

Its colour is lead-grey, the lustre splendid and vitreous, the fracture foliated; and it is composed of grains resembling steel-grained galena. It is opaque, and its specific gravity is 6.129. Its constituents are :

Nickel	24.42
Arsenic	45.90
Sulphur	12.36
Iron	10.46

93.14

(*Annals of Philosophy*, xv. 148.)

This historical sketch has already extended to so great a length that I find it impossible to enter into any details respecting the geological facts and discussions which have been laid before the public since last year. I am not aware of any new geological fact of any great importance. But a good many papers have been published containing interesting descriptions of different parts of Great Britain and other parts of the world. This is of great importance towards the progress of the science. Geologists will have it in their power to speculate with some chance of success when they have made themselves acquainted with all the phenomena, and when every portion of the surface of our globe has been accurately examined and described.

XV. MANUFACTURES.

1. *New Method of preparing Pharmaceutical Extracts.*—This method, for which Mr. Barry has taken out a patent, consists in evaporating the extracts to the requisite consistency in vacuo. The advantages are, that the air is excluded, and the heat never raised higher than 90° or 100°. The extracts in consequence are much stronger, and quite different in their characters from those prepared in the usual way. The vacuum is produced by means of steam. The method, which has been long known, will be sufficiently understood by inspecting the sketch of the apparatus in the *Annals of Philosophy*, xiv. 390.

I have no doubt that this method is a good one. I have myself been in the habit for some years of drying vegetable extracts by placing them in the exhausted receiver of an air-pump along with a quantity of sulphuric acid. This method is, perhaps, still better than that of Mr. Barry. I do not know whether it might not be applied to the laboratory of an apothecary.

2. *New Method of Dyeing Yellow.*—Braconnot has given the following formula for dyeing with sulphuret of arsenic, which he affirms to form a most beautiful and indelible yellow colour.

Mix together one part of sulphur, two parts white oxide of arsenic, and five parts of potash of commerce, and expose the mixture in a crucible to a heat approaching to redness. Dissolve this mixture in water, separating the sediment which consists of metallic arsenic, and a chocolate coloured powder suspected to be a subsulphuret of arsenic. Pour sulphuric acid into the solution, a fine yellow precipitate falls, consisting of sulphuret of arsenic. Wash this powder sufficiently, and then dissolve it in liquid ammonia, and add an excess of ammonia sufficient to render the liquid colourless. Put the cloth to be dyed in this liquid, and allow it to remain till it has become fully impregnated. Then take it out, and expose it equally to the atmosphere. As the ammonia evaporates, the cloth assumes a fine yellow colour, varying in intensity according to the proportion of sulphuret of arsenic in the liquid employed. (*Ann. de Chim. et de Phys.* xii. 398.)

I think that this yellow dye will not be able to withstand soap. The alkalies would undoubtedly destroy it. Its use, therefore, will be limited to those stuffs which are not to require washing. Now I rather suspect that dyers are already possessed of very fine yellow colours; but certainly of none finer than the one proposed by Braconnot.

II. *Comparative Anatomy and Zoology.*

By a Friend of the Editor.

COMPARATIVE ANATOMY.

A WORK on this subject, entitled "Philosophie Anatomique; par M. Le Chevalier Geoffroy St. Hilaire," appeared towards the end of the last year, in which the author attempts to prove the truth of a theory advanced by him about 17 years since; namely, that all vertebrate animals were formed, not only on the same general plan, but that their skeletons consist of the same number of parts. To be brief; that those bones which in fishes and other animals at the lower end of the vertebra, remain separate in the adult animal, may likewise be found in a separate state in the young, belonging to the upper end of the same type. We will now take a very general survey of the subject.

Geoffroy is of opinion that the bones of the ear (which, as every one knows, have been destroyed in both ears by disease, in the human subject without loss of hearing to the individual), are to be considered as but rudimentary in man, and other mammalia; in the same way that the mammæ are but rudimentary in the male sex; and that the said bones of the ear having reached their highest degree of development in fishes, are represented in

that class, by what are denominated the opercular bones; the opercular bone he calls *Stapes*; the subopercule, *Os lenticulare*; the interopercule, *Malleus*; and a bone placed beneath the subopercule, he names *Incus*.

The next subjects of comparison are the *Breast bone*, and the *Os hyoides*.

His descriptions of the *Sternum*, and his observations on it, as far as they relate to the mammalia and birds, and in part to the reptiles, are *sans critique*; while those on the sternum and os hyoides of fishes are irreconcilable.

Dr. Leach, who has considered the subject, is of opinion that the part named os hyoides of fishes, by Cuvier, in his *Règne Animal*, is perfectly analogous to the same part so named in mammalia and birds. Geoffroy, however, refers the *Apo-hyal* and *Cerato-hyal* of these animals to the sternum, merely from their being the bearers of those bones that support the branchiostegous membrane, which he compares to the sternal ribs. He at the same time calls two bones that are interposed between the basi-hyal and the apo-hyal, apo-hyal and cerato-hyal. We suspect what these bones represent; but until we find decided proof, think it better to be silent on the subject.

Dr. Leach is further of opinion that those bones in fishes, which are named by Geoffroy the anterior and posterior thyreal and arythyreal, which support the gill arches, are in fact sternal bones, and represent the hyposternal, entosternal, and posterior bones of the sternum. Dr. Leach has also very strong reasons to suspect, that the criceals of Geoffroy may turn out to be the zyphoids of the same part. He thinks too, that some bones in the vicinity of the sternum in ornithorhynchus are likewise misunderstood; Geoffroy's *Acromium*, he believes to be the true *Clavicles*; the *Clavicles* of Geoffroy to be the *Episternal*; and Geoffroy's *Episternal* to be the *Hyposternal*.

Dr. Leach is perfectly assured that the supposed exerted *Corocoid* of Geoffroy, in *Silurus*, is no more than the *thumb* of the pectoral fins in a high state of development; if this be not the case, he would ask; where then is the thumb, and how can the theory stand, if any part be actually wanting?

The *Scapula* of fishes Geoffroy has shown to be composed of two bones in adult fishes, as is the case in young mammalia. Dr. Leach has found the smaller of these bones, the *Omolite* of Geoffroy, to consist of two bones, in a carp of more than 20 years old, and has since ascertained the same to be the case in the very young foetal state of the human subject.

The book contains a vast number of new observations made generally with great accuracy, but for the most part the theory is vague and hasty. It has, however, opened a wide and a new field, and, if followed up, will ultimately lead to a more complete knowledge of true comparative anatomy; of which this is the first example that has hitherto appeared.

Dr. A. Jacob has given a paper to the Royal Society, on a

new membrane in the eye, which is published in the volume for the year 1819, p. 300.

Magendie still continues his zootomical inquiries with great zeal. He has lately given a note on the mesenteric nerves of the green woodpecker, *picus viridis*.—(Bull. des Sciences, 1819, p. 119.)

ZOOLOGY.

Type I, VERTEBROSA.

Class I, MAMMALIA.

In the *Journal de Physique*, Dr. Leach has pointed out the generic differences that exist between the *Black* and the *Red Orang-otans*. The first genus, *MIMETES*, Leach (*Chimpanse*), the nearest animal to man, has no intermaxillary bone; it has the last joint of the great toe perfect; and has the ligamentum suspensorium of the thigh bone. The type is *Simia Troglodytes*. The second genus *PITHECUS* (*Orang-otan*), has an intermaxillary bone, but wants the last joint of the great toe, as well as the suspensory ligament of the thigh bone. *Simia Satyrus*, of Linné, is the type of this genus. Specimens of both these genera have lately died in London, and are preserved in the Royal College of Surgeons.

Geoffroy St. Hilaire, who has written several dissertations on the classification of bats, has this year discovered a new genus, which he has named *Glossophaga*. It belongs to one of the groupes, having leaf-like processes above their nostrils. The tongue is long, slender, and capable of extension. Four incisors, two canines, and six grinders, in each jaw. He mentions four species. The type of the genus is *Vespertilio soracinus* of Pallas.

Rafinesque has this year commenced a work entitled “Annals of Nature,” in which he has described one new genus of bats named *Eptesicus*, characterized by four acute incisors in the upper jaw, which are placed in pairs, being separated by an interval and by a flat wart. The outer tooth on each side is largest, and is unequally biped. Six lower incisors, equal in size, set close together and truncated. Canine teeth long, sharp, and curved. Grinders unequally trifold. Nose simple. Ears distant, with auriculæ. Tail mucronate. Of this genus, he describes two species.

Of his old genus of bats, *ATELAPHA*, he has likewise described a new species.

Dr. Leach has sent two papers to the Linnean Society on seven new genera of bats; but as the volume is not yet printed, we must defer our notice of it till a future period.

In the *Annals of Nature*, Rafinesque has described a new species of *MEPHITIS*; a new *SPALAX*; two new species of *GERBILLUS*; one new *CRICETUS*; three new *LEMNI*; and one new *SCIURUS*.

Mr. Ord has established a new genus of the natural family

ANTILOPIDÆ, or Antilopes, having furcate horns, under the name ANTILOCAPRA, the type of which is the *Antilope Americana* of Blainville.

Desmarests, an ingenious French naturalist, has shown the opinion of Cuvier respecting the situation of Gmelin's *Sorex cristatus* to be erroneous. Cuvier referred it to the genus TALPA (*Mole*), from which it differs most essentially in having no canine teeth. The animal in question was referred to a new genus by Illiger, and was named by him CONDYLURA, to which the following character is given by Desmarests. Incisor teeth in the upper jaw, 6; in the lower, 4; canine teeth, none; grinders in the upper jaw, 14; in the lower jaw, 16.—(Journ. de Phys. ii. 89, p. 225.)

In the same volume, Blainville has divided the Mammalia unguiculata into two sections, which we have not yet found time to examine.

Class II, CETACEA.

Lacepède, from figures, has described some new species of Cetacea, which we think not worthy of being recorded.

Class III, MONOTREMATA.

Fresh evidence respecting the distinctive characters of this class has been obtained, and will be published in our next annual report.

Class IV, AVES.

In the Annals of Nature, No. I. Rafinesque has described four new species of birds: 1. *Milvus Leucomelas*; 2. *Ardea Phaïoma*; 3. *Charadrius viridis*; 4. *Hirundo phenicephala*.

Temminck's new edition of his Manuel d'Ornithologie is now printing in Paris.

Dr. Horsfeld's interesting paper on the birds of Java will most probably appear in the next volume of the Transactions of the Linnean Society.

Class V, REPTILIA.

Rafinesque has given in the Annals of Nature, No. I, descriptions of what he supposes may be three new species of CROTALUS, the generic name of which he changes to CROTALURUS, without assigning the least reason for this alteration.

Of Coluber, he likewise gives four new species, and one of Anguis.

The great *Sea Snake* of America has been discovered to be no more than a great *Tunny*; and its supposed young, forming the genus SCOLIOPHIS, of Rafinesque, has been proved to be nothing but a monster of the common ringed-snake of America (which is allied to our coluber natrix), having distorted vertebræ!

A new species of Scincus has been discovered to inhabit Maryland, by J. Gilliams.

Sir Everard Home has given another paper on the fossil

reptile found at Lime Regis, in Dorset, and which he formerly referred to the fishes (Phil. Trans. 1816, p. 319, 320), and afterwards compared with *Ornithorhynchus* (Phil. Trans. 1818, p. 24). Sir E. Home has here admitted the animal to be a true reptile (although he denies having given any previous opinion on the subject), but has given to it the extraordinary generic name *PROTEO-SAURUS*, although it had long before been denominated *ICHTHYOSAURUS*,* by C. Konig, Esq. of the British Museum, and the name had generally been admitted by British naturalists. It has in fact no essential affinity in any of its characters, with the animal which has been of late years improperly denominated *PROTEUS*, and which belongs to another and very different class of animals. *PROTEUS*, properly so called, is a genus of infusitory animals, discovered long before the animal so misnamed was known, and is figured in the *Encyclopedie Methodique*, Vers pl. I. f. 1. This animal is well named, as it continually changes its form. The misnamed *PROTEUS* is allied to the Linnean genus *Siren*, and holds with it and the *Axotle* of the South Americans, a place between the *AMPHIBIA* and the *FISHES*, breathing by means of gills, like the larvæ of the true *AMPHIBIA*, having its locomotive organs in the form of walking legs, and not like fins. The sack on each side of these animals is now well known not to be in any degree connected with respiration, and probably is destined to perform the function of the swimming bladder of fishes. We understand that Dr. Leach is engaged with investigating the anatomical structure of the genera *Siren*, *Axotle*, and the misnamed *Proteus*, with a view to point out the character of a distinct class of animals, which he has named *Sirenes*.

Class VI, AMPHIBIA.

In the Journal of the Academy of Natural Sciences of Philadelphia, we find a description of a new species of *Salamandra* (Salamander), discovered in the southern states of North America, by Mr. Gilliams.

In the Annals of Nature are described by Rafinesque three new species of *NECTURUS*, a genus differing from Triton of Duméril, by having teeth, and four toes on all the feet; its tail is compressed.

In the same number we find five new species of *TRITON* (*TRITURUS*, Rafinesque), described.

Class VII, PISCES.

Le Sueur has made us acquainted with several new American species of the family *SQUALIDÆ* (Sharks), in the Journal of the Philadelphian Academy of Natural Sciences; in which work he has also described a new *OSMERUS*, two new species of *COREGONUS* (umber); two new species of *CLUPEA* (herring); four

* A name we are happy to find adopted by the just naturalists of the French Museum, and attached to their specimens in the *Jardin des Plantes*.

of the true genus *Esox* (pike); six species of *PIMELODES*; one of *ACIPENSER* (sturgeon); and two of *MOLVA*.

In the *Memoires du Muséum*, Cuvier has described six new species of *SALMONIDÆ*, which he refers to three new genera: 1. *MYLETES*, having prismatic teeth. This genus contains six species, which belong to two divisions; the first with their belly acute; the second with that part rounded. 2. *CHALCÆUS*, having teeth short and round. 3. *TETRAGNOPTERUS*, a genus before given by Artidi, in Seba's *Thesaurus*.

Rafinesque has likewise given, in the *Journal of the Academy of Philadelphia*, a new genus of the family *SPARIDÆ*, named *POMOXIS*; another, *SARCHIRUS*, belonging to the family *ESOCIDÆ*; and *EXOGLOSSUM*, referable to the *CYPRINIDÆ*.

A new genus of the family *CYPRINIDÆ*, having appendages in the mouth and abdominal fins, has been observed by Rafinesque, who named it *HEMIPLUS*, and has described one species.

In the same number of the *Annals of Nature*, we find described two new species of *CYPRINUS*, and one of *SALMO*.

Type II, ANNULOSA.

Class I, VERMES.

Four new species of *HIRUDO* are published by Rafinesque in his *Annals of Nature*, No. I.

Class II, CHETOPODA.

Blainville has written a very excellent paper on the classification of these animals, in the *Bulletin des Sciences*; and has named the class *CHETOPODA* which he formerly designated *SETOPODA*.

We are happy to have it in our power to state that the celebrated Savigny is soon about to print a work on this class of animals.

Class III, MYRIAPODA.

Some new genera and species of this class are described by Rafinesque in No. I. of his *Annals of Nature*.

Of the first order, we find a genus *SELISTA*, allied to the *LITHOBUIS*, of which he has found one species. Of *CRYPTOMERA* another new genus, allied to *POLYDESMUS*; two species are given. Of *STENOMERA*, a new genus allied to *CRYPTOMERA*, one species is described. Of *MYCOTHERES*, the author says, there are several species, three of which only are described.

Of the second order we find four new genera, viz. 1. *PLEUROLOMA*, allied to *GLOMERIS*. 2. *NARCEUS*, allied to *JULUS*. 3. *RHEXENOR*. 4. *ABACION*.

Class IV, INSECTA.

A very learned work, entitled "*Horæ Entomologicæ*," Part I, on the classification and distribution of the insects composing

the old genera *scarabæus* and *lucanus* of Linné, into natural families, by W. Mac Leay, Esq. has lately been published, and reflects honour on its author, who has treated the subject with philosophical rigour. His first chapter contains admirable observations on the characters which should be employed to distinguish the genera and families, with remarks on the abuse of several of them, by which, genera that are nearly allied, have been placed at a distance from each other. He has also shown that generic distinctions have been founded on the supposed want of parts which have in fact existed, although in a very low state of development. Chapter 2 treats of the actual state of our knowledge with regard to the classification of the Linnean *scarabæi*. In the third, the author discusses the difficult subject of nomenclature. The following chapter contains observations on the affinities which the Linnean *scarabæi* bear to each other. The fifth contains remarks on the Linnean genera *lucanus* and *hister*. The sixth, general remarks on the geographical distribution of the *petalocera*. The seventh and eighth, on the five families of *petalocera* that feed on putrid or decomposed, and the five families that feed on green or living vegetable matter. The last chapter contains concluding remarks; and the appendix treats of the genera of the *rectocera*, or of the old genera *hister* and *lucanus*; and of the *petalocera* or *scarabæus* of Linné.

We feel extremely sorry that we have not time to give minute details of this admirable work, to which we must, therefore, refer our readers.

Mr. Kirby has published two papers in the Transactions of the Linnean Society; the one entitled a "Century of Insects," the other "Description of several new Insects discovered in New Holland by R. Brown, Esq." in which several new genera and species are described.

Class V, CRUSTACEA.

In the Journal of the Academy of Natural Sciences of Philadelphia, the crustacea of the United States have been described by T. Say, Esq. in a manner that reflects great credit on this zealous zoologist, who has established several genera, and discovered several new species of recently discovered European genera.

In the "Nouveau Dictionnaire des Sciences Naturelles," vols. xii. and xiv. are two papers by Dr. Leach, on the family *СΥΜΟΤΗΟΑΔΕ*, and on the *ΕΝΤΟΜΟΣΤΡΑCΑ*, in which are established 18 new genera.

In the Annals of Nature, Rafinesque has given three new genera of those Malacostraca, which have sessile eyes. The first two belong to that great division, having their bodies laterally compressed, both of which are inhabitants of fresh-water, and are named by him *SPERCHIUS*, and *LEPLURUS*. The other genus *LIRCEUS* is referable to the second grand division of the same great group, whose bodies are depressed from above.

Type III, MOLLUSCA.

Class I, GASTEROPODA.

A most splendid work in folio, entitled, "Histoire Naturelle, générale et particulière des Mollusques Terrestres et Fluviales; par le Baron de Férussac," was published in numbers, seven of which have reached England. On the sixth of March last, a report was given, by the Chevalier Cuvier, on the merits of the first six livraisons, to the Royal Academy of Sciences of France, which must have been highly flattering to the ingenious and learned author. The general intention of this magnificent undertaking, which deserves the support of every zoologist and geologist, is to publish in systematic order, descriptions, with most correct and beautiful figures, of all the land and freshwater species of shells, both fossil and recent, with their anatomy, economy, and physiology. Each number contains six coloured plates, with their descriptions, price in colours, 1*l.* 5*s.*; plain, 15*s.* The first six numbers contain an account of all the species of the natural family Limacidae (*slugs*), in the following order: 1. History of the family of slugs. 2. General observations on the structure and faculties of the family, with an account of the organisation of the foot, and of the difference that exists in the respiratory cavity of such as live in water, and those which live on land. 3. The habitation of those having no opercula. 4. The division of the order Pulmones into two families. 5. Historical account of the family Limacidae, from the most ancient period of history. 6. General observations on the structure and faculties of the Limacidae. 7. On the use of the Limacidae in the economy of nature. 8. Synoptic table of the genera which compose the family, in which four new genera are established; namely, 1. LIMACELLUS, of Blainville. 2. ARION. 3. VERONICELLUS, of Blainville. 4. PLECTROPHORUS. The seventh number contains the commencement of the family Helicidae. A most favourable report has likewise been made to the Royal Academy of Fine Arts by Messieurs Van Spaendonck, Desnoyers, Bervie, and Castellan. The work will be comprised in 240 plates. From the vast number of figures, we do not hesitate to pronounce it the cheapest work that has ever appeared on any branch of zoology.

In No. I. of the Annals of Nature, Rafinesque has established a new genus named PHILOMICUS, differing from LIMAX, in having no visible mantle. The superior tentacula are terminal and clavate; the lower ones lateral and elongated. He has described four species, all of which feed on fungi.

In the same place we find a second genus, differing likewise from LIMAX in having no visible mantle. The four tentacula are cylindrical, situated in one parallel line; the inferior ones being near to each other. Two species only are known.

Lastly, he has described a new LIMAX, which he suspects may form a new genus; and a new genus of HELICIDÆ, named

HEMILOMA, whose shell is ovate, elevated, and smooth. Aperture, obliquely-elliptic, with the interior peritreme elevated, and a little twisted. Umbilicus very small.

The last number of the Journal de Physique contains a concordance of the land and freshwater mollusca of Great Britain, arranged systematically by Baron de Férrussac.

De France has established as a distinct genus the *Capulus Cornucopia* of Lamarck, which he denominates HIPPONIX, from the form of its muscular impression, and describes four species, all of which are fossil.—(Bull. des Sciences, 1819, p. 9.)

Say, with his usual zeal, has given some papers on this class to the Academy of Philadelphia, and has discovered some new genera. 1. POLYGYRA, of the family HELICIDÆ. 2. OLYGYRA, belonging probably to the family AURICULADÆ.

Class II, CEPHALOPODA.

Lichtenstein has established a new genus of this class, under the name ONYCHOTEUTHIS, which resembles the genus LOLIGO, in general aspect, but differs in having all the brachial suckers armed with hooks. This genus probably includes Dr. Leach's second division of LOLIGO (see Zool. Misc. iii. p. 141); the third division of which now constitutes the genus ANCI-STROCHIRUS of Dr. Leach, characterized by having only the suckers of the distal extremities of the supplementary arms furnished with hooks.

Blainville has written a very long and interesting dissertation on the animals found in the shells of the genus ARGONAUTA, or *Paper Nautilus*, with a view to prove them to be mere parasites; an opinion long entertained by Sir J. Banks and other naturalists, but denied by Cuvier. A new species of this genus is described by Mr. Say, in a letter to Dr. Leach, which is inserted in the last volume of the Phil. Trans.; and Dr. Leach has lately obtained a second species out of the *Argonauta Argo*. As we are, therefore, now acquainted with five species of OCYTHOE (or of the supposed parasite); and as but three species of *Argonauta* are known, the question respecting the parasitical nature of the OCYTHOE seems to be set at rest.—(See Phil. Trans. 1811, p. 107.)

Class III, ACEPHALA.

Bojanus has given us an anatomical dissertation on the ANODON CYGNEUS.—(Journ. de Phys. tom. lxxxix. p. 108.)

ALASMODONTA* (Say, Journ. Acad. Phil. i. 459), a new genus of the family UNIONIDÆ, having its shell elongate; its hinge with a strong tooth in each valve. Situation between UNIO and ANODON (misnamed ANODONTA).

Dr. Leach's opinion as to the mode by which the acephalous animals close their shells almost entirely, without muscular action, by means of abductive elastic ligaments, is now

* It should be *Alasmodon*.

generally admitted. All the acephala, if we except the old genera TEREDO and PHOLAS, are furnished at their hinge with an elastic ligament that opens their shells. They have one or two muscles and the same number of elastic ligaments to act against this opening power; and the force of the elastic abductive ligament is sufficient to act so as nearly to close the shells, the valves being sufficiently opened to admit only of that quantity of water necessary to convey food, and to serve the purposes of respiration.—(See Bull. des Sciences, 1818, p. 14.) The valves can readily be closed completely at the will of the animal by a very slight action of their strong abductive muscles.

Classes of uncertain Situation.

Class, ENTOZOA.

An admirable work on this subject, from the long experience of Rodolphi, was published towards the end of 1819, entitled, “Entozoorum Synopsis,” in which all the known intestinal worms are systematically arranged and described; with various copious indices to facilitate the investigation of the species. De Blainville has given an admirable analysis of this work in the March number of the Journal de Physique.

III. *Physiology.*

By a Friend.

IN the course of the last year this important branch of knowledge has received but few additions. Some of these have been already noticed under other heads. A few others remain to be briefly related here.

I. Sanguification and the Blood in General.—The observations of Mr. Bauer on this subject, as related by Sir E. Home, are interesting, if they can be relied on. Mr. B. thinks he has ascertained that the coagulable lymph or fibrin exists when circulating in the living body in a state of perfect solution in the serum, and that what is termed the spontaneous coagulation of the blood, consists in the separation of this lymph or fibrin in the form of an infinite number of globules of about $\frac{1}{8000}$ th of an inch in diameter, which, by adhering together, constitute the solid mass termed *fibrin* obtained from coagulated blood, &c. These globules have been termed by Sir E. Home *lymph globules*, to distinguish them from the red globules.

Mr. B. attempted to trace the origin of these lymph globules, and also the red globules found in the blood; and he states that they are first to be seen in the mucus of the pyloric portion of the stomach and of the duodenum. On examining the contents

of the mesenteric glands, he found that the white colour of the fluid issuing from them depends upon an infinite number of white globules floating in a clear perfectly colourless fluid, in the same manner as the red globules do in the serum of the blood. About eight-tenths of these globules varied in size from the smallest speck to the size of the lymph globules, about one-tenth were of the size of the red globules deprived of the colouring matter, and about one-twentieth were of the size of the red globules enclosed in their colouring matter. When this fluid was left a few minutes in the glass, not only many new globules were formed, but the original small ones visibly increased in the field of the microscope, not by several globules uniting, but by accession of substance, and several were watched till they acquired the size of blood globules enclosed in their colouring matter. In that state they became more opaque and white. The addition of water prevented the further formation of globules, and acted upon those already formed in the same manner as it does on the red globules; viz. by dissolving their colouring matter, and reducing their size. Mr. B. concluded from these observations, that the blood globules acquire their full size in the mesenteric glands, and afterwards assume their red colour on exposure to the air in the lungs. (Phil. Trans. 1820, p. 1.)

Menstrual Blood, Fatal Blood, &c.—Dr. F. Lavagna has been led to conclude, from some experiments on the menstrual blood, that it differs from pure blood only in the want of fibrin. He also found that blood collected from the vessels of the funis umbilicalis relating immediately to the placenta, formed a tenacious coagulum, and contained a considerable proportion of fibrin, though it was rather more soft and gelatinous than that collected from the blood of an adult in the state of health; and that on the contrary the blood contained in the vessels of the funis relating to the fœtus, hardly coagulated at all, and seemed to contain a very small proportion of fibrin. This physiologist is induced to conclude from these observations, that the uterus in a gravid state acquires the power of furnishing blood provided with fibrin, which is subtracted from it by the embryo, to be appropriated to its own use for the means of its growth. (See *Esperienze sopra il Sangue Menstruo: de Francesco Lavagna guiniore, M.D. &c. in the Annali Universali di Medicina di Milano, No. 17.*)

Respiration and Circulation of the Blood.—Dr. Carson has lately made some important experiments on the elasticity of the lungs in different animals, and he has found by the application of a simple apparatus that in oxen, and animals of their size, it is more than equal in power to the weight of a column of water a foot and a half high. In calves, sheep, and large dogs, it is balanced by a column of water varying in height from one foot to a foot and a half; and in rabbits and cats by a column of water varying from six to ten inches. To this elasticity of the lungs alternating with the irritability of the diaphragm, Dr. C.

ascribes respiration, or the faculty of breathing; the capacity of the chest being by their means successively enlarged and diminished, and thus air alternately expelled and inhaled. He thinks also with great probability that the movements of the heart and the circulation of the blood are powerfully influenced by the same resiliency. (Phil. Trans. 1820, p. 29.)

The following works have been lately published on this subject, to which, from their nature, we must content ourselves with referring our readers:

An Experimental Inquiry into the Nature, Causes, and Variations, of the arterial Pulse. By Caleb Hillier Parry, M.D. F.R.S.

Additional Experiments on the Arteries of warm blooded Animals, &c. By Charles Henry Parry, M.D. F.R.S.

An Essay on the Forces which circulate the Blood, being an Examination of the Difference of the Motions of Fluids in living and dead Vessels. By Charles Bell, F.R.S.E. Surgeon to the Middlesex Hospital, &c.

II. *Secretion and Secreted Products.*—As every part of animal bodies is formed from the blood, they may be all considered as *secreted* products, though the term is usually limited to those substances formed from the blood by a peculiar glandular apparatus. We shall, however, for the sake of convenience, take the word in its general signification.

The Teeth.—We have been informed that an eminent French physiologist, M. Blainville, has lately advanced the opinion that the teeth are to be considered as hairs. A similar opinion was advanced in 1811 by Dr. Prout, who at that time drew up the sketch of a paper, the object of which was to prove that the teeth are to be considered as appendages to the integuments, and to be classed with horns, nails, &c. This opinion was principally founded upon extensive anatomical inquiries, showing the analogy between the formation of the teeth, and horns, feathers, &c. and partly also upon physiological and pathological reasonings. The paper was never published, owing to reasons which need not be mentioned, but the opinion was stated to many of the author's friends at the time; and he intends at some future opportunity to lay the subject before the public in an extended form.

III. *Nervous System, and its Functions.*—It would be well for physiology if its cultivators would leave for a while this abstruse and difficult subject, and turn their attention to something more within their power; that is to say, if they would make themselves better acquainted with the ultimate chemical and mechanical constitution of organic bodies; for till this is known, it is evident we can hope for very little real progress in physiology. We may here allude to the controversy that has been lately carried on in the medical journals between Dr. Wilson Philip and some other gentlemen on nervous action, &c. but we do not think it necessary to enter upon the subject. Those who are interested in the dispute may consult the original work of Dr. Philip, entitled “An Inquiry into the Laws of Vital Functions,”

and the last two volumes of the Medical and Physical Journal, and of the London Medical Repository.

Of the Senses. New Theory of Vision.—Dr. Reade has published a paper, the object of which is to show, that a minute inverted image of external objects is not formed on the retina of the eye, as commonly maintained, but a magnified erect image. (See *Annals of Philosophy*, vol. xv. p. 260.)

IV. *Generation.*—The difficulties attending the investigation of the generative function have caused differences of opinion among physiologists respecting many important circumstances connected with it; some, for example, maintaining that corpora lutea are the result of impregnation, and that the absolute contact of the semen masculinum with the ovum is not necessary to conception; others, the reverse. We believe, however, that for some time the most eminent physiologists have agreed on these two important points; viz. that in the mammalia, as in the bird tribe, ova may be formed and even expelled, independently of the male; and that the actual contact of the male semen with the ovum is necessary to impregnation. Dr. Blundell has lately made some experiments on this subject, which, though they possess no novelty, confirm both these points. These experiments were made upon rabbits, and consisted chiefly in cutting off the communication of the male semen from one or both of the ovaries by obliterating the communications between the vagina and uteri. The results were in these cases that “young animals were invariably found in the sound womb, but not in the interrupted.” In other experiments, the vagina was obliterated. In this case no conception took place; but it was found that by connexion with the male, corpora lutea were generated, and the uteri developed, and also rendered probable, that the “rudiments were transferred to the uterine cavity by the play of the fallopian tubes.” (*Med. Chirurg. Trans.* vol. x. p. 245.)

In a paper read before the Medicinisch-Chirurgische Gesellschaft, of Berlin, by Dr. Stein, a case was related in which on performing the operation for imperforate vagina no uterus could be found. This occurred in a young married lady 24 years of age, of the most perfect female form, slender and delicate, with full breasts, a fair complexion, and an animated countenance expressive of the finest sensibility. She had never menstruated, although the orgasm generally accompanying the appearance of the menses, with some distress about the pubes, recurred at the ordinary periods. From the peculiarity of her situation, she was induced to submit to an examination, when a firm membrane completely closing the vagina was found. This being cut through, the absence of the uterus was discovered; its place being supplied by a soft mass of cellular tissue. The wound united, and fortunately no serious accident occurred. From this and other similar cases related by the author, he infers that it is the ovaria and not the uterus in which the menstrual orgasm originates, and which, by their influence, give to the woman her characteristics in respect to form and manners.

IV. Botany.

By Samuel Frederick Gray, Esq.

1. SYSTEMATIC BOTANY.

Ranunculaceæ.—A very able monograph of the genus *Pæonia* has been written by the late Mr. Anderson. Linnæus had reduced all the pæonies to one species; but Mr. Sabine having collected upwards of seventy plants, under different names, in consequence of this opportunity of comparing the species, the monograph was drawn up. All the species are confined to the colder climates of the old Continent, and they are now carried to the number of 13, of which one is shrubby, and forms the pride and glory of the Chinese gardens; who boast of having rendered it a shrub by their skill in horticulture, it having been originally, like all the rest, herbaceous: no wild specimens have yet been found of this species. Of the 13 species here enumerated, two of them, *P. arietina* and *P. mollis*, are undoubtedly new. Mr. Sabine, who publishes the paper of his deceased friend, compares the species contained in this monograph with those enumerated by De Candolle, in the first volume of his “*Systema Naturale Regni Vegetabilis*,” and shows the coincidence between the two lists. He is inclined to reject the two additional species mentioned by M. De Candolle, *P. Tatarica*, for which he seems to have had no other authority than Miller, and Miller’s plant is the *P. paradoxa* of Anderson, and the *P. peragyna* of De Candolle; and *P. laciniata*, taken from Willdenow, which is probably a strong growing plant of *P. tenuifolia*. *Trans. Lin. Soc.* xii.

The organs of growth in *myosurus minimus* have, according to Cassini, been mistaken. The plant consisting of a tuft of fibrous roots, buried in the earth, from which springs a white, cylindrical, hard caudex, hitherto considered as part of the root, but which really participates of the nature of both root and stem, and serves to raise up the tuft of leaves and peduncles to the surface of the water. *Bull. Philom.* Rafinesque has also described a new species of *myosurus*; under the name of *M. Shortii*, in Silliman’s Journal.

Cruciferæ.—A new genus has been lately established of these plants, whose general resemblance is so striking, namely, *Stevensia*. Its characters are—Calyx rather spreading, 2-bagged at the base; stamens simple, lanceolato-subulate; siliqua sessile, crowned by the style, compressed, linear, elliptic, edge sinuated, few-seeded, opening by a straight valve; seeds immarginate; cotyledons accumbent. There is only one species, *S. alyssoides*.

Passifloreæ.—A new species of *passiflora*, *P. racemosa*, is described and figured by Professor Brotero. *Linn. Tr.* xii.

Sauvagesicæ, Violaceæ, Frankenicæ.—A. de Saint Hilaire has published observations on these families in *Mem. Mus.* iii.

Sapindi.—Five species of melicocca have been described and figured by A. L. De Jussieu in the *Mem. Mus.* iii.

Terebintaceæ.—Heterodendrum, a new genus, is constituted by Desfontaines; who has described and figured *H. oleafolium* in *Mem. Mus.*

Leguminosæ.—Desfontaines has described and figured three species, all belonging to new genera, constituted by him, viz. *mezoneoron glabrum*, *heterostemon mimosoides*, and *ledocarpon Chiloense.* *Mem. Mus.*

Rosaceæ.—The *corchorus Japonicus* of Thunberg, called by Linnæus, *rubus Japonicus*, is made by De Candolle into a genus of rosaceæ, under the name of *Kerria*; so called, from Kerr, the gardener, who brought it and many other plants from China. The *tigarca tridentata* of Pursh is also removed by De Candolle to the rosaceæ, and made into a genus by the name of *Purshia*. Both these genera are to be placed between the tribes of *spireæ* and *dryadæ.* *Linn. Trans.* xii.

Joseph Woods, Esq. has examined, with great attention, our British roses, and given a detailed description of them. He enumerates no less than 26 species; of which seven are entirely new, namely, *R. Doniana*, *Sabini*, *heterophylla*, *pulchella*, *nuda*, *bractescens*, *surculosa*. In the preliminary part of his paper, he shows the necessity of attending to the distinction of the arma of roses, which he divides into *aculei*, broad at bottom, and generally hooked; *setæ*, always straight, and tipped with a gland; glands which are almost always pedicelled, and, indeed, in some species, as in *R. Eglanteria*, these three kinds of arma pass, by almost insensible gradations, into one another; weak white hairs, as in *R. Borreri*: chaffs, as in the axillæ of the leaflets of *R. spinosissima*; and lastly, pubescence. The appropriate name for the *hip* of a rose has not been determined by botanists. Linnæus, Smith, and Willdenow, call it the germen, although the latter censures Linnæus for adopting this name. Gærtner and Jussieu describe the genus as having a calyx *urceolaris*, and some other French botanists call it the tube of the calyx. Mr. Woods calls it the receptacle, and considers the juicy part of a strawberry as being the inner vessels of this receptacle diluted into a spongy body. The mode of growth of the rose is peculiar: the seedling plants are feeble, and produce only a few flowers, but if they are cut down, or some injury done to the original growth, a strong shoot proceeds from the root or the base of the stem, which rises higher than the original plant, is armed with abundance of prickles, and flowers freely at first, but grows weaker as it branches, and requires fresh setting to restore its vigour. *Linn. Trans.* xii.

An account of 50 species of Swiss roses is published by Seringe in the first volume of his *Mélanges Botaniques*. And Mr. Neil, in the *Edinburgh Philosophical Journal*, has described the

Argyleshire rose as distinct from *Rosa arvensis*. The characters as drawn up by Mr. D. Don are *Rosa arvensis*; shoots very long, procumbent; leaflets ovate, unequally serrated, hairy on the under side only; fruit globular; peduncles nearly naked: *Rosa capreolata*, the *Argyleshire rose*; shoots flagelliform, procumbent; leaflets elliptic, unequally serrated, quite bald on both sides; fruit ovate, contracted at the neck; peduncles glandular.

Hygrobeæ.—Goldbach, in a *Spicilegium Floræ Mosquensis*, corrects the specific differences of the two species of callitriche 1. *C. pallens*; upper leaves aggregated, seeds scarcely margined on the back; *C. verna*, Linn. 2. *C. virens*; leaves uniform, equidistant; seeds margined, back membranaceous; *C. autumnalis*, Linn. *Mem. Mosq.*

Ficoideæ.—A very handsome species of cactus, called, from its beauty, *C. speciosissimus*, which flowered in the *Jardin du Roi*, is described and figured by M. Desfontaines. *Mem. Mus.* iii.

Saxifrageæ.—Mr. R. Brown has given a notice of three new species of saxifraga in the Appendix to Captain Ross's Narrative of his Northern Expedition.

Umbelliferæ.—Sprengel has examined afresh several of these plants, whose family character is so strongly marked, that their specific differences are peculiarly difficult to determine. He thinks the true character of the physospermum of Cusson should be seeds ovate, obtusely angular, included in a striated loose utricle; involucre, both general and partial, many-leaved; the type of the genus being the *P. commutatum* of Sprengel; ligusticum Cornubiense Linn. He forms a new genus, under the name of *Dondia*, and characterizes it thus:—Fruit oblong, narrowed at each end, furnished with four folds, and as many arches; involucre single, 5-leaved, leaflike; flowers capitate, female and hermaphrodite sessile, male peduncled, with 5-cut persisting calyces: the type being *D. epipactis* Spr. *astrantia epipactis* Willd. He has also laboured upon that confused genus, *cachrys*, and enumerates five species of it. His character of *Smyrnum* is—Fruit ovate-subrotund, three-ribbed; albumen including, involute; involucre scarcely any: and he considers *S. olusatra* as the type. To this information he has added some remarks on the genus *bupleurum*. *Mem. Mosq.*

Sir James E. Smith, the President of the Linnæan Society of London, has amended the specific differences of two species of *tordylium*, and has given as the characters of *T. officinale*; Involucells as long as the flowers, leaflets ovate, cut, crenate; petals radiant, double, unequally 2-lobed: and those of *T. apulum*; Involucells shorter than the flowers, leaflets jagged, the upper ones narrower; petals radiant, solitary, unequally 2-lobed. *Linn. Trans.*

Rubiaceæ.—A new genus, *ancylanthos*, is established, and one species of it, *A. rubiginosa*, is described and figured by Desfontaines. *Mem. Mus.* iiiii.

Boopideæ.—Mr. R. Brown has formed a new natural family,

under the name of Calycereæ (or Boopideæ, as it has been since called by Cassini), intermediate between the compositæ and dipsaceæ; differing from the former by the pendulous ovulum and albumen enclosing the embryo, the radicle pointing to the apex of the pericarpium, the accretion of the base of the style with the tube of the corolla, the absence of the epigynous disk or nectarium, and the longitudinal division of each cell of the anthers by a receptaculum pollinis; and from the latter by the vascular structure and valvular æstivation of the corolla, the æstivation, insertion, and connexion of the antheræ, in the absence of the partial involucre, and in having alternate leaves. *Linn. Trans.*

Compositæ.—Mr. R. Brown has inserted in the *Linn. Tr.* xii. a number of observations on these plants, to which he has appended several original observations upon various other plants: in which he shows, 1. The æstivation of the compositæ is valvular, except in the genera *chuiraga* and *corymbium*, in which it is induplicate. 2. The fasciculi of vessels, or nerves, which at their origin are generally equal in number to the divisions of the corolla, instead of being placed opposite to these divisions, and passing through their axes as in other plants, alternate with them; each of the vessels at the top of the tube dividing into two equal branches, running parallel to and near the margin of the corresponding lacinia, within whose apices they unite. In several genera other vessels occur alternating with the primary, and occupying the axes of the lacinia: in some cases these secondary vessels are most distinct in the lacinia, and become fainter as they descend the tube, so that they might be regarded as recurrent, originating from the united apices of the primary branches, only that in other cases they are equally distinct at the bottom of the tube. 3. The pollen of the *cichoraceæ* is a regular icosahedron, except that of *geropogon glabrum*, in which it is a regular dodecahedron; these angular figures are not observed in any other family of plants; the pollen of the *corymbiferae* and *carduaceæ* is spherical or oval. 4. The branches of the style and stigmata are anterior and posterior. 5. The ovarium contains two slender cords originating from opposite points of the base of the ovulum, or of its footstalk, which run up, and are more or less connected with the lateral parietes of the ovarium, until they unite at the top of its cavity, immediately under the style, between which and the ovulum a connexion is thus formed. Now as the pistil, or female organ of all phenogamous plants, seems formed on the plan of a circular series of polyspermous legumens, or folliculi, whose seeds are disposed in a double series, the legumens corresponding in number to that of the parts of the calyx or corolla; of course the ovary of the compositæ is composed of two confluent ovaria. 6. In a simple spike the expansion of the flowers is generally ascendent, that is, the lower open first; but in a compound spike, the expansion of the partial spikes is generally ascendent, and that of the

spike itself descendent; that is, the terminal spike is first expanded, and the lower afterwards. The head of a compound flower may be considered as a depressed spike, and when the head is simple, the flowers expand from the circumference to the centre; but if the heads are placed in a corymbus, then the expansion of the heads, in respect to each other, proceeds from the centre to the circumference. An attention to this observation will show the real structure and proper appellation of the parts, in cases where these points may be doubtful, as in *Lagasca* and *Cæsulia*. This order of expansion of the florets is also of use in determining the nature of some of the grasses; and is generally accompanied with greater perfection of the parts, and apparently a greater power of resisting the ordinary causes of abortion or obliteration. Indeed the occasional reduction of any of the parts of a flower is indicated by the expansion. The earlier expansion denoting greater perfection, and of course less liability to reduction. And as wherever a separation of the sexes takes place, the female flower may be assumed as the most perfect, so in spikes and other compound inflorescences, the females are most usually found in the parts that expand first. Of which *Euphorbia* may be adduced as an example, although this genus was placed by Linnæus among the dodecandrous hermaphrodites, yet it is in reality composed of several monandrous male flowers surrounding a single female. The fasciculus of euphorbia is capable of being considered either as a simple depressed spike, or as a compound spike, in which the central female flower is the representative of the terminal spike, or as consisting of one or more verticilli with a single flower in the centre. But although in an assemblage of flowers priority of expansion generally indicates a greater degree of perfection, yet in a hermaphrodite flower the development of the stamina usually precedes that of the pistilla: the most remarkable exception occurring in some species of *plantago*, where the stigmata are fully developed, and often even withered, before the anthers burst.

Mr. Brown then offers some remarks on certain genera of *compositæ*, which either occur under different names in late systematic works, or whose structure and limits seem to be imperfectly understood, viz. *Soliva*, *Grindella*, *Tridax*, *Angianthus*, *Meyera*, *Melampodiun*, *Craspedia*, *Calca* (to which he pays particular attention), *Isocarpa*, *Salmea*, *Baccharis*, *Brachylena*, *Melananthera*, *Neurolæna*, *Antennaria*, *Ozothamnus*, and *Cassinia*.

The 4th volume of Kunth's *Description of the new Genera and Species of Plants observed by Humboldt and Bonpland*, is entirely devoted to this family, which he divides into six sections: 1. *chichoracæ*; 2. *carduacæ*; 3. *eupatoreæ*; 4. *jacobææ*; 5. *heliantheæ*; and, 6. *anthemidææ*. He describes four genera as new, but they had been already constituted by Cassini in the *Bull. de la Soc. Philom.*

Cassini has also constituted several new genera in this numerous family of plants, namely, *fornicium*; *facelis*, whose type is the *gnaphalium retusum* of Lamarck; *enalcida*, which differs from *diglossum* and *tagetes* by the pappus and corona; and *endoleuca*. *Bull. Philom.* And he has also published his remarks on the fourth volume of Kunth's work above-mentioned.

Desfontaines has described a new genus, *Gymnarhena*, and figured one species, *G. micrantha*. *Mem. Mus.*

The president of Schlechtendal has also described a new genus, *Lasiocephalus*, of which he gives the following essential character: Receptacle naked; pappus pilose, sessile; corolla tubulose; calyx imbricate, with a double series of leaflets. The genus contains only two South American shrubs. *Berl. Mag. Nat. Fr.* 1818.

Brunoniæ.—R. Brown has constituted this family, as intermediate between *compositæ* and *goodenoviæ*: differing from the former in having the ovarium free or above; in the want of a glandular disk; in the immediately hypogynous insertion of the filaments; in the indusium of the stigma; and in the vascular structure of the corolla, whose tube has only five nerves, continued through the axes of the lacinia, either terminating simply, or dividing at top into two recurrent branches forming lateral nerves, which hardly reach to the bottom of the lacinia: and from *goodenoviæ* in having both calyx and corolla distinct from the ovary; in the disposition of the vessels of the corolla; in the filaments being jointed at top; in the seed being without albumen; and in its remarkable inflorescence. *Linn. Trans.* xii.

Rhodoraceæ.—O. Swarz has amended the character of the *Fischeria* thus: Calyx 5-parted; corolla 5-petaled; capsule 3-celled, 3-valved, opening at the top, many-seeded. The genus should, he thinks, be placed between *ledum* and *rhododendrum*, and *F. buxifolia*, the *ledum thymifolium* Pers. Syn. considered as the type. *Mem. Mosq.*

Myrsinæ.—A. de St. Hilaire has given to the public observations on the plants of this family in the *Mem. Mus.* iii.

Apocineæ.—A new species of *asclepias*, *A. lanceolata*, has been described by E. Ives, in Silliman's Journal.

Professor Brotero, of Coimbra, has given a description of a new and very curious genus of plants—*Araujia*; remarkable like the orchides for its pollen, which is a single compact mass, exerted from each cell of its five anthers, the extremities of which are applied immediately, and enter into five pits in the single stigma. *Linn. Tr.* xii.

Rhinanthaceæ.—Lehman has described a new genus, *Diplophyllum*, which he characterizes as having the calyx 2-leaved, compressed-closed, leaflets heartshape; corolla subrotund, 4-cut, the two opposite lobes smaller; capsule subrotund, compressed, 2-celled, 4-valved, 4-seeded. The type of the genus is *D. vero-*

nicæforme, being the veronica crista galli of Steven in Trans. Lin. Soc. xi. 408. *Berl. Mag. Nat. Fr.* for 1818.

Verbenaceæ.—Observations on the genus *Avicennia* have been given by A. de St. Hilaire in the *Mem. Mus.* iv.

Primulaceæ.—A new species of *lysimachia* was described under the name of *L. tenuifolia*, in the Trans. of the Philadelphia Society of Nat. Science; in which some doubts were expressed which Eli. Ives has resolved, and finds that it is really a dicotyledon plant.

Amaranthaceæ.—Baron F. A. Marshal, of Beerberstein, has given the following character of the new genus *Hablitzia*. Calyx 5-leaved, rotate; corolla none; capsule cut round, one-seeded. The type is *H. tamnoides*; leaves petioled, acute; stem climbing.

Polygonaceæ.—R. Brown, in the appendix to Captain Ross's narrative of his expedition, has restored Hill's genus, *Oxyria*, the type of which is *rumex digynus* of Linnæus.

Santalaceæ.—A. de Saint Hilaire has published, in the *Memoirs of the Museum*, several observations on this family.

Euphorbiaceæ.—*Micranthemum ericoides* has been described and figured by Desfontaines, who has constituted that new genus. *Mem. Mus.*

Coniferæ.—The tree found in the garden of a convent at Cyprus, called by the inhabitants 'The wood of our Lord,' and to which miraculous properties are attributed, has been considered as liquidambar imberbe; it is now found by Sir J. E. Smith (*Linn. Trans.* xii. 1,) to be the liquidambar styraciflua, a native of America, but in what manner this tree was introduced into Cyprus does not appear.

Orchideæ.—Mr. J. E. Bicheno (*Linn. Trans.* xii. 28) has unravelled the confusion that was occasioned by Linnæus reducing several orchides mentioned by the old botanists to a single species, *O. Militaris*, and has shewn that three species have been confounded together, 1. *O. strateumatica* of Johnson, 215, 12, the segments of whose lip are unequal, which Mr. Bicheno calls *O. fusca*; 2. *O. strateumatica minor* of Johnson, 216, 13, with equal and broad incurved segments; to which he restrains the name of *O. militaris*, and 3. *Cynosorchis major altera* of Johnson, 205, with equal and narrow segments, and the lip also not scabrous, to which he gives the name of *O. tephrosanthos*.

Amaryllideæ.—This family has been investigated by the Hon. Mr. Herbert, in the *Botanical Magazine*, No. 389.

Narcissi.—Mr. Haworth, in the appendix to his *History of Succulent Plants*, has published a revision of the plants contained in the Linnæan genus, *Narcissus*, now divided into eight genera; in which he has given characters to the genera which had been distinguished and named by R. A. Salisbury, and added one of his own.

Colchicaceæ.—An excellent monograph of the genus *crocus* has been published by G. L. Goldbach, in the *Mem. Mosq.* in

which he describes 15 species. Sir J. E. Smith has also investigated the genus *Tofieldia*, which was in fact the real and original anthericum of Linnæus in the first edition of his *Genera Plantarum*. In the second edition he combined it with the bulbine of his first edition, and this confusion was continued in all his subsequent works: but the English botanists, Aiton, Brown, and Hudson, laboured to unravel the confusion thus introduced. Smith now enumerates six species of *tofieldia*, of which only one, *T. palustris*, is a native of our island. *Linn. Trans.*

Junceæ.—An excellent monograph of the British *junci* and *luzulæ*, in which 21 species of the former genus and 8 of the latter are described, is published in the Linnæan Transactions, from the pen of Mr. Bichenou.

Aroideæ.—C. Kunth, having examined some of the genera contained in this family, has corrected the characters assigned to *calla*; formed a new genus from the *calla Æthiopica* of Linnæus, under the name of *Richardia*; and restored the genus *arisarum* of Tournefort. *Mem. Mus.* iii.

Piperaceæ.—The situation proper for pepper in the natural arrangement of plants has been a subject of doubt; but it is now determined by Kunth, from a consideration of the structure of the embryo, which is truly monocotyledon, that the *piperaceæ*, a family containing at present only two genera, *piper* and *pipromia*, ought to be arranged among the monocotyledones, near the *aroidæ* and *typhaceæ*. *Mem. Mus.* iv.

Gramina.—But little has been done in respect to this very difficult family; Rafinesque has, however, described a new genus, which he calls *Deplocia*. *Sillim. Journ.*

And M. Seringe has published in the first volume of his *Melanges Botaniques* a monograph of the different species of corn cultivated in Sweden.

Lycopodiaceæ.—R. A. Salisbury, on examining with attention the *lycopodium denticulatum*, found the capsule always 4-lobed, instead of 3-lobed; and also that he could never distinguish an embryo in the seed, which contains at first a clear liquid; this evaporates away quickly, and the germination then appears, which approaches much nigher to that of the dicotyledons, than to that of the monocotyledons, especially if the vitellus of Brotero is considered as the radicle.

Equisetaceæ.—Mr. Stewart, in giving an account to the Wernerian Natural History Society of a collection of North American ferns which he received from Dr. Torry, of New York, has proposed to distinguish a new species of *equisetum*, he found among them, and which is intermediate between *E. limosum* and *palustre*, by the name of *E. torryanum*; stem, branched; branches thick, roughish, hexagonal, pressed close to the stem; spike terminal: it differs from the above two nearest species in height and general habit.

Filices.—Olaf Swartz has described 26 species of *filices* found

in the Brasils, and figured 10 of them, in the Stockholm Transactions for 1817. And a new species of trichomanes, found in Jamaica, which had been sent to Desvaux by an English botanist, as a well-known plant, is described by him under the name of *T. spiciforme*; barren fronds pinnatifid, lacinia linear, lanceolate, entire, blunt; fertile fronds denudated, indusia disposed in 2 rows, alternate, spiked. *Mem. Mosq.*

Musci.—The plants of this interesting family have been of late much illustrated by the publication of Hooker and Taylor's *Muscologia Britannica*, and Hooker's *Musci Exotici*, the execution of the latter being peculiarly beautiful; nor must the poor but indefatigable collector, Hobson, of Manchester, be forgot, his Collection of dried Specimens of British Mosses and Hepaticæ, being of the greatest assistance to the students of this minute tribe.

Mr. R. Brown has newly given the characters and description of *lyellia*, a new genus, and observations respecting the section of this order to which it belongs. To this he has added, remarks on *leptostomum*, and a new genus, *hymenostomum*, the type of which genus is *gymnostomum microstomum*; *orthodon*, the type of which is *splachnum squarrosum*. This eminent botanist has also described the leaves of *buxbaumia aphylla*, a plant which has hitherto been supposed to be leafless. This latter observation has also been made by Mr. Stewart. The leaves are palmate, much lobed, and reticulated like the substance of a *jungermannia*. Mr. Stewart also observed sometimes more than one fruitstalk on the same bulb in different stages of vegetation, and, therefore, concludes, that it cannot be an annual plant, as hitherto described.

Dr. Nies has published a thesis, in which he shows that many musci appear at first merely as a greenish organized slime or jelly, with globules disseminated among it; they then assume a fibrous form, and at last appear as true mosses: in their intermediate state they have been taken for *confervæ*, or *ulvæ*.

Hepaticæ.—Nees von Esenbeck divides this family into two sub-families, 1. *Hepaticæ elateratæ*, furnished with decussating spiral fibres included in a folliculus, so that they resemble the ferns and their ring; 2. *Hepaticæ nudæ*, which have not these decussating fibres, so that they resemble the lichenes *homallophylli*. He has improved the characters of the genera included in the first sub-family, which are *Jungermannia*, capsule 4-valved, naked; *Staurophora*, capsule 4-valved, affixed to the under part of a cross-like receptacle towards the ends; the type of the genus being *S. pulchella*, the *marchantia cruciata* of Linnæus; *Marchantia*, capsule opening at the tip, tiptoothed; affixed to the under surface of a receptacle placed on a footstalk; *Duvallia*, a new genus by the author, capsules 1 or 2, cut off at top, immersed in a globose cucullate receptacle placed on the side

of a footstalk ; of which, at present, only one species is known, *D. rupestris*, found in Germany. *Berl. Mag. Naturf. Fr.*

Lichenes.—A descriptive monograph of the genus tryptothelium, by Professor Acharius, is inserted in the *Mem. Mosq.*; this monograph describes 10 species. And he has given (*Linn. Trans.* xii. 35) a monograph of 2 genera, Glyphis and Chiodec-ton, with an enumeration of their species, which were found on the bark of some tropical trees. He has described also the sub-family caliciodea, which he divides into those having the apothecia sessile, as limbora containing 9 species; cyphelium, 18 spec., or having the apothecia stipulated, as calicium, 41 spec. concocybe, 4 spec. and has figured 37 of the species in the *Stock. Trans.* continued from the preceding volumes.

Hypoxyli.—De Candolle has lately divided all the known genera into 3 tribes; 1. Sphæriæ, pulp very abundant, base fleshy, or corky, not lichenoid; containing sphæria, rhizomorpha, næmatospora, and stilbospora. 2. Xylomæ, pulp not abundant, base fleshy or corky, not lichenoid, containing polystigma, xyloma, asteroma, hypoderma and hysteriorum. 3. Lichenoides, pulp not abundant, base powdery, lichenoid, containing opegraphia, verrucaria, and pertusaria. *Mem. Mus.* iii.

In a subsequent paper he has examined the parasitic fungi, and subdivides xyloma into 3 sections, spiloma containing 14 species; microma, 22 spec.; and decroma, 5 spec. He enumerates 6 species of asteroma, 3 of polystigma, and subdivides stilbospora into 2 sections, stilbospora containing 5 spec., and hygrochroma 2 spec. Of these parasitic fungi he has figured 10. *Mem. Mus.* iii.

A new British species of rhizomorpha having been found at Derby by Mr. Booth, Sir J. E. Smith has described and figured it under the name and character of rhizomorpha medullaris; cylindrical, very much branched, snow-white; internally cellular, yellowish. *Linn. Tr.* xii.

Fungi.—A most extensive German work upon fungi has lately been brought to England, namely, *Das System der Pilze und Schwämme*, by E. G. Nees von Esenbeck; in 2 vols. 4to. with 44 plates, containing 366 figures to illustrate the genera.

Dr. Esenbeck is an advocate for the successive transition of natural substances, and of their passage from one species to another. Accordingly, he usually designates one species as the type of each family, and then shows the several shades of difference by which this species passes into the types of the other natural families. He divides fungi into 2 grand divisions, vegetabilia mycetoidea (angiocarpia Pers.) and fungosa (gymnocarpia P.) The mycetoidea are subdivided into 3 kingdoms: 1. protomyci, which includes the uredines, xylomata, and several other fungi akin to them, 2. næmatomyci, including the mu-

cedines and byssoides, 3. gasteromyci, including the lycoperdaceæ, mucores, &c. The vegetabilia fungoca are subdivided into 4 kingdoms: 1. The fungi aerei and hypogei, as the tremellæ, and the truffle; 2. fungi clavati and f. pileati, or mushrooms, properly so called. 3. f. utrini, containing the pezizæ and phalloides. 4. f. myelomyci, containing the sphæriæ and hysteriæ. Finishing the whole with the anomalous genus aphyteia of Linnæus, which he looks upon as forming the transition from fungi to phenogamous plants.

M. Otto has published in German an essay towards the arrangement of the agarics, according to the disposition of their gills. It is not that agarics are, in themselves, more difficult to determine than other species of plants, but, as their number is considerable, their duration very short, and the greatest part of them cannot be preserved for future reference, a student is, of necessity, pressed for time in endeavouring to discriminate their species.

The division adopted by Mr. Otto is singular, and has no respect to the natural sections. It is well known that some are enveloped in a volva, others have only an annulus on their pedicell; some want this part, some are milky, others dissolve entirely into a clear water: and these striking differences should undoubtedly be regarded in their arrangement. But Mr. Otto first divides them into mesopodii, with a central pedicell, of which he enumerates 225 species, and pleuropodii, in which the pedicell is excentric or lateral, containing only five species. Each of these grand divisions are subdivided, 1. into amicti, or agarics with a volva, annulus or curtain; 2. gymnopodii, or those having a naked pedicell; which are subdivided into those whose pedicell is cylindrical or bulbous. The length of the gills are then used, by which all the agarics are divided into five sections: 1. gills equal in length; 2. gills in pairs; 3. gills three in a set; 4. gills four in a set; and 5. gills many in a set, regularly placed. Here we cannot but recognize part of the arrangement of Withering.

D. Liboschitz has published an enumeration of the Russian fungi, amounting to 101 species, of which he has figured four which are new. *Mem. Mosq.*

Desvaux has constituted a new genus of these plants, sphærotheca, of which *æcidium Thesii*, Desv. Journ. Bot. 2, 311, is the type. He has also improved the specific differences of three species of sclerotium, and described a new one, *S. moschatum*, found in autumn in the woods, upon the heaps of putrid leaves, which is very like the *S. subterranea* of Tode, but differs in colour and odour. This author has also shown that the *merulius lycoperdoides* of De Candolle, Fl. Franc. 340, or *agaricus lycoperdoides* of Bulliard, is in fact a new genus, to which he has given the name of *mycoconium*; and that the *lycoperdon corium* of De Candolle is a true *scleroderma*. *Mem. Mosq.*

Mr. Macbride has also made some observations on the lycop-

perdon solidum of the flora Virgin. *Trans. Linn. Soc.* xii. And Elias Tries has published in the *Stock. Trans.* an arrangement of the Scleromyci, in which he describes no less than 210 species of spheria, divided into 13 sections, and the paper is not yet concluded.

Algæ.—The knowledge of the algæ is perhaps the most imperfect of any of the departments of natural history, on account of the difficulty of obtaining and examining any considerable number of them; besides, the fresh-water algæ present so little beauty to the eye, being in general only simple threads of a green colour, which, on drying, lose their distinctive characters, inasmuch as the *chloroma*, or green matter contained in their tubes, and probably their seed, although sufficient, when the plants are fresh, to distinguish them, becomes totally insufficient for that purpose when they are dry, as it is no longer distinguishable from the general mass.

Dr. Nies, as above stated, has found, like Mr. Drummond, that several of the confervæ and ulvæ, are in fact immature plants of musci, or lichenes. This is the case with *conferva frigida*, *C. castanea*, and *ulva rupestris*. These plants are of great importance in forming the first soil at the bottom of water, and even of the sea, whose bed is gradually raised by a species of *conferva*, the *C. chthonoplastes*.

Linnaeus arranged the algæ in only four or five genera; and this contented the botanists for a long time, till Roth, in his *Tentamen Floræ Germanicæ*, began, but with a sparing hand, to augment the number. Vaucher, in his *Histoire des Conferves d'Eau douce*, was more bold; and Lamouroux, in his *Essai sur les Genres de la Famille des Thalassiophytes non articulées*, examined the fucoides and ulvaceæ; arranging them principally by their organization and habit; as the marine algæ can seldom be found in a state of fructification.

Agardh has adopted much of the method of Lamouroux; and although he has only peculiarly examined those that grow in the Northern countries, he gives a systematic arrangement of nearly the whole family, with reference to the authors who have treated of them at greater length. He divides the algæ into four sections: 1. *Fucoideæ*; 2. *Florideæ*; 3. *Ulvoideæ*; 4. *Confervoideæ* and *Tremellinæ*; which are afterwards subdivided into 45 genera.

This family has also been very much illustrated by the labours of Hans Christian Lingbye, who published this year his *Tentamen Hydrophytologiæ Danicæ*, in 4to. with 70 plates, containing from three to six figures each. He divides them into—1. *Phycoidata*, frondose, continued, solid, flat, compressed, or round; 2. *Soleniata*, frondose, continued, tubular; 3. *Stereogonata*, frondose, jointed, round, solid; 4. *Siphonogonata*, frondose, jointed, round, tubular; 5. *Homalagonata*, frondose, jointed, flat; 6. *Tremelloidata*, gelatinous. Under these ar-

tifical divisions, he enumerates 49 genera, 11 of which are new.

Fuci.—Mr. Dawson Turner having discontinued the publication of his excellent History of the genus *Fucus*, we have great pleasure in saying, that the Swedish Professor, Agardh, means to continue it.

2. PHYSIOLOGY OF PLANTS.

Epidermis of Plants.—Grew, Malpighi, and Mirbel, have regarded the epidermis of plants as a mere scurf, formed by the action of the air and light upon the exterior and pulpy substance of the parenchyme. Mr. Keith, however, (*Linn. Trans.* xii. 6.) observes, that the epidermis may be observed completely covering the inclosed parts of buds before they have been exposed to the air; and the action of the air is even detrimental to its formation in wounded plants: hence, the epidermis is a distinct organ, discharging a peculiar function of the vegetable economy, and entirely analogous to the epidermis of animals.

Leaves.—A paper, by Dr. Macbride of South Carolina, refers the power of the urceolate leaves of *sarracenia* to entrap insects, to the inverted position of the hairs in the mouth of the tube, which causes them to slip into the hollow of the leaf, and prevents their return. A saccharine juice is secreted in the bottom of the leaves which attracts the insects; so that these plants are used as fly-traps by the inhabitants of those countries. *Linn. Trans.* xii.

Transformation of Organs.—Aubert du Petit-Thouars has lately considered the tendency of the parts of fructification to change into leaves. He observes, that all the parts of the flower of *verbascum pyramidatum*, except the stamens, manifest a tendency to change into leaves. The author imagines that all the observations hitherto made on this subject confirm his opinion of the origin of the flowers of plants: according to which, the calyx, corolla, and stamens, result from the transformation of a leaf, and the pistill from that of a bud, produced in the axilla of that leaf; the leaves of this bud, agglutinating together, forming the pericarp. The ovules are, according to him, formed by other buds, produced from the former, but whose leaves, insulated or folded together, and incapable of producing other buds of themselves, form only barren cocci, until they are fecundated by the male organ.

From the apparent decomposition of the glandular disk in the *boopideæ*, Mr. Brown thinks it may be inferred, that, in many families, the nectarium may be considered as a series of modified stamina, whether it exists as a simple ring, or consists of several bodies, alternating with the stamina, and, in some cases, putting on the appearance of barren filaments. *Linn. Trans.* xii.

Seeds.—Botanists have generally admitted, that seeds are never produced absolutely naked; but the systematists have agreed to

term a naked seed, not only those fruits in which the integument, to which the ovulum was originally attached by a point only of its surface, has acquired so complete an adhesion to the proper coat of the seed as not to be separable from it, but also every monospermous pericarpium bearing a general resemblance to a seed, and whose outer covering, though distinct from the nucleus, is only ruptured after germination has commenced. Of these fruits, improperly called naked seeds, Richard distinguishes two principal kinds: 1. the akenium, in which the pericarpium is distinct; and 2, the cariopsis, in which the pericarpium coheres with the seed.

Although an ovulum has not yet been found to be produced without a covering, yet Mr. Brown has shown, that in *Leontice*, the immediate effect of impregnation is to produce a swelling of the ovulum without a correspondent enlargement of the ovarium; in consequence of which, the ovarium is ruptured, and a seed, provided with its proper integuments, is protruded, at the base of which, when ripe, the withered remains of the ovarium are visible. The *peliosanthes Teta* has also originally three cells, each containing two ovula, but, soon after impregnation, from one to three of these ovula rapidly increase in size, stifle the others, and rupture the ovarium, which remains but little enlarged at the base of the fruit, consisting of from one to three naked berry-like seeds. In *sterculia*, the ovarium is thus ruptured in the direction of its sutures, and the valves acquire the form and texture of leaves, to whose thickened margins the ovula continue attached until they ripen. A somewhat similar instance occurs in *reseda*, whose capsule opens at top, at a very early period.

Other anomalies from the usual economy of seeds are observable. In the mangroves, *rhizophora* and *Bruguiera*, the embryo, long before the seed is detached from its parent plant, pierces the pericarpium, and acquires the length of one or two feet, and the proper integument of the seed is absorbed. In *eugenia*, the integument of the seed is completely absorbed, even while the pericarpium remains entire. In some species of *pancratium*, *crinum* and *amaryllis*, the seed separates from the plant, and even from the pericarpium before the embryo becomes visible, the seed having a central cavity filled with a glairy fluid, in which the embryo is afterwards formed, which in some cases does not become visible unless the seed is placed in a situation favourable to germination, so that we can regulate the direction of the radicular extremity. In certain *aroidæ*, especially in *calladium*, the nucleus of the seed is composed of a mass resembling the tuber of a root, of an uniform structure, having neither cotyledon, plumula, nor radicle, and frequently with more than one germinating point on the surface. *Linn. Trans.* xii. 143.

3. FLORÆ OF DIFFERENT COUNTRIES.

Germany.—Opiz has lately published, at Leipsic, an arrange-

ment, or rather catalogue, of the German cryptogamous plants, according to the soils or plants on which they grow.

Cape of Good Hope.—Thunberg has lately investigated several genera found there, and figured some of them, enumerating three new species of rhamnus, seven of solanum, 33 of lobelia, one each of the genera schœnus, alopecurus, dactylis, and ehrhartia.

Russia.—The Memoirs of the Imperial Academy of Moscow, lately imported, contains many observations and descriptions of several hitherto undiscovered plants found in the Russian empire. The dissertation of Mr. Stevens contains a monograph of the genus Fedia: of this genus he enumerates 13 species, divided into five sections; also, a new species of iris, *I. paradoxa*; a new genus of the family of scirpideæ, viz. *bulbostylis*, to contain *scirpus ovatus* and *s. palustris*: observations on the several species of *corispermum*, of which he describes five: on *veronica*; on *lycopus amethystinus*, which he denies to be a separate genus; and on the *valerianææ*.

India.—Mr. Colebrook has given a description of several select Indian plants, of which the following are the principal points: *Sabia*, a new genus, whose characters are, petals 5, lanceolate, persistent, inferior; stamens 5, straight; drupe superior, kidney-shape, 1-seeded; style one; embryo direct, spiral; perisperm none: of one species, *s. lanceolata*, he gives a figure.—*Strychnos axillaris*, a new species with the leaves ovate, pointed; tendrils axillary, thickened; berry oval, 1-seeded.—*Dischidia Bengalensis*; leaves oblong, ancipital.—*Tylophora exilis*; panicles terminating in umbellets; corolline segments oval, obtuse; leaves subcordate, acute, smooth, becoming firm and fleshy.—*Macrolobium bijugum*; flowers triandrous, 6 sterile filaments, leaves 2-paired.—*Pygæum acuminatum* of Gærtner; flowers icosandrous, monogynous; calyx inferior 6-toothed; petals 6; drupe dry, transversely oblong; embryo inverse; perisperm none. *Linn. Tr. xii.*

Canary Islands.—Von Buch has given an account of the plants of those islands in the Berlin Magazine of Natural History.

Tristan de Cunha.—Captain Dugald Carmichael, having examined this insulated spot, has given a list of 55 species of plants he found upon it, of which 8 are new.

America.—M. Rafinesque has published in the Journ. de Physique, Aôut, 1819, an account of fifty new genera of plants, established or discovered by him. They are all American plants. And he has also published in the same journal his remarks on the works of Pursh, and other American botanists.

Java.—A most magnificent and singular flower has been discovered in Java, rising immediately out of the ground, without any leaves at the time of flowering. The flower is between two and three feet in circumference, flat like a soup plate, the lower part or base is covered with excrescences and filaments; the limb

is 6-cut, the lobes blunt, irregular, reddish marbled. A specimen of the flower, but which arrived in a very bad state, and of two young flowers, unopened, and appearing like cabbages, have been received in this country; the latter have been dissected, and drawings made of them by Mr. Bauer, which drawings, and the remains of the specimens, are deposited in the library of the munificent patron of natural history, Sir Joseph Banks, whose recent death we have, in common with every other naturalist throughout the world, reason to deplore.

On reviewing this hasty sketch of the progress of botany for the last year or two, no one can avoid being struck with the rapid progress made by the natural system, and the continually increasing neglect of the sexual arrangement of Linnæus. Even in the Transactions of the Linnæan Society the papers are, with scarce a single exception, upon the plan, and couched in the language, of that school which professes to follow nature through all her devious windings, whatever may be the difficulties that occur in the search. This release from the fetters of authority, cannot but augur good to the science; and we have no doubt, but that in a few years, botany will be able to regain the time which has been lost in the arrangement of plants by the mere number, proportion, and connexion of their sexual organs, to the total neglect of the study of their affinities, and the rising generation of botanists look back with astonishment at the exclusive reception of the Linnæan system, and the neglect of the systems of Rivinus, Tournefort, and Ray; the first of which is certainly more simple, and better adapted for a mere pinax than that of Linnæus.

ARTICLE II.

On two Acids of Manganese, the Manganeseous and the Manganesic Acid. By Dr. G. Forrhammer.

It has been long known that potash or nitre when mixed with black oxide of manganese, and ignited, forms a compound, which dissolves in water with a green colour, and which, when exposed to the air, soon changes its colour to a beautiful purple. This compound, well known under the name *chamæleon mineral*, has been viewed by chemists in quite a different way. Some persons have considered it as a compound of protoxide of manganese and potash; some even as suboxide united to the alkalis; while others are of opinion that it consists of peroxide and potash. Scheele, however, showed long ago that the green body, when heated with arsenic or charcoal, soon lost its colour

entirely; and Chevillot and Edwards have proved by an ingenious experiment that even the peroxide of manganese and potash, when heated together, absorb oxygen, in order to form the green compound. It is well known, that their experiments induced them to consider the *red* compound of oxide of manganese and potash as a neutral manganate of potash, and the *green* compound as a submanganate of potash; but they were unable to procure the manganetic acid in a separate state.

I happened about the same time to be engaged in an inquiry into the nature of the different oxides of manganese, and was fortunate enough, though unacquainted with the late papers of these French chemists, to procure the same acid quite in another way, and even to hit upon a method of obtaining it in a separate state.

I prepared a solution of green chamæleon by igniting potash with peroxide of manganese, and dissolving the green salt in water. After the solution, there remained deutoxide of manganese; the peroxide being decomposed by the action of potash, and a portion of its oxygen accumulated in that part of the peroxide which was dissolved. The green solution very soon became red, but at the same time it deposited deutoxide of manganese with a brown colour.

When I added to the red solution of chamæleon a solution of very pure potash, it did not change the colour at all; but when I afterwards added the least drop of alcohol, the colour began immediately to assume a tint of green, and if there was alcohol enough, the most beautiful green was produced. When too much of alcohol was added, all the colour disappeared, and deutoxide of manganese was produced.

These experiments rendered it probable that the difference between the green and the red chamæleon consisted not in the quantity of potash combined with the oxide of manganese, but in the different quantity of oxygen combined with the manganese; for it is certain that the oxide of manganese combined with potash in the green chamæleon contains more oxygen than the deutoxide. Indeed as it contains even more oxygen than the peroxide, and as part of the oxide of manganese in the green solution is reduced to deutoxide, it is necessary to suppose either that the oxygen is accumulated in the other part, or that it is combined with the potash or water, which certainly is not very probable. I took another portion of the red solution and poured into it a solution of potash containing the powder of carbonate of manganese.

As soon as it was added, the colour changed directly into a more or less beautiful green, and the protoxide of manganese was directly converted into deutoxide. If I added too much of the liquor, the whole was changed into deutoxide. This experiment proves quite clearly that the green solution contains less oxygen than the red solution of the chamæleon. However the oxide in

the green solution contains more oxygen than the peroxide on this account; and because it forms certain compounds with alkalis I have named it *manganeous acid*.

The necessity of adding potash together with a body absorbing oxygen appears contrary to this opinion; but this is owing to the very weak nature of the manganeous acid, its very existence depending entirely on the presence of potash (or some other alkali) and water. The compound of manganeous acid and potash is a submanganosite. Whenever the potash is saturated, or its action weakened, the manganeous acid is immediately decomposed into deutoxide of manganese and manganic acid. It will be very easy to explain by this fact all the different phenomena which the chamæleon shows. A green solution, when exposed to the air, turns red, by the absorption of carbonic acid, which always precipitates deutoxide of manganese; sulphuric and nitric acid have the same effect, and even water will do the same, when added in great quantity, as it weakens the power of the potash; just as that liquid precipitates the oxide of bismuth from its nitric solution. It is the same thing with the reduction of the red colour to the green, which best takes place when an alkali and some matter absorbing the oxygen are added together; but that it is not absolutely necessary in every case to add alkali is proved by an experiment quoted by Chevreul, who obtained green chamæleon, when he filtered red chamæleon through paper. It is plain that by the contact of the paper some oxygen from the manganic acid was absorbed. This reduced it to manganeous acid, and gave it the green colour.

Let me remark here a very interesting fact in respect to the changes which the different oxides of manganese undergo by acids and alkalis. The deutoxide of manganese, when boiled with nitric or sulphuric acid, is entirely converted into *protoxide*, which is dissolved, and *black peroxide*; the peroxide, when heated with potash, is changed into *manganeous acid*, which combines with the alkali, and into *deutoxide of manganese*. The manganeous acid, when combined with potash is, by acids, changed into the soluble manganic acid and deutoxide, and perhaps the manganic acid, treated in a gentle heat with potash, would form a still higher degree of oxidation and manganeous acid.

I prepared a solution of manganiate of potash by passing carbonic acid through a solution of submanganosite of potash. When the red colour was no more altered by the carbonic acid, I separated the clear red liquid from the precipitated deutoxide of manganese, and added sulphuric acid. Exposed to spontaneous crystallization, it let fall crystals of sulphate of potash, of manganiate of potash, as the French chemists have described them, and of bisulphate of potash, which shows that the sulphuric acid, if it decompose the manganiate of potash at all, only decomposes it when it can form bisulphate of potash, which

holds also with the nitrate of potash. As there is, therefore, no hope that in that way a tolerably pure manganic acid may be obtained, I shall not repeat the numerous experiments which I tried in vain to get this acid pure, but only state the process by which I succeeded.

I prepared a solution of submanganite of potash, and added to it a solution of nitrate of lead, which threw down a brown powder, and destroyed the green colour entirely. This brown powder was washed as long as it contained any soluble body. It was then carefully mixed with sulphuric acid, diluted with 10 times its weight of water. It is necessary to preserve a portion of the brown powder in order to be able to neutralize any excess of sulphuric acid that may happen to have been added. A long digestion of the liquid with the brown powder is necessary in order to combine all the sulphuric acid with the lead. We must beware not to dry the brown powder as it is only in the state of hydrate that it is capable of forming the manganic acid.

This process is easily explained. The protoxide of lead is by the manganeous acid changed into peroxide, which combining with the deutoxide of manganese forms the brown powder. When sulphuric acid is added, it causes the peroxide of lead to give a part of its oxygen to the deutoxide of manganese, and produces manganic acid, deutoxide of manganese, and sulphate of lead. The manganic acid thus obtained is of the most beautiful red colour, is easily soluble in water, has a sharp unpleasant taste, and stains the skin and other animal and vegetable substances of a beautiful brown colour. It destroys the colour of litmus paper, and renders it brown by depositing deutoxide of manganese, and it would bleach quite as well as chlorine or oxidated water, if its oxide was not coloured itself, and produced a new colour.

When heated and evaporated, it is decomposed; it forms brown oxide of manganese, and gives a smell almost like that which the glass of an electrical machine gives when rubbed; the same smell rises, when the manganic acid is exposed to the beams of the sun. The nature of this vapour I was not able to make out; whether it was an oxide of manganese, or other substance, I cannot say. When manganic acid is heated with muriatic acid it is entirely decomposed, and reduced to protoxide, which combines with the muriatic acid. As I was not in possession of a good air-pump, I could not ascertain what effect the concentration of the manganic acid by means of sulphuric acid in the vacuum would produce, and whether it would yield crystals. Potash, or any other alkali, is not at all required for the formation of the manganic acid; hydrated peroxide of lead, sulphate of magnesia, and sulphuric acid, mixed together will produce the same body; but in this case, it is still more difficult to get it free from sulphuric acid. In several analyses I used the following way to separate salts of *manganese* and *mag-*

nesia from each other. I added to the sulphuric or nitric solution of these two bodies hydrated peroxide of lead and sulphuric acid, and digested it for some hours. The red solution thus formed contains *sulphate of magnesia, manganic acid, and sulphuric acid.*—The magnesia is precipitated by carbonate of potash, and the manganic acid by alcohol, which converts it into brown oxide. As it is impossible that any salt of the protoxide of manganese can exist in the same solution which contains the manganic acid, the magnesia must be free from manganese, unless during the precipitation of the first body the manganic acid be destroyed, and deutoxide precipitated, which may easily be avoided. To be sure that in the brown powder which remains, and consists of peroxide of lead and sulphate of lead, there is no peroxide of manganese, I boil it with sulphuric acid, and heat it gently; water then will dissolve all the sulphate of manganese which would be formed, and carbonate of potash will precipitate the carbonate of manganese, which, together with the deutoxide before obtained, will, when ignited to a cherry-red heat in an open crucible, give the whole quantity of deutoxide of manganese. Another way in which manganic acid is formed is when chlorine gas, prepared by means of peroxide of manganese, is absorbed by a solution of potash. The chlorine acid, volatilizing a small quantity of peroxide of manganese, converts it, when acted upon by the potash, into manganic acid, which is combined with the potash. This has been long ago remarked by the chemists; and Dr. John, of Berlin, believed that he had found out a new acid, which was the oxide of an unknown metal; but a consideration of the effects produced by the different tests will show that this new acid is identical with the manganic acid.

*The Solution of the new Metal,
according to Dr. John.**

1. The tincture of gall-nuts gives a precipitate of brown colour like chesnuds.

2. Ferrochyzate of potash changes the colour directly into lemon-yellow.

3. Carbonate of potash does not change it.

4. Alcohol immediately alters its colour into green.

*The Solution of Manganiate
of Potash.*

1. The same effect.

2. The same effect when the solution of the manganiate is very weak, but when it is concentrated, it makes a brown precipitate.

3. The same.

4. The same effect is produced.

I obtained the same body when I passed chlorine through

* John, Laboratorium. Berlin, 1811.

water containing the powder of peroxide of manganese. My object was to see whether I could produce a degree of oxidation higher than the peroxide. When I added potash to the water thus saturated with chlorine, I got a small quantity of deutoxide of the manganese, probably because a small quantity of deutoxide had existed in the peroxide, and the fluid over this powder soon became red. There exists some submagnesiates which are insoluble, and form a red powder, but I never could find out any method of producing them in a direct way. They are merely obtained by accident; and I have principally noticed that of barytes. All the oxides of metals seem either to form soluble compounds of a red colour with the manganic acid, or to decompose it, in which case the oxide of the metal is converted into peroxide, and the manganic acid reduced to deutoxide. This I found to be the case with lead, and it seems to hold also with silver. The brown powder thus obtained by lead is soluble in cold nitric acid with a brown colour; when heated, it turns red. This fact proves that it is a real chemical compound of the brown peroxide of lead (acting as an acid), and the deutoxide of manganese, for neither the first nor the second of these bodies is of itself soluble in nitric acid. By the elevation of the temperature to that of boiling water, the oxygen from the peroxide of lead goes over to the manganese, and changes it into acid. I first believed that under certain circumstances the nitric acid might be able to reduce this compound to protoxide of the two metals; without that supposition I could not make out what had become of the oxygen; but afterwards I found it probable that it depended upon a small quantity of nitrous acid combined with the nitric acid.

The *manganesite of barytes* I obtained by igniting nitrate of barytes together with oxide of manganese, and washing the green powder thus obtained with boiling water. It has a beautiful emerald green colour, and, when dry, is very little altered by the air. *Manganesite of lime* I could not obtain in the same way.

In order to ascertain the quantity of oxygen in the manganic acid, I precipitated a solution of the green chameleon which was very clear, by nitrate of lead. The brown powder was dried, and I put it with bisulphate of potash into a retort, which, by means of a tube of glass, was connected with a glass vessel, open below, and shut above, by means of a stop-cock. I filled the vessel with water before I put the apparatus together, and then opened the cock to see whether it was quite tight. When after some hours no change had happened, I heated the retort so as to melt the bisulphate of potash, and kept it warm until the brown powder had lost its colour entirely, and was converted into sulphate of lead and sulphate of manganese. The glass vessel contained a volume of air equal to the volume of oxygen expelled from the brown compound, and it might con-

tain some traces of carbonic acid if submagnesite of potash had absorbed any carbonic gas, and there had been formed together with the brown powder some carbonate of lead. However, the volume of carbonic acid was very small, and easily removed by cold water. After all the reductions necessary for the difference of temperature and pressure of the air after and before the experiment, &c. &c. the volume of oxygen thus obtained was 4.82 French duodecimal cubic inches at a temperature of 10° of the centigrade scale, which, according to Biot, is equal to 0.1303057 grammes oxygen. The salt in the retort was dissolved by water, and as the sulphate of lead is insoluble in water, carbonate of soda precipitated the carbonate of manganese, which, by igniting in an open crucible, gave 0.282 grammes deutoxide. I shall prove hereafter that 100 parts of deutoxide of manganese consist of 92.4342 protoxide, and 7.5568 oxygen; and, therefore, 0.282 deutoxide are equal to 0.261 protoxide, and this quantity of protoxide was combined with 0.1303 of oxygen in the manganeseous acid; for the protoxide of lead which was added only served to fix the oxygen, and it was removed in the same state of protoxide in which it was first used; therefore 100 protoxide are combined in the manganeseous acid with 49.92 oxygen. But I shall prove by direct experiments that the suboxide of manganese consists of 20.576 oxygen, and 100 metal; that the protoxide consists of 100 metal and 31.29 oxygen; that the deutoxide consists of 100 metal and 42.04 oxygen; and that the peroxide consists of 100 metal and 62.819 oxygen; and that, therefore, the oxygen in the different oxides is very near to the numbers 2, 3, 4, 6.

The manganeseous acid consists of 100 metal and 96.847 oxygen, which is pretty near to the number 9, but the number 9 in the oxides of a metal never has been yet found, and it is not very probable that this metal should make an exception. Besides that, it appears almost as necessary that the effect which water has in the green solution, already, in a certain degree, has been exercised during the first solution of the dry salt, and that the green solution contains already some portion of manganeseic acid, that, therefore, the blue salt which now and then is obtained by igniting potash and peroxide of manganese is the real manganeseite of potash, and that the green solution is a compound of blue and red. There is still another reason which induces me to believe that not 9 but 8 is the number for the manganeseous acid, and which depends on the nature of the brown compound of lead, manganese, and oxygen. We have considered it as a compound of peroxide of lead and deutoxide of manganese, but that is only possible if the manganeseic acid contains double the quantity of oxygen of the deutoxide; for as the peroxide of lead contains double the quantity of oxygen of the protoxide of lead, it is only in that case possible that such a compound can exist. If the manganeseous acid contains really three times as much

oxygen as the protoxide, it is necessary to consider the brown powder as composed of acid of lead, containing three times as much oxygen as the protoxide, and protoxide of manganese, which is merely hypothetical.

In the analysis of the brown powder as well as in many other analyses of deutoxide and peroxide of manganese, I always used bisulphate of potash to expel all the oxygen which is combined with the protoxide, and this salt is much preferable to the pure sulphuric acid, as it decomposes the deutoxide, and hyperoxide very easily, does not give any vapour of sulphuric acid, and melts so easy that all the parts of the oxide come in contact with the salt.

The manganic acid I analyzed in the following way: I made a solution of green manganite of potash, quite the same as before, and converted it by carbonic acid gas into manganiate of potash and deutoxide. The weight of the ignited deutoxide was 0.136 gramme. The manganiate of potash I decomposed by alcohol, and obtained 0.214 gramme of ignited deutoxide. As in the experiment above quoted 0.282 gramme deutoxide are combined with 0.1093 gramme oxygen in the manganeous acid, 0.350 (0.136 + 0.214) gramme deutoxide will be combined with 0.1354 gramme of oxygen in the green manganite potash, and this whole quantity of oxygen = 0.1354 gramme is combined with 0.214 gramme of deutoxide in the manganic acid; and as 0.214 gramme deutoxide consist of 0.1506 metal and 0.0634 of oxygen, 0.1506 of metal are combined with 0.1988 of oxygen in the manganic acid, or 100 of manganese metal are combined with 132 of oxygen in the manganic acid, which approaches pretty near to the number 12, if we consider the difficulty of the analysis, and the small quantity of matter on which the experiment was made.

ARTICLE III.

Researches into the Mathematical Principles of Chemical Philosophy. By J. B. Emmett.

(To Dr. Thomson.)

SIR,

Hull, June 28, 1820.

INTRODUCTORY REMARKS.

THAT the primary laws of chemical action may be investigated upon mathematical principles appears probable when we consider that in a neutral salt the proportion of the elements to each other is invariable, that when a sub and neutral, or neutral and super-salt may be formed by the union of the same acid and base, the ratio that subsists between them in one of the compounds, is to that in the other, as 1 : 2 or 1 : 3 or 1 : 4, &c. never as 2 : 3 or 3 : 4,

&c. that the same attains in the different compounds formed by the union of oxygen and chlorine with metallic bases; that in crystals of the same substance, how varied soever be the external form, the figure of the nucleus is invariable; that all bodies expand by an increase, and contract by a diminution of their sensible heat, following regular and peculiar laws; that under the same degree of atmospherical pressure, any given liquid invariably boils at the same degree of heat, as indicated by the thermometer; that all gases are equally dilated by the same increment of heat; and that, as has been clearly proved by Sir I. Newton, every particle of matter in nature attracts every other particle, the force varying reciprocally in the duplicate ratio of the distance between their centres. To suppose that all chemical phenomena can admit of rigorous mathematical investigation, would be extravagant; we can only expect, at least in the present state of chemical science, that the primary laws of action can be developed; such are cohesion, expansion, construction of solids, liquids, and aeriform fluids, radiation of caloric, crystallization, and some facts relating to chemical affinity, and the atomic theory.

Numerous hypotheses have been advanced to explain the phenomena of corpuscular action; many of these appearing to be liable to objections, the introductory part of these papers will contain a brief examination of some of the principal systems.

Philosophers are generally agreed that every species of ponderable matter is composed of indefinitely minute particles, atoms, or corpuscles, which are constantly under the influence of two powerful antagonist forces; viz. the centripetal, or that by which, when at liberty, they tend towards each other; and the centrifugal, or that repellent force which results from the operation of that which causes the sensation of heat, which, in the permanent state of any form of matter, must be in mutual equilibrio.

That matter is composed of such solid, indestructible, immutable atoms appears from the general phenomena of chemical combination, expansion, and the existence of the same matter, in the solid, liquid, and gaseous state; this has been so fully and perfectly demonstrated by many eminent philosophers, that any further proof would be useless. With regard to their figure, nothing can be proved; I shall assume it to be spherical, since this adapts them in the most simple and perfect manner to the formation of every variety of crystal, as that which of all solids contains the greatest quantity of matter under the least surface, and the only solid which can exert the same force at all equal distances from its centre. In explaining the phenomena of cohesion, it is immaterial of what figure we assume the particles of matter, provided it be such as may be formed by the revolution of a curve of finite curvature, and which returns into itself, upon its axis, as will subsequently be demonstrated, but if the parti-

cles be supposed to have any part of their surface plane, the observed phenomena cannot be produced. It appears, Newtoni Princip. lib. 1, prop. 11, that the planetary spheres tend towards each other, the force varying reciprocally as the square of the distance between their centres; and from prop. 71 and 74, cor. 3, every particle of which these bodies are composed attracts every other particle, according to the same law. This force has generally been considered incapable of producing the phenomena of cohesion from reasons similar to the following: Suppose a corpuscle or minute mass of matter to be placed upon a sphere of considerable magnitude; experiment proves that their mutual attraction is insensible; let the diameter of the sphere be continually diminished, and the attraction will diminish as the diameter of the sphere (Princip. lib. 1, prop. 72); much more then will the attraction be insensible when the sphere is reduced to the magnitude of one of the ultimate atoms of matter; besides, if the attraction of such a sphere be sufficient to produce cohesion in contact, it must be sensible at even a considerable distance from contact (Princip. lib. 1, prop. 74). This difficulty has induced some philosophers to assume the existence of another species of centripetal force, which decreases as the cube, or some higher power of the distance. The existence of such a force cannot be admitted without sufficient proof; it has been introduced to free the subject from one difficulty, and at once involves it in many of much greater magnitude. Suppose chemical attraction to result from the operation of such a force; since it is inherent in, and exerted by, all the particles of matter, masses composed of such must possess an attractive force, which is made up of this, and of that which is known to exist, and which varies reciprocally in the duplicate ratio of the distance. Now by the united researches of the most eminent mathematicians, all the phenomena of the motions of the bodies belonging to the solar system have been demonstrated to result from the operation of the latter force only; but if the former force, which has been assumed by chemists for the sake of convenience, existed, there must arise a new set of disturbing forces which are inconsistent with, and inexplicable upon, the Newtonian system, particularly in the moon's motion; and had such existed, from the present perfect state of astronomical science, they could not have escaped observation: also the perihelia of the planetary orbits must be affected with a very considerable motion, inexplicable by the known operation of that force which has been proved to exist; for instance, the perihelion of the earth's orbit would have an annual motion of $1' 5''$, if we increase by $\frac{1}{1000000}$ th part the power of the distance to which the solar gravity is proportional; whereas the real observed motion is only $11.6''$ (Laplace's System of the World, vol. ii. p. 12), and this results from the operation of the Newtonian law of gravitation only. It would be useless to investigate the nature of these

perturbations; it is quite sufficient to know that no motion whatever has been observed in our system which gives the slightest trace of the operation of any other centripetal force; and if any other existed, the planets, and more particularly the comets, could never describe ellipses, having the sun in one focus.

In order to explain the phenomena of the expansion and contraction of solid matter, many have supposed that the particles of solids are not mutually in contact with each other. Now if they do not touch, they must be preserved at a certain distance from each other, by the equilibrium of two opposite forces; under these circumstances, they must have perfect freedom of motion among themselves, which is highly inconsistent with the phenomena of solidity and cohesion, this freedom of motion of the particles being the primary characteristic of fluidity. By whatever centripetal force cohesion is supposed to be produced, it can attain only in perfect contact, as is sufficiently proved in Princip. lib. 1, prop. 81, note 526, by Le Seur and Jacquier, prop. 86, or Simpson's Fluxions, art. 384: but all solids expand by an increase of their sensible heat, if, therefore, this expansion arises from an absolute separation of the particles from each other, the least possible augmentation of temperature must produce fusion; besides, since diminution of temperature produces contraction, the particles, according to this hypothesis, cannot have previously been in contact, and consequently cannot have had any cohesion, which is absurd. Arguments have been advanced in favour of this hypothesis, deduced from the apparent want of contact between a convex lens and flat plate of glass, although urged by a very considerable force; the inflexion of light is quite sufficient to account for the apparent anomaly; but should this not be allowed, it is easy to prove from other phenomena that they must be in actual contact. While the glasses are strongly pressed together, let the convex lens be moved along the surface of the other glass, and they will mutually scratch each other. In solid matter, the attraction of cohesion is allowed to exceed all the other corpuscular forces; this, as well as all the other corpuscular forces, is experimentally proved to vanish at the least distance from contact. Now this experiment proves that these pieces of glass do really scratch each other; i. e. each removes from the surface of the other some of its ultimate atoms; by which it appears that by some means the attraction of cohesion is overcome; it cannot have been effected by the agency of the caloric resident in the pieces of glass, because then the repulsive force of caloric in glass must exceed, even at a considerable distance, the force of cohesion, much more then must it exceed it in contact; nor can it have been effected by means of a centripetal force, for then the attraction at a distance must exceed that in contact, or the attractive force vary directly as some power of the distance;

both of which are absurd. The phenomenon must remain inexplicable, unless we admit that the two glasses can be brought into perfect contact.

There has been much speculation respecting caloric, or that which produces the phenomena of heat; there are at present two principal hypotheses; according to one, the immediate cause of the phenomena of heat is supposed to be motion, and the laws of its communication precisely the same as those of motion; and this motion is supposed to be of a vibratory or undulatory nature, or a motion of the particles round their axes, or round each other (Davy's *Elem. Chem. Philos.* p. 94); according to the other, caloric, or the matter of heat, is supposed to be a real ethereal, or highly subtile elastic fluid, capable of insinuating itself into the interstices between the particles of all matter. Of these, the first appearing totally untenable, it will be requisite to examine the grounds on which it appears objectionable before the principles which are intended to be established are advanced; and here we must particularly observe that no analogy between the phenomena of heat and those of undulatory motion has ever been proved; the only sort of vibratory or undulatory motion with which matter is known to be affected is that which produces sound, or which is communicated to solid matter, by those vibrations of the air by which sound is produced; and the phenomena which hence arise are by no means analogous to those of heat. It is possible to imagine other sorts of vibratory motion, and to suppose them capable of producing different effects; but in strict philosophical research, nothing can be admitted which has not been proved to exist: besides, this motion, if there be such, can never be rendered perceptible to any of our senses; the assumption of it, therefore, ought not to be allowed, unless some physical cause be assigned which must produce it; now there certainly is no power or force inherent in matter by which any vibration whatever can be produced among the particles of matter; for the only force whose existence has been proved is centripetal, and the only effect of this is to cause the particles from a state of rest to tend to each other's centres in right lines, with an accelerated motion, till perfect contact is attained, when all motion whatever ceases: in addition, such motion, at least in solids and liquids, must meet with great and constant resistance, especially from the force of cohesion in the former; consequently when a solid has attained the temperature of the surrounding medium, the vibration of its particles must be continually diminishing; and in order that it may preserve the uniform temperature to which it is exposed, it must constantly be absorbing heat, or have the motion which is lost in consequence of the resistance which opposes it renewed; and, therefore, a solid can never attain the precise temperature of the surrounding medium, but, however long it may be exposed to it, it will always excite the

sensation of cold; which is diametrically opposite to the observed phenomena. Another objection arises from the phenomena of the radiation of heat. Heat radiates with great facility in vacuo (Davy's *Elem. Chem. Philos.* p. 207). Also the motions of the planetary bodies demonstrate that they are performed in a medium void of all resistance; the portion of space, therefore, in which they move must be a perfect vacuum, yet the solar calorific rays pass through a vacuum of nearly 95,000,000 miles in extent before they reach the earth; it is extremely difficult to see by what means the vibration of the solar particles can affect those of the earth so as to excite upon its surface the sensation of heat. It may certainly be urged that the vibration of the solar matter excites undulations in an ethereal fluid which pervades all space, and which communicates similar vibrations among the particles of which terrestrial matter is composed. This is certainly impossible; for this medium, if such exist, possesses so great rarity that its effects cannot be rendered sensible. It is, therefore, perfectly impossible for undulations in such a medium to excite so strong vibrations in dense, solid, and liquid matter as to produce in them an expansive force which is almost infinite; besides, it has never been proved that undulations, communicated in this manner, can produce any expansion, or any change of volume whatever. Supposing all this possible, there remain other objections of very considerable magnitude; since the phenomena of heat arise from vibratory motion, and expansion is the consequence of elevation, and contraction of diminution, of temperature, in a gas the extent of the vibrations of the particles must be as the

$\sqrt[3]{}$ volume: when, therefore, by compression a gas is made to occupy a less volume, the extent of the vibrations is diminished, and cold ought to result from the operation; whereas heat is actually excited. To this it may be replied, that the motion which is lost by the gaseous particles is communicated to those of the solid matter which is immediately in contact with the gas so compressed, and produces in it the phenomena of heat: this will be opposed by another experiment: Mix one volume of dry oxygen with two of dry nitrous gas, they are reduced to $1\frac{1}{2}$ volume, and heat is excited; here there is a considerable loss of vibratory motion, and which can only arise from the impact of the dissimilar particles, by which they mutually destroy in part each other's motion; external matter, therefore, ought to be affected with the sensation of cold. By similar reasoning it may be proved that the mixture of snow and muriate of lime should excite heat; whereas it is a powerful frigorific mixture.

This hypothesis having been principally supported by some experiments made by the late Count Rumford; and since there appear several sources of fallacy in them, they will require rather minute examination; they are fully described in the *Philoso-*

phical Transactions for 1798, Part I. p. 86. Having noticed the great degree of heat which is excited by turning or boring large masses of metal, this philosopher was led to inquire, "Is the heat furnished by the metallic chips which are separated by the borer from the solid mass of metal? If this be the case, then, according to the modern doctrines of latent heat and of caloric, the capacity for heat of the parts so reduced to chips ought not only to be changed, but the change undergone by them should be sufficient to account for all the heat produced; but the capacity is not changed. From hence it is evident that the heat produced could not possibly have been furnished at the expense of the latent heat of the chips."

In order to try the experiment in the most satisfactory manner, a cylindrical hole, 3·7 inches in diameter and 7·2 in depth, was bored in a cylinder of brass $7\frac{1}{2}$ inches in diameter, and $9\frac{9}{10}$ long; this cylinder was turned by means of powerful machinery upon its axis; while a solid blunt steel cylinder, having nearly the same diameter as the hole, was pressed upon the bottom of it, by means of a screw, with a force equal to about 10,000 lb. avoirdupois. The cylinder soon became very hot, and the heat continued without diminution during the whole time of the experiment. The cylinder was next enclosed in a wooden box filled with water, the content of which was $2\frac{1}{4}$ wine gallons; the water was prevented entering the cylinder, by means of a tight collar of leather; in $2\frac{1}{2}$ hours the water was in a state of ebullition; and after the experiment was concluded, between 4000 and 5000 grs. of brass dust were worn off by the friction. He now adds, "In reasoning upon this subject, we must not forget to consider the most remarkable circumstance, that the source of heat generated by friction in these experiments appears evidently to be inexhaustible. It is hardly necessary to add, that any thing which an insulated body or system of bodies can continue to furnish without limitation, cannot possibly be a material substance; and it appears to me to be extremely difficult, if not impossible, to form any distinct idea of any thing capable of being excited and communicated in the manner heat was excited and communicated in these experiments, except it be motion." In the commencement of this reasoning, an assumption is made, which has been particularly unfortunate for science; viz. that if heat being an elastic fluid be evolved by the compression of solid matter, the capacity of that solid for heat must be diminished in proportion to the quantity which has been separated. The whole quantity of heat contained in the solid is doubtless diminished, but why is the capacity to be changed? Now the specific or latent heat of a body is the whole quantity of caloric which it contains, at any given temperature, and the capacity for heat is measured by the quantity that is absorbed or evolved during the passage of the body from any one to any other given temperature, very near to each other. It, therefore,

follows that at any temperature the specific heat of a mass of matter is made up of the sum of all its several capacities from the point of absolute cold, or the true zero, to the temperature at which it is examined. Let, therefore, this range of temperature be divided into an indefinitely great number of equal intervals; or let the interval between the point of greatest density of the solid and its density at any given temperature be similarly divided; and let the quantities of heat absorbed or evolved while the body passes through these intervals be c , c' , c'' , &c. and let s represent the specific heat; then $c + c' + c'' + c''' + \&c. = s$. Now if $c = c' = c'' = c''' = \&c.$ in what degree soever s is diminished, the heat is evolved (the quantity evolved at each interval being denoted by c), still no change takes place in the capacity. If $c'' > c' > c$, then by reduction of temperature, at each interval a quantity of heat = c'' , or c' , or c , &c. will be evolved, and the diminution of capacity at each successive interval will be $c'' - c'$, $c' - c$, &c.; but if $c > c' > c''$, &c. then by diminishing s , the capacity will increase, the heat is evolved. It may now with propriety be asked, what experiments have been made upon the capacities of the metals for heat that decisively prove that the capacity of the same metal changes in the least sensible degree under the different circumstances of temperature or pressure while it retains its solid state? Or can we expect a sensible difference to be perceived? The best experiments hitherto made differ too much from each other to show, with any degree of certainty, such small differences, which are less than the probable and almost certain errors of the experiment; so that we have no proof from experiment of any such change; nor has any principle yet been developed which shows whether the capacities of the same metal under different circumstances may not be all equal, or even greater, at a low than a high temperature, or after than before compression. That the quantity of heat evolved in this experiment was great cannot be disputed, yet it was by no means sufficient to warrant the conclusions that have been drawn. The ignition of soft iron by hammering demonstrates that caloric exists in the metals in a state very little, if at all, inferior to that of ignition, and though an apparently great quantity may be thus evolved, it may, and most probably does, bear a very small proportion to the whole quantity contained in the metal, as will subsequently be demonstrated; besides, we do not know how small a quantity of caloric is sufficient to boil the quantity of water employed in this experiment; there is every reason to suppose it much less than is generally imagined; and in these experiments, a very large mass of metal was submitted to an excessive pressure, and of the mass, fresh strata were continually exposed to the compression by the wearing off of the brass: hence a definite quantity of heat was separated from each stratum in succession. Now if we admit the existence of caloric in a state of great density in

the metals, this cause would be quite adequate to the production of the observed effect. Besides, had the brass dust lost any of its capacity for heat by the operation, this must have been very nearly, if not entirely, restored by the subsequent treatment; for after it had been separated from the solid metal by friction, it was exposed for a considerable length of time to the extreme heat of the interior of the cylinder, and was afterwards heated, in order to ascertain its capacity: by which means it is well known its specific gravity and capacity for heat would be restored, at least in a very considerable degree. The greatest error appears to be the assumption that the source of the heat thus generated is inexhaustible; the quantity that can be thus excited is finite. Let the end of a thin bar of soft iron be submitted to repeated blows of the hammer, it soon becomes ignited; let the operation be continued, the heat gradually abates, and soon ceases entirely, after which it cannot be brought into an ignited state by the same process without being previously exposed to the fire. This is a process decidedly the same as that of Count Rumford in its principle, but entirely opposes the conclusions which he drew from his own experiment; he would have doubtless come to the same conclusion had his experiments been continued so long that the whole mass of metal employed had been brought to the greatest density, to which the friction used could have brought it; but since this was not the case, it is evident that the whole quantity of heat which the metal used could have furnished was not obtained.

On account of the complexity of this hypothesis, its incapability of explaining the phenomena of nature, and the impossibility of such motions taking place in consequence of the operation of those forces which are known to be inherent in matter, I shall in the succeeding papers assume caloric to be an elastic fluid, and from its effect joined to that of the centripetal force which Newton has demonstrated to exist, explain most of the primary laws of chemical and corpuscular action.

(*To be continued.*)

ARTICLE IV.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

Dr. Wollaston has been elected President of the Society, *ad interim*.

June 29.—A paper by Dr. Wollaston was read, entitled, "On sounds inaudible by certain Ears." The author, after some introductory remarks, proceeded to describe a peculiar

insensibility to certain sounds in persons not otherwise deaf, which he was led to observe by trying different modes of diminishing the sense of hearing in himself. He found, for example, that when the *membrana tympani* was thrown into a state of tension by external pressure by closing the nose and mouth, and expanding the chest, the ear was rendered insensible to grave tones, while its sensibility to sharper sounds was not affected. In this case all sounds below F in the bass cliff were inaudible.

In the healthy state of the ear, the power of discerning low sounds is great, and of uncertain limits; but if we attend to the opposite extremity of the scale of audible sounds, and with a series of small pipes exceeding each other in sharpness observe their effects successively upon the ears of different persons, we shall find considerable difference in their powers of hearing them, and see reason to infer that the sense of hearing in man is more limited than has been supposed. The author's attention was particularly called to this circumstance by observing a person insensible to the sound of a small organ pipe, which, with respect to acuteness, was far within the limits of his own hearing. By subsequent examination, it was found that this person's hearing terminated at a note four octaves above the middle E of the piano-forte. Other instances were then referred to of the insensibility of certain persons to various acute sounds; such as the chirping of the grasshopper, cricket, and sparrow, and especially the squeak of the bat, the existence of which is unknown to many individuals from its being inaudible to them. The pitch of this sound was stated to be about five octaves above the middle E before mentioned. The author fixed the limits of his own hearing at six octaves above the same note, and stated that from numerous trials he was induced to think that the interval of a single note between two sounds was sufficient to render the higher note inaudible.

The range of human hearing includes upwards of nine octaves, the whole of which are distinctly audible to most ears, although the vibrations of the acuter sounds are 600 or 700 times more frequent than those of the lower; and the author concluded by observing that it is very probable that other animals are so organized as to be able to distinguish sounds still more acute, and of course inaudible by human ears, and thus to possess what may be considered as a new sense.

At this meeting a paper was read by the Rev. F. H. Wollaston, on the Measurement of Snowdon by the Thermometrical Barometer. After some preliminary observations, the author stated, that, as obtained by the thermometrical barometer when the proper corrections were made, the height of this mountain above the N.E. end of Caernarvon Quay was 3546.25 feet. Gen. Roy found it trigonometrically to be 3555.4 feet, and by the barometer, 3548.9. An opportunity was also taken of comparing the height of another mountain as taken by the thermometrical

barometer with the determination of Gen. Roy. In this case some discrepancies were observed, which the author referred to the difficulty of ascertaining the stations on account of the indeterminate form of the hill.

At this meeting there was likewise read a short paper, entitled, "Further Particulars respecting the Anatomy of the Dugong," by Sir E. Home. This paper was intended as a supplement to Sir Stamford Raffles's account of that animal, and contained a few additional remarks.

The titles of the following papers were read :

"On the Compressibility of Water," by Jacob Perkins, Esq.

"A Sketch of an Analysis and Notation applicable to the Estimation of the Value of Life Contingencies," by Benj. Gompertz, Esq.

"An Inquiry into the Anatomy of certain Parts of the Globe of the Eye, upon the Structure of which Anatomists have not agreed in Opinion," by Arthur Jacob, M.D.

"Various Astronomical Observations, accompanied by some Tables," by Stephen Groombridge, Esq.

"A Letter from M. Stocklar, For. M.R.S. to Dr. Young, on some General Expressions for the Limits of Functions."

The Society adjourned till November.

GEOLOGICAL SOCIETY.

June 16.—A paper entitled "Additional Remarks on Glen Tilt," by Dr. M'Culloch, M.G.S. was read.

A remarkable variety of limestone occurs among the micaceous schist and gneiss which form the northern boundary of the Tilt, and near to its junction with the Garry. It is of a laminated structure, each lamina of calcareous matter being separated from the next by one of mica, and rarely exceeding the 20th of an inch in thickness. The laminæ are perfectly flat and parallel.

A more complete examination of the beds of yellow marble described at p. 39 of the third volume of the Geological Transactions, has shown that the predominant gneiss is of a white colour intersected by numerous reticulating veins of a pale ochry yellow.

The white marble, mentioned at p. 50, has since been found in its natural place between Tsalair and the Ward of Glen Fernat; another bed of the same nature exists in the hills that bound the Tilt to the southward. A bed of white marble similar to that at Gow's Bridge has been discovered in the hills that form the south eastern boundary of the valley at a great distance from the former, and with an immense series of quartz rocks interposed.

Gneiss has not been observed among the strata lying upon the granite of the southern side of the valley, but in the hills at the northern side it occurs in considerable abundance, together with quartz rock, micaceous schist, and varieties of hornblende schist.

Several additional varieties of tremolite have been discovered. This mineral is occasionally so intermixed with the sahlite, that it appears not impossible that the one graduates into the other. Wherever Dr. M'Culloch has observed sahlite in Scotland there tremolite has also been found.

Crystallized felspar occurs in veins in the micaceous schist on the hills that form the southern boundary of the Tilt. Adularia is sometimes found in the same cavities.

Mica, which is not often met with in Scotland in a detached and crystallized state, occurs, however, in the hills so often mentioned, in three distinct forms of crystallization. It is found imbedded in porphyry, in the form of regular hexagonal prisms, about the tenth of an inch in diameter and length, of a black colour, and with the flat surfaces of the prisms all lying in parallel planes. This parallelism is conformable to a fossile tendency of the including rock, which is also parallel to the sides of the veins, even where it is at right angles with the surrounding strata.

Chlorite is found in irregular nodules in cavities of micaceous schist, or occupying similar situations in a blue schistose limestone, or like the mica above-mentioned, it is met with in cavities in micaceous schist, occasionally accompanied by crystallized felspar.

ARTICLE V.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Fibrous Zeolite.*

Professor Freyssmuth subjected some fine specimens of Werner's fibrous zeolite, recognized to be so by Werner himself, to a careful chemical analysis. The specific gravity was 2.284. This is the mineral to which Prof. Fuchs has given the name of *mesolite*. He states its specific gravity to be 2.333. The constituents, according to Freyssmuth's analysis, are as follows :

Silica	44.562
Alumina	27.562
Lime	7.087
Soda	7.688
Water	14.125
Oxide of iron	Trace

101.024

Professor Freyssmuth considers the chemical constitution of this mineral to be as follows: $3 \text{ A S} + (\frac{1}{2} \text{ S o} + \frac{1}{2} \text{ C}) \text{ S}^3 + 3 \text{ A q}$; that is to say, three atoms of silicate of alumina, one atom of trisilicate of soda-and-lime, and three atoms of water.

II. *Aereolite.*

The following analysis of a meteoric stone which fell at Jonzac on June 13, 1819, has been made by M. Laugier :

Oxide of iron	36·0
Silica	46·0
Alumina	6·0
Lime.	7·5
Oxide of manganese	2·8
Magnesia.	1·6
Sulphur	1·5
Chrome	1·0
	102·4

The increase of weight is attributed to the oxidizement of the metals during the process of analyzing.

The author observes that the above stone is not only remarkable from the absence of nickel, but also for the proportions in which its other constituents exist, the quantity of sulphur and magnesia being much less than usual, and that of the alumina and lime greater. The author is disposed to believe, from the experiments hitherto made, that the presence of chrome rather than of nickel is to be considered as characteristic of meteoric stones.—(*Annales de Chimie et de Physique*, xiii. 441.)

III. *Curious Atmospheric Phenomenon.*

The following account of a very rare appearance has been taken from the *Cambridge Chronicle* as a circumstance worthy of being recorded :

“ One of those very singular and curious atmospheric phenomena which are occasionally seen among the Hartz mountains in Hanover, and have once or twice been observed on Souter Fell, in Cumberland, has been seen in Huntingdonshire. About half-past four o'clock on Sunday morning, July 16, the sun was shining in a cloudless sky, and the light vapours arising from the river Ouze were hovering over a little hill near St. Neot's, when suddenly the village of Great Paxton, its farm houses, barns, dispersed cottages, trees, and its different grass fields, were clearly and distinctly visible in a beautiful aerial picture, which extended from east to west about 400 yards. Nothing could exceed the astonishment and admiration of the spectator as he looked at this surprising phenomenon from a gentle declivity in an opposite direction at the distance of half a mile, or his regret at its disappearance in about 10 minutes.”

IV. *Compressibility of Water.*

Mr. Perkins, the ingenious inventor of the siderographic art, or of multiplying engraved impressions by transferring them from

a hardened steel plate to soft steel, or copper, is said to have placed beyond a doubt the most important fact that water submitted to a pressure of 326 atmospheres is diminished in bulk about $\frac{1}{9}$ th, or $3\frac{1}{3}$ per cent.

V. *Pure Potash.* By Dr. Prout.

Many practical chemists are doubtless aware of a fact which ought to be known generally; viz. that what is usually considered as *pure potash* almost always contains a minute quantity of some metal. This may be rendered sensible by passing a current of sulphuretted hydrogen through a solution of such potash, the solution instantly assuming more or less of a dark colour. Some years ago I made a great number of experiments on this subject, and I never was able to procure a specimen of potash or soda which did not become coloured when treated as above. The metal (or rather oxide) present is probably, in most instances, silver (derived from the silver vessels in which it is prepared), but sometimes it appears to be lead, and sometimes iron, or copper. One or other of the latter is very likely to be present if the common subcarbonate of potash of commerce has been employed, which is frequently so very impure that I have seen it become quite black upon simple exposure to the fumes of sulphuretted hydrogen. The presence of these metals in the alkalies has doubtless often misled chemists by inducing unnatural appearances. I may mention one striking instance. Some years ago, Mr. Miers announced that he had discovered a gas having the property of precipitating potash and soda black.* This gas was a mixture containing sulphuretted hydrogen, and the precipitates arose from metallic impregnation in the alkaline solutions employed.

VI. *Urine of the Horse.* By the Same.

As the urine of the horse has been said to contain very little urea, I was induced lately to examine it with the view of determining that point. The result was, that it contained an unusually large proportion of that principle, so that, without being previously concentrated by evaporation, it yielded crystals of nitrate of urea very readily on the addition of nitric acid. This fact accounts, therefore, for the strong ammoniacal odour of stables. The urine which was subjected to the above experiment was nearly transparent, and of a pale amber colour. It was rather of a mucilaginous consistence, foamed a good deal on shaking, and passed through the filter with great difficulty. It was not sensibly acid or alkaline, and its specific gravity was 1029·3.

Those who wish to repeat this experiment should operate upon the urine while still warm, or, if this is not convenient, should put a few drops of some acid (the muriatic is preferable) into the vessel in which it is voided and intended to be kept. This, by

* See *Annals of Philosophy*, vol. iv. p. 191.

neutralizing the lime existing in the urine of this animal (sometimes in great abundance) prevents it from acting upon the urea and decomposing it.

VII. *Fluid of Hydrocephalus.* By the Same.

This fluid had been taken two days before I examined it from the head of a child, 10 years of age, who had died of the disease. It was colourless, and nearly transparent, possessing only the slight opacity of river water. Smell fresh, something like that of new whey. It very faintly reddened litmus paper, and restored the yellow colour of turmeric paper, which had been rendered brown by ammonia. Specific gravity, 1008·5.

Exposed to heat, it became opaque at a temperature between 160° and 170°, and the opacity was increased if nitric acid was added. The oxymuriate of mercury produced a slight opacity. The acetic acid and prussiate of potash a sensible precipitate.

On being submitted to analysis, it was found to consist of

Water	987·18
Albumen precipitated by nitric acid and heat	1·66
Substances soluble in alcohol, fatty adipocerosus matter, lactate of soda?	1·65

Substances soluble in Water.

Muriates of potash and soda.	6·80	} 9·51
Sulphate of soda, and some animal matter incoagulable by heat. . . .	2·71	

1000·00

This fluid has been examined by Dr. Bostock, whose conclusions do not differ much from the above. The following were his results :

Water.	986·0
Albumen	1·2
Uncoagulable matter	2·8
Salts, chiefly muriate of soda.	10·0

1000·0*

Hence this fluid is remarkable for the quantity of the muriates which it contains. Dr. Bostock thinks he has estimated the quantity of albumen rather too high, from not having used sufficient heat in drying it. The standard of exsiccation for animal substances which I have employed for many years is that of 212° in a vacuum with sulphuric acid.

VIII. *Education in France.*

M. Cuvier, in a late discussion in the Chamber of Deputies,

* See *Medico-Chirurg. Transactions*, iv. 73.

observed, that elementary instruction was for the most part given gratuitously, or at a very small expense in France. That "1,070,500 children learn at this moment to read and write in the minor schools under the care of 28,000 masters; and that 46,000 youths are admitted into the colleges, each paying a small tax of from 15 to 40 francs to the state."

IX. *Method of preserving Currants fresh till January or February.*

The following is taken from the Journal de Pharmacie for June. The method is not new, but it does not appear to be generally known. When the fruit is ripe, choose those bushes enjoying a southern aspect, and which are most convenient in their shape, and most loaded with fruit, and surround them with thick straw mats, so that they shall be completely sheltered from atmospheric cold and other changes. By this simple method it will be found that the fruit may be preserved quite fresh till after Christmas.

X. *Meteorological Observations at Cork.* By T. Holt, Esq.
(With a Plate. See CVI.)

DEAR SIR,

(To Dr. Thomson.)

Cork, June 16, 1820.

I TRANSMIT to you the meteorological scale for Cork for the last quarter of 1819. I am, dear Sir, with due respect,

Your very obedient humble servant,

THOMAS HOLT.

REMARKS.

OCTOBER.

1. Cloudy; showery.
- 2, 3. Bright; showery.
- 4, 5. Bright; dry.
- 6, 7. Cloudy; dry.
8. Cloudy; light showers.
- 9, 10. Heavy and protracted showers.
11. Fair; foggy evening.
12. Cloudy; rain.
- 13, 14, 15, 16, 17, 18. Bright; dry.
19. Misty; showers.
20. Bright; showery.
21. Heavy showers; gale.
22. Showery.
23. Cloudy; gale.
- 24, 25, 26, 27, 28, 29. Bright; frosty.
- 30, 31. Showery.

NOVEMBER.

1. Cloudy; showery.
2. Bright; dry.

3. Cloudy; dry.
- 4, 5. Cloudy; showery.
- 6, 7. Bright; showery.
8. Bright; frost; wind.
9. Fair; rainy evening.
- 10, 11, 12. Bright.
13. Cloudy; dry.
- 14, 15, 16. Cloudy; showery.
17. Bright; dry.
18. Cloudy; dry.
- 19, 20, 21. Heavy showers.
- 22, 23, 24. Fair; frost.
- 25, 26, 27. Showery.
28. Rainy.
29. Fair; rainy night.
30. Cloudy; dry; rainy night.

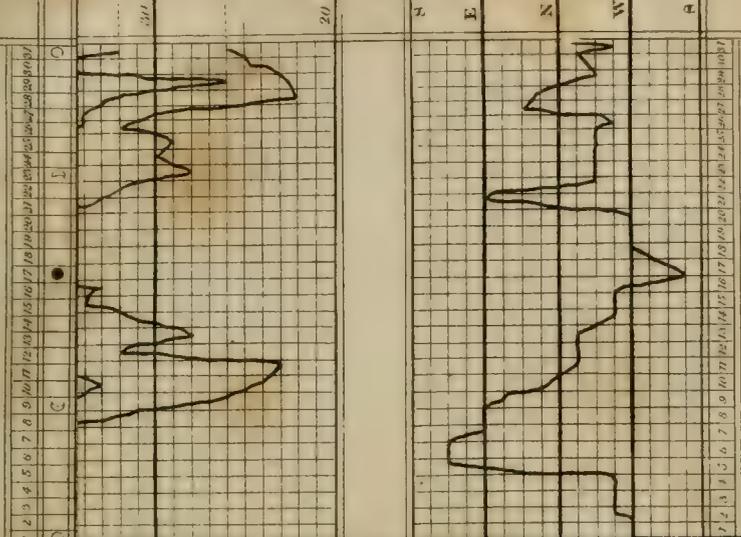
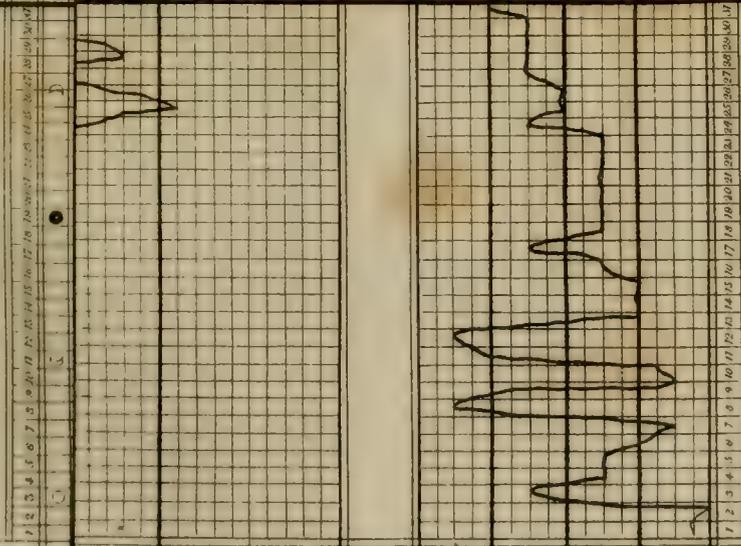
DECEMBER.

1. Fair; rainy night; gale.
2. Showery; ditto.
3. Fair; ditto.
4. Cloudy; light showers.

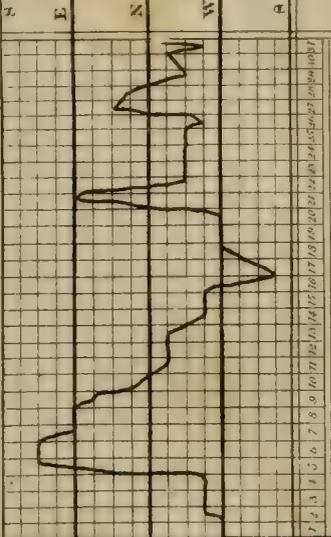
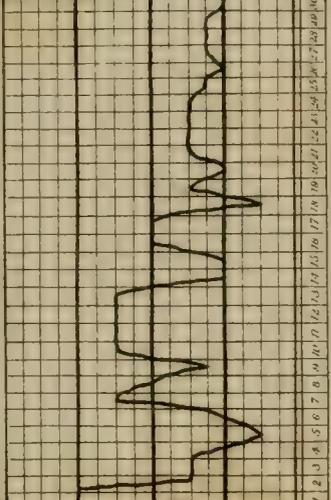
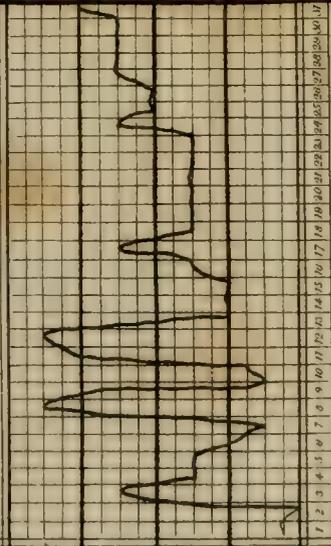
October

November

December



Wind



Highest rain

Lowest rain

5, 6. Cloudy ; dry.
 7. Showery.
 8. Rain ; wind.
 9, 10, 11, 12, 13. Fair ; dry.
 14. Showery.
 15. Fair.
 16, 17. Rain.

18, 19. Bright.
 20. Cloudy.
 21, 22, 23. Rain.
 24, 25, 26. Cloudy ; dry.
 27. Snow.
 28, 29. Fair ; frosty.
 30, 31. Fair ; thaw.

RAIN.

1819.	Inches.	1819.	Inches.	1819.	Inches.
Oct. 1	0.402	Nov. 1	0.084	Dec. 1	0.552
3	0.072	5	0.084	2	0.206
9	0.192	9	0.158	4	0.036
10	1.014	15	0.162	7	0.120
12	0.240	16	0.060	8	0.264
19	0.060	19	0.144	14	0.144
20	0.132	20	0.288	16	0.024
21	0.072	21	0.256	17	1.068
22	0.012	26	0.312	21	0.402
30	0.036	27	0.174	22	0.720
31	0.058	28	0.408	23	0.136
		29	0.912	27	1.584
		30	0.384		
Oct.	2.290				5.256
Nov.	3.426				
Dec.	5.256		3.426		
	11.972				

SUMMARY OF 1819.

Barometer : (Hill) Highest point, Oct. 16. Wind, N.W. 29.58 inches.
 Lowest, April 16. Wind, S.W. 27.77
 Mean of 730 observations 28.65
 (Town) Highest, Oct. 16. Wind, N.W. 30.58
 Lowest, Jan. 25. Wind, N. 28.87
 Mean of 730 observations 29.81
 Thermometer : Highest, Aug. 8. Wind, W.N.W. 86°
 Lowest, Dec. 28. Wind, N.N.E. 22
 Mean of 730 observations 49.6
 Rain in 1819 38.676 inches.

Wind.	Days.		Days.
E.	43	Bright.	151
W.	89	Dry ; cloudy	58
S.	18	Showery.	94
N.	56	Rainy.	25
S.E.	19	Variable.	37
S.W.	43		
N.E.	27		365
N.W.	59		
Var.	11		
	365		
Prevailing winds, N.W.			

ARTICLE VI.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

Dr. Elliotson is about to publish numerous Cases illustrative of the Efficacy of the Hydrocyanic or Prussic Acid in Affections of the Stomach, with a Report upon its Power in Pectoral and other Diseases in which it has been already recommended, and some Facts respecting the Necessity of varying the Doses of Medicines according to Circumstances, and the Use of Opium in Diabetes.

Mr. J. Zweed, of Bocking, will shortly publish, Popular Observations on Regimen and Diet, with Rules and Regulations in Regard to Health.

Mr. J. W. W. English, of Wellinborough, has in the Press, Medical and Surgical Remarks, including an effectual Method of removing Enlargements from the Throat, commonly called Wens.

Mr. W. Daniell has announced Sketches, on 48 quarto Plates, representing the native Tribes, Animals, and Scenery of Southern Africa, from Drawings made by the late Mr. S. Daniell.

Mr. Swainson is about to publish in monthly Numbers, Zoological Illustrations, or Original Figures and Descriptions of new, rare, or otherwise interesting Animals, from Lithographic and Copper-plates.

Mr. Joseph Swan, Surgeon to the Lincoln County Hospital, has in the Press, a Dissertation on the Treatment of Morbid Local Affection of Nerves, to which the Jacksonian Prize of the College of Surgeons was adjudged.

Mr. Belzoni announces his Intention to publish an Account of his late Discoveries in Egypt, Nubia, &c. His Work will contain Three Journeys in Upper Egypt and Nubia, one on the Coast of the Red Sea, and one to the Oasis of Ammon. It will be accompanied by a Volume of Plates in folio, containing Drawings of the various newly discovered Places, Figures, and Hieroglyphics, taken from the Originals found in the Tombs of the Kings lately discovered in Thebes, with an exact Imitation of Egyptian Costume, Colours, &c. with other Views in Nubia: the interior and exterior of the grand Temple of Ibsambul, near the second Cataract of the Nile, and other Views of that Country, and in Upper and Lower Egypt; the interior and exterior of the newly opened Pyramid: Plans and Topographic Maps, &c.

JUST PUBLISHED.

Cases of Serous Morbid Affection; principally incident to Females after Delivery, Abortion, &c. and arising from Uterine Hæmorrhage, &c. &c. By Marshall Hall, M.D. F.R.S.E. 8vo. 4s.

A Refutation of a Letter from Dr. Adam Neale to a Professor of Medicine, with a Statement of ulterior Proceedings to quiet the Minds of the Public, respecting Cheltenham Waters. By Thomas Jameson, M.D. 2s.

A Letter to the Editor of the Literary Gazette upon the Misrepresentations contained in a Pamphlet recently published by Dr. Neale upon the Subject of the Cheltenham Waters. By Thomas Newell, M.D. 8vo. 1s.

Cases in Surgery at the St. George's and St. James's Dispensary. By H. Jeffreys, Esq. 8vo. 8s.

FOREIGN.

Devèze, J. Traité de la Fièvre jaune. 8vo. 8s. 6d.

Histoire Naturelle des Lepidoptères, ou Papillons diurnes des Environs de Paris, 2ième Livr. 8vo. 5s.

Arcade, Philosophie Naturelle, où les Phénomènes Naturels sont expliqués par les lois de la Mécanique. 2 vols. 8vo. 16s.

D'Audebard de Ferussac *Histoire Naturelle générale et particulière des Mollusques terrestres et Fluviales* 6e. et 7e Livraisons in fol. et fig. col. chaque. 2l. 2s.

Dictionnaire des Sciences Medicales par une Societé de Médécins et de Chirugiens, vol. 44ème. Poet—Poum, 8vo. 10s.

ARTICLE VII.

NEW PATENTS.

John Hague, of Great Pearl-street, Spitalfields, engineer, for an improvement in preparing the materials for making pottery-ware, tiles, and bricks. June 2, 1820.

William Bate, Esq. of Peterborough, for a combination of, and additions to, machinery calculated to increase power. June 3.

William Bate, Esq. of Peterborough, for improvements in preparing hemp, flax, or other fibrous material, for spinning. June 3.

Simion Teissier, of Bucklersbury, London (late of Paris), for improvements in propelling vessels. Communicated to him by a foreigner residing abroad. June 3.

John Hague, of Great Pearl-street, Spitalfields, for certain improvements in the making and constructing of steam-engines. June 3.

Jacob Perkins, of Austin Friars, London, for improvements in the construction of fixed and portable pumps, such as pumps fixed for raising water from wells or ship-pumps, &c. &c. June 3.

John Wakefield, of Manchester, engineer, for improvements in the construction of furnaces for boilers of various descriptions, and in the mode of feeding the same with fuel, calculated to lessen the consumption of fuel, and to burn the smoke. June 6.

William Kendrick, of Birmingham, chemist, for the manufacture of a liquid from materials now considered useless for that purpose, and the application of the same liquid to the tanning of hides, and other articles requiring such process. June 6.

Jonathan Brownell, of Sheffield, table knife cutler, for better securing the blades of table knives and forks in the handles, by means of caps being soldered upon the tangs, whether of iron, steel, or other material, after the handles are upon them. June 8.

Samuel Parker, of Argyle-street, Bronzish, for an improved lamp. June 15.

William Erskine Cochrane, Esq. of Somerset-street, for an improvement in the construction of lamps. June 17.

Joseph Wollams, of Wells, Somersetshire, land-agent, for improvements in the teeth or cogs formed on, or applied to, wheels, pinions, and other mechanical agents for communicating or restraining motion. June 20.

John Butler Lodge and John Billeston, jun. of the Strand, truss-makers, for improvements in the construction and application of spring trusses, or bandages, for the cure of hernia. June 20.

John Vallance, of Brighton, brewer, for a method and apparatus for freeing rooms and buildings from the distressing heat sometimes experienced in them, and of keeping them constantly cool, or of a pleasant temperature, whether they are crowded to excess or empty, and also whether the weather be hot or cold. June 20.

John Vallance, of Brighton, brewer, for a method and apparatus for packing and preserving hops. June 20.

John Shaw, of Mary-street, Fitzroy-square, watchmaker, for a new method of making bricks by machinery. June 21.

James Harcourt, of Birmingham, brass-founder, for an improvement in castors applicable to tables and other articles. June 21.

ARTICLE VIII.

Magnetical and Meteorological Observations.

By Col. Beaufoy, F.R.S.

*Bushey Heath, near Stanmore.*Latitude $51^{\circ} 37' 44.27''$ North. Longitude West in time $1^{\circ} 20.93''$.*Magnetical Observations, 1820. — Variation West.*

Month.	Morning Observ.			Noon Observ.			Evening Observ.		
	Hour.	Variation.		Hour.	Variation.		Hour.	Variation.	
June 1	8 ^h 40'	24° 31'	32''	1 ^h 15'	24° 40'	42''	— ^h —'	—° —'	—''
2	8 35	24 31	01	1 25	24 39	24	7 45	24 32	06
3	8 35	24 29	54	1 15	24 39	25	7 40	24 33	22
4	8 40	24 28	10	1 15	24 39	19	7 40	24 34	11
5	8 35	24 30	40	1 30	24 39	33	— —	— —	—
6	8 40	24 29	44	1 35	24 39	27	7 45	24 33	53
7	8 35	24 33	00	1 35	24 37	55	7 40	24 33	57
8	8 40	24 31	13	1 25	24 40	02	7 30	24 33	16
9	8 40	24 29	37	1 55	24 35	20	7 55	24 33	32
10	8 40	24 30	55	1 10	24 39	35	7 55	24 34	06
11	8 50	24 29	57	1 25	24 41	38	7 55	24 34	21
12	8 10	24 28	44	— —	— —	—	— —	— —	—
13	8 35	24 30	15	1 40	24 38	12	— —	— —	—
14	8 40	24 28	59	1 15	24 41	12	— —	— —	—
15	8 35	24 29	54	1 20	24 38	29	7 50	24 33	41
16	8 40	24 29	50	1 30	24 38	54	7 45	24 33	37
17	8 40	24 30	14	1 20	24 39	46	7 50	24 33	41
18	8 40	24 29	26	1 35	24 38	20	7 50	24 32	23
19	8 35	24 28	17	— —	— —	—	7 50	24 34	30
20	8 40	24 28	51	1 25	24 36	26	7 50	24 34	58
21	8 35	24 31	44	1 20	24 40	08	7 55	24 34	46
22	8 40	24 30	17	— —	— —	—	— —	— —	—
23	8 05	24 28	32	— —	— —	—	7 55	24 35	21
24	8 35	24 29	40	1 20	24 40	35	7 55	24 32	57
25	8 45	24 30	15	1 30	24 38	06	— —	— —	—
26	8 40	24 29	44	1 20	24 39	43	7 35	24 34	27
27	8 35	24 28	15	1 25	24 38	22	7 25	24 32	45
28	8 40	24 28	58	1 25	24 39	53	7 25	24 34	21
29	8 40	24 27	50	1 25	24 39	30	7 55	24 32	41
30	8 45	24 29	25	1 25	24 40	37	7 45	24 34	36
Mean for the Month.	8 37	24 29 50		1 22	24 39 16		7 45	24 33 48	

Meteorological Observations.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
		Inches.				Feet.		
June	Morn....	29·069	51°	59°	W by S		Fine	42°
	1	Noon....	29·068	57	55	WSW	Stormy	57
		Even....	—	—	—	—	—	46
		Morn....	29·089	52	64	W by S	Rain	46
	2	Noon....	29·109	55	63	SW by W	Showery	57
		Even....	29·175	48	72	W	Hail,rain	43
		Morn....	29·275	49	76	W by N	Rain	43
	3	Noon....	29·292	56	61	WNW	Fine	60½
		Even....	29·355	51	66	NW	Showery	46
		Morn....	29·409	50	69	NW	Showery	46
	4	Noon....	29·425	52	65	WNW	Showery	52½
		Even....	29·447	48	70	WNW	Showery	41
		Morn....	29·500	52	65	W by N	Fine	41
	5	Noon....	29·513	60	59	WSW	Fine	63½
		Even....	29·466	—	69	WNW	Rain	47½
		Morn....	29·370	50	85	N by W	Rain	47½
	6	Noon....	29·488	55	73	SE by E	Cloudy	56
		Even....	29·628	50	73	NNE	Fine	44½
	Morn....	29·602	53	66	W	Cloudy	44½	
7	Noon....	29·549	60	60	W by N	Cloudy	61⅔	
	Even....	29·510	53	64	NW by N	Cloudy	47	
	Morn....	29·500	52	70	NW	Sm. rain	47	
8	Noon....	29·483	58	63	NW	Cloudy	61⅔	
	Even....	29·458	56	65	NW by W	Rain	52	
	Morn....	29·346	54	76	NW	Cloudy	52	
9	Noon....	29·315	58	68	NW	Showery	59⅔	
	Even....	29·275	51	65	WNW	Rain	42	
	Morn....	29·269	50	67	WNW	Fine	42	
10	Noon....	29·273	54	63	N	Rain	57	
	Even....	29·250	51	61	W by N	Very fine	41	
	Morn....	29·124	50	63	W by N	Cloudy	41	
11	Noon....	29·088	50	72	WSW	Rain	52	
	Even....	29·090	50	—	SW by S	Rain	48	
	Morn....	29·249	51	79	NE	Cloudy	48	
12	Noon....	—	—	—	—	—	61	
	Even....	—	—	—	—	—	43	
	Morn....	29·422	54	69	NNE	Cloudy	43	
13	Noon....	29·406	59	62	NNE	Thun. rn.	62	
	Even....	29·412	—	—	E	Hardrain	46½	
	Morn....	29·600	52	73	NNE	Rain	46½	
14	Noon....	29·619	55	65	NW	Cloudy	57⅓	
	Even....	—	—	—	—	—	48	
	Morn....	29·362	53	79	NW by W	Rain	48	
15	Noon....	29·407	56	75	NNW	Cloudy	58⅔	
	Even....	29·483	53	72	N	Fine	46	
	Morn....	29·500	54	69	NW	Fine	46	
16	Noon....	29·500	60	61	NW	Showery	60	
	Even....	29·492	55	67	W	Cloudy	51½	
	Morn....	29·502	55	69	NW	Fine	51½	
17	Noon....	29·517	63	55	WNW	Fine	66	
	Even....	29·530	53	64	W by N	Showery	49	
	Morn....	29·603	56	64	WNW	Fine	49	
18	Noon....	29·585	62	55	W	Cloudy	64⅔	
	Even....	29·490	58	63	SW	Cloudy	64⅔	

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
		Inches.				Feet.		
June	Morn....	29.277	56°	77°	WNW		Fine	52
	Noon....	—	—	—	—		—	63
19	Even....	29.288	54	58	WSW		Fine	} 49½
	Morn....	29.178	52	74	W		Rain	
20	Noon....	29.268	56	66	WNW		Showery	} 59½
	Even....	29.343	55	72	NNW		Showery	
21	Morn....	29.410	56	67	NW		Fine	} 46½
	Noon....	29.469	61	62	WNW		Cloudy	
22	Even....	29.500	57	67	Calm		Fine	} 62½
	Morn....	29.602	59	68	SW by S		Fine	
23	Noon....	—	—	—	—		—	} 51½
	Even....	—	—	—	—		—	
24	Morn....	29.679	58	74	W		Very fine	} 67½
	Noon....	—	—	—	—		—	
25	Even....	29.712	68	63	W		Very fine	} 72½
	Morn....	29.768	68	63	SSE		Clear	
26	Noon....	29.804	76	52	Var.		Clear	} 58
	Even....	29.841	70	56	E		Clear	
27	Morn....	29.915	72	64	NNW		Clear	} 63
	Noon....	29.940	81	50	Var.		Clear	
28	Even....	29.933	—	49	Var.		Clear	} 83
	Morn....	29.973	76	61	NNE		Clear	
29	Noon....	29.987	84	49	Var.		Clear	} 65½
	Even....	29.971	78	54	NE		Clear	
30	Morn....	29.963	76	62	NE		Clear	} 65
	Noon....	29.975	84	52	ENE		Very fine	
1	Even....	29.955	80	53	E by N		Clear	} 85½
	Morn....	29.900	76	63	W		Clear	
2	Noon....	29.868	84	45	WNW		Clear	} 67½
	Even....	29.811	79	57	E by N		Clear	
3	Morn....	29.725	63	83	ESE		Clear	} 85½
	Noon....	29.688	79	66	ESE		Showery	
4	Even....	29.603	66	69	ESE		Hazy	} 59
	Morn....	29.514	63	76	NE		Thunder	
5	Noon....	29.514	68	67	E		Thun. rn.	} 76½
	Even....	29.590	56	68	NE		Cloudy	
6	Morn....	29.514	63	76	NE		Cloudy	} 56
	Noon....	29.514	68	67	E		Cloudy	
7	Even....	29.590	56	68	NE		Cloudy	} 69

Rain, by the pluviometer, between noon the 1st of June, and noon the 1st of July, 1.724 inch. Evaporation, during the same period, 4.391 inches.

ARTICLE IX.

METEOROLOGICAL TABLE.

1820.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.	Hygr. at 9 a. m.
		Max.	Min.	Max.	Min.			
6th Mo.								
June 1	W	29·60	29·57	62	47			71
2	W	29·79	29·60	65	42		33	71
3	N W	29·91	29·79	63	46		11	81
4	N W	30·01	29·91	59	38	44	02	75
5	N W	30·01	29·88	68	48		33	74
6	N W	30·12	30·01	61	38		01	84
7	N W	30·10	30·02	61	43			77
8	N W	30·02	29·88	64	52	39	01	76
9	N W	29·88	29·81	61	40		03	79
10	N W	29·81	29·66	62	42			75
11	S W	29·77	29·61	57	48	28	08	72
12	N E	29·92	29·77	64	34			88
13	N W	30·12	29·92	56	46		30	77
14	N	30·12	29·90	61	48		32	82
15	N W	30·01	29·90	64	40	30	02	92
16	N W	30·02	30·00	65	48			78
17	N W	30·09	30·02	67	48			84
18	N W	30·10	29·79	69	51		14	72
19	N W	29·79	29·72	68	50	50	02	80
20	N W	29·91	29·72	61	42		60	99
21	N W	30·11	29·91	67	41			77
22	S W	30·18	30·11	74	50			76
23	N W	30·24	30·12	81	47	42		79
24	N E	30·36	30·24	89	51			78
25	N W	30·40	30·36	91	60	35		67
26	N W	30·41	30·36	88	55			70
27	Var.	30·36	30·31	88	56	38		69
28	N W	30·31	30·21	88	56		18	68
29	S E	30·21	29·99	74	53		04	89
30	S E	30·23	30·21	70	49	38		86
		30·41	29·57	91	34	3·44	2·54	99—67

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Sixth Month.—1. Showery: cold: windy. 2. Showery: cold wind: a smart shower of hail about a quarter before two, p.m.: wind shifted to the NW in the afternoon. 3. Cloudy: cold. 4. Showery. 5. Cloudy. 6. Cloudy. 7. Cloudy. 8. Cloudy. 9. Cloudy. 10. *Cirrus: Cirrostratus: Cirrocumulus*. 11. Showery. 12. Fine. 13. A strong smell of electricity in the morning: heavy rain, accompanied by thunder in the afternoon. 14. Cloudy. 15. *Cirrocumulus: Nimbus: a Stratus* in the marshes at night. 16. *Cirrocumulus*: cloudy. 17. Fine. 18. Cloudy: fine. 19. Fine: cloudy. 20. Showery. 21. Fine: a very extensive *Stratus* on the marshes at night. 22. Fine: cloudy: very fine *Cirrocumuli* at night, with lunar halo and corona. 23. Clear: *Cirrus*: a *Stratus* on the marshes at night. 24. *Cirrus*: sultry: a thick fog in the morning. 25. Clear: sultry. 26. Sultry: *Cirrus*. 27. *Cirrus*: clear. 28. Fine. 29. Fine. 30. Fine.

RESULTS.

Winds: N, 1; NE, 2; SE, 2; SW, 2; W, 2; NW, 20; Var. 1.

Barometer: Mean height

For the month.	30.003 inches.
For the lunar period, ending the 2d.	29.800
For 13 days, ending the 4th (moon south)	29.679
For 14 days, ending the 18th (moon north)	29.937

Thermometer: Mean height

For the month.	57.950°
For the lunar period, ending the 2d.	55.327
For 30 days, the sun in Gemini.	54.583

Hygrometer: Mean for the month 78.2

Evaporation..... 3.44 inch.

Rain..... 2.54

ANNALS
OF
PHILOSOPHY.

SEPTEMBER, 1820.

ARTICLE I.

On the Specific Gravity of Gases. By T. Thomson, M.D. F.R.S.

IN the preceding volumes of the *Annals of Philosophy* I have more than once drawn the attention of my readers to this important subject. I have given two different tables of the specific gravity of gases constructed from the experiments which I considered to be the most accurate, and I have likewise inserted a table drawn up by M. Gay-Lussac, which I have reason to believe has been very generally followed by French chemical writers; indeed probably by the greater number of continental chemists. But the very great importance of determining these specific gravities with precision made me anxious to have it in my power to go over the experiments in my own laboratory. This has been fortunately accomplished chiefly in consequence of my removal to the university of Glasgow. The balance which I employed was sensible to the thousandth part of a grain, even when loaded with my weighing flask. Every attention was paid to the purity of the gases subjected to experiment; and to prevent inaccuracies from accidental and unforeseen sources every experiment was repeated at least three times; and, in general, we did not rest satisfied while a discrepancy existed between two experiments amounting to one thousandth part of the whole weight. I need not describe the method which I followed, as an account of it has been already given in a former volume of the *Annals of Philosophy*. It is simple and easy, and founded upon a well-known property of gases. It saves the trouble of the

numerous and intricate corrections, which Biot describes at such length in his *Traité de Physique*. It renders an absolute vacuum, or even a very good vacuum, unnecessary, and enables us to obtain a more correct result by a single experiment of five minutes' duration than Berzelius was able to accomplish by almost innumerable repetitions, when he endeavoured to determine the specific gravity of sulphurous acid gas.

It is proper to mention before going further that the weighings and the exhaustions of the flask were all conducted by my assistant Mr. Harvey, who indeed performed the whole of the experiments, and to whose accuracy and care I have been very often indebted in other chemical investigations.

Gay-Lussac first proved by a series of very satisfactory inductions that the gases unite with each other in certain definite proportions which may be denoted by very simple numbers. One volume of one gas either unites with one volume, or two volumes, or three volumes, or $1\frac{1}{2}$ volume of another gas. He showed too that a determinate change always takes place in the volume of the new formed gas. The volume sometimes remains unaltered. Thus one volume of chlorine gas and one volume of hydrogen gas, when united together, constitute two volumes of muriatic acid gas. More generally the volume is reduced to one half. Thus a volume of oxygen gas united to a volume of vapour of sulphur constitutes only one volume of sulphurous acid gas. In one particular case, the volume is reduced to one-fourth part of the original volume of the two constituents. Thus when one volume of hydrogen gas and one volume of vapour of carbon unite together so as to constitute olefiant gas, the bulk of the olefiant gas is not two volumes, nor even one volume, but only half a volume.

It is this remarkable general fact which gives such importance to the accurate determination of the specific gravity of the gases. We see from it that a simple relation exists between the specific gravity of the gases and the weights of their atoms. This relation is such that if we know the one we can very easily deduce the other. I have shown this relation in a paper which I published in a preceding volume of the *Annals of Philosophy*. Indeed I was led by my knowledge of it to conclude that many of the specific gravities of gases already determined by preceding experimenters, were inaccurate. I even went the length of adopting a set of hypothetical specific gravities of gases as the true ones in the last edition of my *System of Chemistry*. My object in the present paper is to give the result of a set of experiments made on purpose to ascertain how far these hypothetical numbers were borne out by actual trial.

There are four simple or undecomposed gases, the specific gravities of which it is important to determine in the first place; because it is from them we are enabled to deduce the verification

of Gay-Lussac's law, and to establish the ratios between the weights of their ultimate atoms. These four gases are *oxygen*, *azote*, *hydrogen*, and *chlorine*.

1. Oxygen and azote mixed together in a determinate proportion constitute atmospherical air. Now it was shown by Dr. Prout, in a former volume of the *Annals of Philosophy*, that if the specific gravity of air be 1.000, and if air be a mixture of four volumes of azotic and one volume of oxygen gases, then the specific gravity of these two gases is as follows :

Oxygen gas.	1.1111
Azotic gas.	0.9722

This hypothesis does not accord completely with the analysis of atmospherical air, as hitherto made by the most careful experiments. The present opinion entertained is that air is a mixture of 79 volumes of azotic and 21 volumes of oxygen gas. If air be a mixture of 80 volumes of azotic and 20 volumes of oxygen, then it follows that it consists of two atoms of azote and one atom of oxygen, probably in chemical combination. Now the very close approximation of the experimental result to 80 and 20 leaves little doubt in my mind that air is really a chemical compound of two atoms of azote and one atom of oxygen. When I attempted to analyze common air by abstracting its oxygen by means of a stick of phosphorus, I have never been able to satisfy myself, though I have repeated the experiment above 100 times, that the diminution of bulk is not exactly one-fifth of that of the whole air acted upon. But when 100 volumes of air are mixed with 42 or 44 volumes of hydrogen gas, and an electric spark passed through the mixture, the diminution of bulk always amounts to 63 volumes. Now as this diminution is occasioned by the formation of water, which is a compound of two volumes hydrogen and one volume oxygen, it is obvious that one-third of the diminution of bulk must be oxygen. This gives us 21 volumes for the bulk of oxygen contained in 100 volumes of air; but I think it very probable that when air is analysed by the combustion of hydrogen gas some nitrous acid, or ammonia, is always formed, which would of course diminish the quantity of azote, and render the residual volume less than it ought to be.

The specific gravities of oxygen and azotic gases taken with as much accuracy as possible in my laboratory were found to be as follows :

	Oxygen.
By first trial.	1.1117
By second trial.	1.1117
By third trial.	1.1117
	Azotic gas.
By first trial.	0.9730
By second trial.	0.9728
By third trial.	0.9728

Now the theoretical specific gravities are :

Oxygen	1.1111
Azotic	0.9722

Both our results are higher than theory indicates; oxygen by $\frac{6}{11111}$, or little more than $\frac{1}{1000}$ th part, and azotic gas by $\frac{6}{97220}$, or little more than $\frac{1}{1500}$ th part. Now, though every possible pains was taken to render these experiments as accurate as possible, I have not a doubt that both $\frac{1}{1000}$ th and $\frac{1}{1500}$ th are considerably within the limits of error. The gases were weighed in a glass flask capable of holding about 50 cubic inches; so that the weight of the gas subjected to experiment (supposing it the same as that of air) did not differ very much from 15 grs. The flask was first weighed with great care. It was then exhausted of air, and weighed again. It was then screwed to a gas holder, and filled with the gas whose specific gravity was required and weighed again. The specific gravity was obtained by dividing the weight of the gas thus ascertained by the weight of the same bulk of air previously taken out of the flask by the air-pump, and its weight determined by weighing. Now in each of these three processes a stop-cock was first screwed to the flask, and afterwards unscrewed from it. This stop-cock was furnished with a collar of leather which I was under the necessity of greasing in order to make air-tight. I was at pains after each unscrewing to wipe the brass part attached to the flask quite clean; but it is difficult to be sure that no alteration whatever is made in the absolute weight of the flask. An error of the thousandth part of a grain would be sufficient to occasion all the difference which exists between the numbers obtained by experiment and the theoretical numbers. Now I am afraid that there is scarcely any care that will be sufficient to prevent the chance of an error amounting to $\frac{1}{1000}$ th of a grain; a quantity which certainly does not exceed the millionth part of the whole weight of the flask and its contents.

But this is not the only source of error to which we are exposed. Unless the gases examined be absolutely pure, or unless we have the means of determining the absolute quantity of foreign gaseous matter mixed with the gas tried, it is obvious that the specific gravity of the gas which we obtain cannot be the true one. The azotic gas, the specific gravity of which was ascertained in my laboratory, was obtained by abstracting the oxygen gas from it by means of nitrous gas. Now had an excess of nitrous gas been added amounting to no more than $\frac{1}{300}$ th part of the azotic gas, or, in other words, had the azotic gas subjected to experiment been a mixture of 299 volumes of pure azote and one volume of nitrous gas, this small proportion of impurity would have been sufficient alone to account for the increase of specific gravity observable in our trials.

The specific gravity both of the oxygen and azotic gas, as

found by experiment, is somewhat higher than the theoretical specific gravity. Now this sufficiently shows that the experimental result is not absolutely correct. If air were a compound of 21 volumes of oxygen and 79 volumes of azotic gas instead of 20 volumes of the former and 80 of the latter as theory indicates, then the specific gravity both of oxygen and azotic gas would be somewhat less than the theoretical number; for the specific gravity of air is always unity. Therefore (supposing the specific gravity of oxygen = x , and that of azotic gas = y) $21x + 79y = 100$. Let us suppose the specific gravity of oxygen gas to be 1.110; then the equation becomes $21(1.11) + 79y = 100$ or $23.31 + 79y = 100$, from which it is easy to deduce $y = 0.9700$ nearly. It is clear from this, that if air be a mixture of 21 oxygen + 79 azote, the specific gravity obtained by experiment, supposing it absolutely correct, would have been below 1.1111 and 0.9722 instead of above it. Nobody who considers these facts can have any hesitation in concluding that there was an error in our experiments, and that the weight of each gas turned out a little higher than the truth; but the experiments were the most accurate that my apparatus would admit; and I am of opinion that they sufficiently confirm the specific gravities of oxygen and azote as pitched upon by Dr. Prout from theoretical considerations. Henceforth then chemists need be under no hesitation in fixing upon 1.1111 for the specific gravity of oxygen gas, and 0.9722 for the specific gravity of azotic gas.

2. Hydrogen is the lightest of all the known gases. I have satisfied myself by many experiments that carburetted hydrogen gas, the next in point of lightness to hydrogen, is very nearly eight times as heavy as this last gas. This great disparity between the weight of hydrogen and the other gases renders it much more difficult to determine its specific gravity than that of any of the other gases; because the least mixture of any of the heavier gases occasions a material difference in the result. Thus, for example, a mixture of 99 volumes of pure hydrogen and 1 volume of common air will have the specific gravity 0.0787; which is more than one-eighth part higher than the true result. A mixture of 1 volume of air with 199 volumes of pure hydrogen has the specific gravity of 0.074, which is the result obtained by Biot and Arrago. Hence I am disposed to suspect that the hydrogen gas upon which they experimented was in reality contaminated with $\frac{1}{20}$ th of its volume of common air. The presence of a little carburetted hydrogen gas would act precisely in the same manner, though not quite so powerfully as air. If the hydrogen gas examined by Biot and Arrago contained a mixture of one per cent. of carburetted hydrogen, its specific gravity would have been at least as high as they found it, or 0.074.

The necessity of operating upon extremely pure hydrogen gas, and the tediousness which necessarily attends all experiments which aim at precision, will account for the length of time which

elapsed before chemists acquired any precise notions respecting the true specific gravity of this gas. Mr. Cavendish, the first person who attempted to determine the specific gravity of hydrogen gas, found it 10·8 times lighter than common air, which is about 0·092.* Lavoisier, whose method of taking the specific gravity of gases was better than that of Mr. Cavendish, found the specific gravity of hydrogen gas only 0·0769, which is considerably below the result of his predecessor, though still above the truth.†

M.M. Biot and Arago made a careful set of experiments to determine the specific gravity of different gases, the results of which were published in the *Memoires de l'Institute* for 1806. They appear to have been at considerable pains in preparing their gases, and their experiments, as far as appears, were made with very great care. They found the specific gravity of hydrogen gas 0·0732. This is the weight of pure hydrogen gas, which has been generally adopted by chemists. Dr. Wollaston, for example, was decided by it in his paper on Chemical Equivalents, and in the construction of his sliding rule, which has proved of such utility in chemical researches; for he has considered the specific gravity of oxygen gas as 15 times greater than that of hydrogen gas, and has concluded in consequence that the atom of oxygen is 7½ times greater than that of hydrogen. Sir H. Davy has adopted the same specific gravity of hydrogen gas, and the same relation between the weight of the atom of oxygen and hydrogen (*Elements of Chemical Philosophy*, p. 112), swayed doubtless by the same authority; though, as he is in the habit of neglecting to state the sources of his information, the reader would be apt to conclude that he had himself determined the specific gravity of hydrogen gas, and found it to be the same as had been before stated by Biot and Arago. Indeed I should not wonder if Davy's statement were really derived from an original experiment of his own; for I myself took the specific gravity of hydrogen gas about 10 years ago, and obtained 0·073, a result coinciding so nearly with that of Biot and Arago that I considered it as quite unnecessary to publish any account of my experiment.

Thus it appears that every successive experimenter had found the specific gravity of hydrogen gas lower than his predecessor. This will appear at one glance from the following table in which the successive results are registered:

Specific gravity of hydrogen gas	0·092	Cavendish
	0·0769	Lavoisier
	0·0732	Biot and Arago.

Dr. Prout had the merit of discovering that even the experiments of Biot and Arago gave the specific gravity of hydrogen

* *Phil. Trans.* 1766, p. 141. † *Lavoisier's Elements*, p. 569, *Engl. Trans.*

gas above the truth and the sagacity to determine the true specific gravity without any original experiments of his own; but merely by a close and ingenious comparison of the experiments of others. This is a degree of skill that places its possessor in a more elevated rank than a mere experimenter, and induces me to prognosticate with confidence that if Dr. Prout persevere in the career which he has begun with so much ardour, the science of chemistry will be indebted to him for discoveries of a far higher and more important kind than have hitherto been made. Chemistry, for a long series of years, owed its progress to experimental industry alone. At that time mathematical reasoning could not be applied to it, and the utmost stretch of human sagacity could not prevent the merely speculative chemist from going altogether astray. It was then that activity and industry led to the greatest harvest of discovery, and that Dr. Priestley, without any mathematical knowledge whatever, and without any turn for complete investigation, contributed more to the improvement of chemistry than Mr. Cavendish himself, the most accurate experimenter, and the most sagacious reasoner, that Great Britain has produced, with the sole exception of Sir Isaac Newton; but the introduction of the atomic theory has put the science upon a very different footing. We now begin to have glimpses of the general laws which nature obeys in the union of elementary bodies. Henceforth the great object of chemists must be to discover these general laws; and no progress can be made in such investigations without the aid of analytical reasoning, and without a mind trained to mathematical speculations. Chemistry has not advanced far enough to enable us to make a single step without the aid of experiment; but neither can we make any progress in it of the least value unless we call in mathematics to our assistance. Now, therefore, is the period of the science, when such men as Dr. Prout and Mr. Herschel, may be certain of reaping a rich harvest of discovery and renown, if they choose only to devote to this delightful science the requisite degree of attention, and combine their experimental skill with their mathematical acquirements. Mere experimenters may relinquish the field; for there is not a great deal more which they can do, and to mere mathematicians chemistry at present, and probably for some time to come, is forbidden ground. Indeed to be convinced of the little utility of mere mathematical formulas towards promoting this science without the aid of experiment, the reader has only to peruse the chemical part of Biot's *Traité de Physique*, where he will find abundance of specimens of most elaborate mathematical investigations, which leave every subject precisely in the state in which they found it.

Dr. Prout demonstrated from the known composition and specific gravity of ammoniacal gas that the true specific gravity of hydrogen gas is 0.0694, or exactly $\frac{1}{16}$ th of that of oxygen

gas.* It deserves to be recorded, and the fact cannot but strike posterity with surprise, that though Dr. Prout's demonstration is complete, and though it has been before the public these five years, I am not aware of any British chemist who has adopted the true specific gravity of hydrogen gas, except myself. Every writer, either of elementary chemical books, or of experimental investigations (as far as my reading has gone), has uniformly contented himself with the specific gravity as determined by Biot and Arago. The same observation applies to the French chemists; but I have seen papers which have been published both in Germany and in the United States of America in which the true specific gravity of hydrogen gas, as determined by Dr. Prout, has been adopted.

The first persons who attempted to verify Dr. Prout's conclusion were Berzelius and Dulong. They found the specific gravity of hydrogen gas only 0.0688, or 0.0689.† As Berzelius gives no details of the way in which his experiments were made, we are unable to point out the reason why his gas was lighter than that which had been weighed by preceding experimenters. Those in my laboratory, of which I am going to give an account, were made before I had heard that the subject had attracted the attention of Berzelius; but I think it not unlikely that he preceded me in point of time.

The hydrogen which we used was obtained from zinc. In order to get rid of the impurities with which common zinc is always contaminated, and which from previous experiments there was reason to believe added somewhat to the weight of the hydrogen evolved, the zinc was distilled over by exposing it to a white heat in a stoneware retort. The requisite precautions were taken in employing pure water and pure sulphuric acid, and every particle of common air was carefully excluded. The specific gravity of hydrogen gas thus prepared was taken three different times. The following were the results obtained :

By first trial	0.06954
By second trial	0.06933
By third trial	0.06933

Now the mean of these three experiments gives us 0.06940, which is precisely the number already deduced by Dr. Prout, from the specific gravity of ammoniacal gas. I conceive then that there cannot be the least hesitation in adopting 0.0694 as the true specific gravity of pure hydrogen gas; consequently oxygen gas is precisely 16 times heavier than hydrogen gas, and if the weight of an atom of oxygen be reckoned unity, then that of hydrogen is 0.125; and $0.125 \times 8 = 1$; so that the atom of oxygen is a multiple of the atom of hydrogen.

* *Annals of Philosophy*, vi, 322.

† *Essai sur la Theorie des Proportions Chimiques*, p. 129.

We have $0.0694 \times 16 = 1.1111$, the sp. gr. of oxygen gas
 $0.0694 \times 14 = 0.9722$, the sp. gr. of azotic gas

3. The very disagreeable smell of chlorine and its corrosive properties prevented chemists for a good many years from attempting to ascertain the specific gravity of chlorine gas. Sir Humphry Davy, and Gay-Lussac and Thenard were as far as I know the first chemists who made direct experiments on the subject. The following were the results which they obtained :

Davy	2.395
Gay-Lussac	2.470

Dr. Prout, in the paper above referred to, has shown from the specific gravity of muriatic acid gas, and from the known fact that it is composed of one volume of chlorine gas and one volume of hydrogen gas united together without any change of bulk that the true specific gravity of chlorine gas must be 2.500. Thus we see that with regard to chlorine gas the very reverse has happened that took place with respect to hydrogen gas. Every succeeding experimenter found it heavier than his predecessor: nor is it difficult to see the reason of this: it is owing to the common air with which the chlorine had been mixed. The chlorine on which Davy made his experiments must have contained seven per cent. of common air; for, supposing the specific gravity of chlorine to be 2.5, and that of air to be 1, then the specific gravity of a mixture of 93 chlorine + 7 air would be 2.395, which is the number obtained by Davy. The chlorine gas examined by Gay-Lussac and Thenard was a mixture of 99 volumes of pure chlorine and 1 volume of common air; for the specific gravity of such a mixture would be 2.47. Being aware of the source of inaccuracy in preceding experiments, our business in the experiments made in my laboratory was to endeavour to procure chlorine gas perfectly pure. This was accomplished by preparing the gas in a pretty large quantity, and not beginning to collect it till it had passed over so long that all the common air was expelled. It is obvious, however, that this will not be accurately accomplished unless the apparatus is arranged in such a way that the air can make its escape at the upper extremity of the vessel in proportion as the chlorine occupies the lower portion of it. A good deal of practice, and every possible attention, enabled us to succeed completely. I have reason to believe that the chlorine whose specific gravity we took was perfectly pure. Three different trials gave the following results:

By first trial	2.500
By second trial	2.500
By third trial	2.500

Thus all these experiments exactly coincide with each other; and what is still more satisfactory, they are all the very specific

gravity previously determined by Dr. Prout from other considerations. When the results thus obtained in my laboratory are contrasted with a flippant and petulant criticism on this very specific gravity pitched upon by Dr. Prout, which appeared some years ago in a London periodical publication, they cannot but give birth to a train of reflections of rather a mortifying nature.

We may conclude then that the specific gravity of pure chlorine gas is 2.5, or exactly 36 times heavier than hydrogen gas.

Such are the results of the experiments made in my laboratory to determine the specific gravity of the four simple gases. Every possible attention was paid to insure the utmost degree of accuracy of which my apparatus was capable; and I am very much mistaken if they do not fully confirm the specific gravities previously determined by Dr. Prout from a most ingenious and profound train of reasoning. Let us contrast the theoretical and the experimental specific gravities of these four gases:

	Theory.	Exper.
Specific gravity of oxygen gas.	1.11111	1.1117
azotic gas	0.97222	0.9728
hydrogen gas	0.06944	0.0694
chlorine gas	2.50000	2.5000

The greatest deviations are in the case of oxygen and azote; but even the greatest of these does not exceed $\frac{1}{1500}$ th part, which I am persuaded is within the limits of unavoidable error. Indeed I have some doubts whether we are in possession of the means of obtaining azotic gas in a state of absolute purity.

2. Let us now examine the specific gravity of the gaseous compounds of the four simple gases. Here the general law discovered by Gay-Lussac will be of great service to us by furnishing us with a method of comparing our results with a correct standard. My readers are aware that Gay-Lussac's general law is this: "Gaseous bodies combine in certain very simple proportions reckoning by volume: 1 volume of one gas unites with either 1 volume, or 2 volumes, or 3 volumes, or $1\frac{1}{2}$ volume of another gas. The new compound, if gaseous, has either exactly the volume of the two constituent gases, or it loses a third, or the half or three quarters of that volume." I have made a great many experiments upon gaseous bodies with a view of satisfying myself whether this general law be entitled to confidence. The result has been the fullest conviction of its precision. I consider it as one of the most important general laws hitherto discovered in chemistry. Indeed I am of opinion that posterity will place it as the most important of all the discoveries for which we are indebted to Gay-Lussac, and that they will rank it among the most important discoveries of the present age.

1. We have two gaseous compounds of oxygen and azote; namely, 1, *protoxide of azote*, the gas which Davy distinguished

by the name of *gaseous oxide of azote*; and 2, *deutoxide of azote*, the gas which has been so long known by the epithet *nitrous gas*, a name first imposed upon it by Dr. Priestley.

I consider it as demonstrated that *protoxide of azote* is a compound of half a volume oxygen gas and one volume azotic gas condensed into one volume. Of course we obtain its specific gravity by adding to the specific gravity of azotic gas half the specific gravity of oxygen gas.

$$\begin{array}{r} \text{Specific gravity of azotic gas} \dots = 0.9722 \\ \text{Half specific gravity of oxygen gas} = 0.5555 \\ \hline 1.5277 \end{array}$$

So that the specific gravity of protoxide of azote by theory comes out 1.5278. A quantity of this gas was prepared in the usual manner from nitrate of ammonia, and great care was taken to exclude every portion of common air. Three different trials were made on the specific gravity of this gas. The results were as follows:

By first trial	1.5269
By second trial	1.5269
By third trial	1.5269

All these results agree perfectly with each other. They give the specific gravity of the gas not quite so much as $\frac{1}{1300}$ th part less than the theoretical result. Now this small difference being within the limits of unavoidable error, I consider myself entitled to conclude from the above experiments that the true specific gravity of protoxide of azote is 1.5278, as theory indicates. The mixture of rather less than $\frac{1}{300}$ th part of the volume of the gas of common air would have been sufficient to produce the alteration which we found in the specific gravity of our gas. In other words, if the gas which we weighed had been a mixture of 499 volumes of protoxide of azote and 1 volume of air, its specific gravity would have been only 1.5269, as we found it, allowing the specific gravity of pure protoxide of azote to be 1.5278. Now I am not aware of any good method of detecting so small a quantity of common air in protoxide of azote as this. The only person who attempted to determine the specific gravity of protoxide of azote before the experiments made in my laboratory above stated, was M. Colin. He obtained 1.5204. His gas was no doubt a mixture of 98.6 volumes of protoxide of azote and 1.4 volume of common air; for such a mixture would have precisely the specific gravity found by M. Colin.

Gay-Lussac showed long ago that deutoxide of azote is a compound of one volume of oxygen gas and one volume of azotic gas united without undergoing any condensation whatever; therefore the true specific gravity of this gas is exactly the mean between the specific gravity of oxygen gas and that of azotic

gas; or, in other words, we obtain it by adding together the specific gravities of oxygen gas and of azotic gas, and dividing the sum by 2.

Specific gravity of oxygen gas	1·1111
azotic gas.	0·9722
	2)2·0833
	1·04166

The first person who attempted to ascertain the specific gravity of this gas was Mr. Kirwan. He found its specific gravity 1·1887.* This greatly exceeds the theoretical specific gravity; but Mr. Kirwan was not remarkable for the accuracy of his experiments; and at that time it was customary to obtain nitrous gas by the application of heat to a mixture of copper filings and dilute nitric acid. Now when the process is conducted in this way some protoxide of azote is apt to be evolved, and to mix itself with the nitrous gas. This is more especially the case when the copper filings contain a mixture of brass, which, as far as my own experience goes, is almost always the case, unless the copper filings be prepared on purpose for the experiment. A mixture of 96·5 volumes of nitrous gas and 3·5 volumes of protoxide of azote would have the specific gravity found by Mr. Kirwan.

The nitrous gas upon which we made our experiments in my laboratory was prepared by dissolving slips of pure copper in dilute nitric acid. No heat was applied, and great care was taken to exclude common air as completely as possible. Three trials with gas prepared in this way gave the following results:

By first trial.	1·0409
By second trial	1·0410
By third trial	1·0410

The mean of these three gives 1·04096 for the specific gravity of deutoxide of azote. This is about $\frac{1}{1500}$ th part less than the theoretic specific gravity—a quantity within the limits of unavoidable error of experiment. Hence I consider myself entitled to conclude from the preceding experiments that the true specific gravity of deutoxide of azote is 1·04166.

Though I have prepared deutoxide of azote many hundred times, I have never been able to obtain it absolutely free from azotic gas. The volume of this gas which it contains sometimes amounts to 10 per cent. and very frequently to 5 per cent. When the quantity is so small as 1 or 2 per cent. I reckon the gas very pure. I do not know the reason of this, unless it be owing to the azote which is always contained in water, and which is evolved when the water is left in contact with nitrous gas. If

* Lavoisier's Elements, p. 569. English Translation.

the volume of azotic gas present in the nitrous gas subjected to experiment in my laboratory had amounted to 1.42 per cent. and I am persuaded it could not be less, it would fully account for the specific gravity formed; for the specific gravity of a mixture of 98.58 volumes of nitrous gas and 1.42 volumes of azotic gas would be 1.04096, if we suppose the specific gravity of pure nitrous gas to be 1.04166, and that of azotic gas 0.97222.

Thus the specific gravities of the gaseous compounds of oxygen and azote correspond with theory, and serve, therefore, to confirm and establish the important law of Gay-Lussac.

2. There is a gaseous compound of oxygen and chlorine first recognised as a peculiar substance by Sir H. Davy, though it had been prepared, and even slightly examined, many years before by Mr. Cruikshanks. It is prepared by pouring dilute muriatic acid on chlorate of potash, and exposing the mixture to a very gentle heat. The gas, which is of a strong yellowish-green colour, comes over, and must be received over mercury. Before submitting it to examination, it should be allowed to stand over mercury for 24 hours in order to get rid of the chlorine with which it is always mixed at first. The mercury gradually absorbs and condenses the chlorine, while it does not act upon the other gas. Davy gave to this gas the name of *euchlorine*, but I have distinguished it by the more systematic appellation of *protoxide of chlorine*. When this gas is exposed to a slight heat, it is decomposed with an explosion, and, according to the observations of Sir H. Davy, five volumes of it when thus exploded are converted into six volumes, which consist of a mixture of two parts by bulk of chlorine and one part of oxygen. Hence it follows that this gas is a compound of four volumes of chlorine and two volumes of oxygen condensed into five volumes; so that by uniting together these two gases undergo a diminution of one-sixth of their bulk. It is obvious from this that in order to obtain the specific gravity of protoxide of chlorine we must add together four times the specific gravity of chlorine gas and twice the specific gravity of oxygen gas, and divide the sum by 5.

$$\begin{array}{r}
 \text{Specific gravity of chlorine} = 2.5000 \times 4 = 10.0000 \\
 \text{oxygen} = 1.1111 \times 2 = 2.2222 \\
 \hline
 5)12.2222 \\
 \hline
 2.4444
 \end{array}$$

Thus it appears that the specific gravity of protoxide of chlorine is 2.4444.

Two experiments were made upon the specific gravity of this gas in my laboratory. The results obtained were as follows:

By first trial,	2.400
By second trial	2.403

The mean of the two gives 2.4015 for the specific gravity of protoxide of chlorine. This result differs a good deal from the theoretical number. The deviation indeed is not far short of $\frac{1}{50}$ th part. Though this may appear an enormous error to the mere speculative chemist, I am persuaded that it will not in the least surprise any person who has been in the habit of making experiments on this gas. The slowness with which the gas comes over, and the small quantity to which we are limited by the necessity of collecting it over mercury, render it peculiarly difficult to exclude common air completely. It was to the presence of a certain portion of air I am persuaded that the deviation in our experiments was owing; and my stock of chlorate of potash happened to be exhausted, which put it out of our power to repeat our experiments at the time. A mixture of rather less than three per cent. of air with the protoxide of chlorine subjected to experiment would have reduced the specific gravity to 2.4015, which was the number we obtained. I do not, therefore, consider the deviation of my experiments from the theoretical result as affording any presumption that the theoretical result is erroneous. Indeed I was rather gratified to obtain so near an approximation as I did, considering the great difficulty of collecting this gas in a state of absolute purity.

3. The only known gaseous compound of azote and hydrogen is ammonia. This gas has been long known, and has been shown by decisive experiments to be a compound of one volume of azotic gas and three volumes of hydrogen gas condensed into two volumes. Hence we obtain its specific gravity by adding together the specific gravity of azotic gas and three times the specific gravity of hydrogen gas, and dividing the sum by 2.

Specific gravity of azotic gas	= 0.9722
hydrogen gas..	= 0.0694 × 3 = 0.2083
	2)1.1805
	0.59027

According to this determination, the true specific gravity of ammoniacal gas is 0.59027.

In consequence of the opinion advanced by Sir Humphry Davy that ammonia contains oxygen as a constituent, the specific gravity of this gas has been determined with more care than, perhaps, has been bestowed upon any other gas. The following are the results obtained :

Kirwan.	0.600*
Davy.	0.5505†
Allan and Pepys.	0.6022‡

* On Phlogiston, p. 28.

† Recherches, p. 565.

‡ Phil. Trans. 1808.

result about $\frac{1}{2000}$ th part, and leads to the suspicion that the gas which they experimented upon was not pure, but in reality a mixture of about 98 volumes of muriatic acid and two volumes of common air; at least such a mixture would have the specific gravity indicated by them.

Three trials were made in my laboratory with muriatic acid gas prepared with every attention to ensure its purity. The results were as follows:

By first trial	1·2843
By second trial	1·2844
By third trial	1·2844

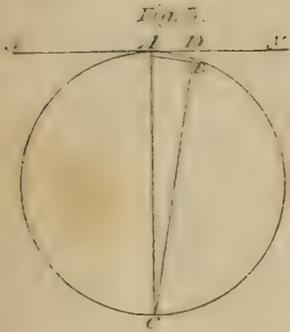
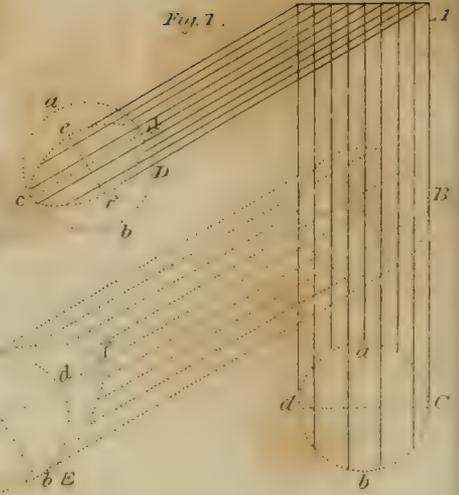
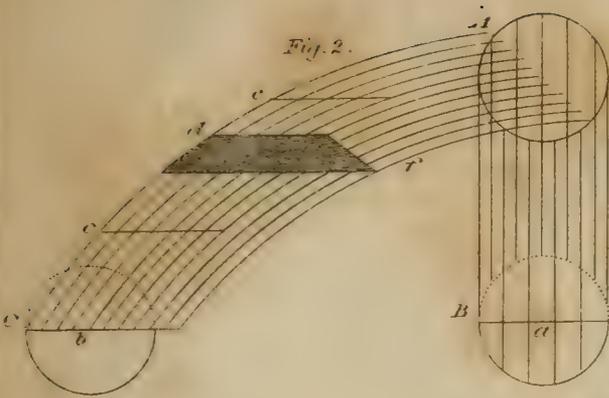
The mean of these gives 1·284366 for the specific gravity of muriatic acid gas. This number differs from the theoretical number by little more than $\frac{1}{5000}$ th part. This number being far within the limits of unavoidable error, we are warranted to conclude without any hesitation that muriatic acid is a compound of equal volumes of chlorine and hydrogen gases united without any change in their volume, and that its specific gravity is 1·2847.

This paper has already extended to such a length that it will be proper to reserve the statement of the remaining gases whose specific gravities have been ascertained in my laboratory, amounting in number to 12, to another opportunity. I shall conclude the present paper with exhibiting a table of the true specific gravities of the nine gases which have come under our review, together with the experimental results which have been obtained, and the difference between the true numbers and the experimental ones.

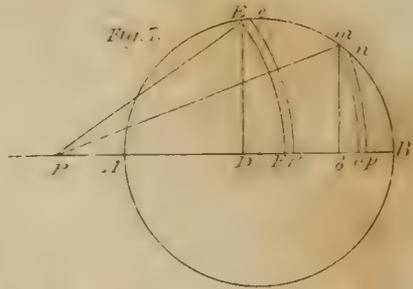
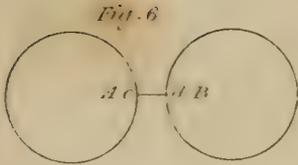
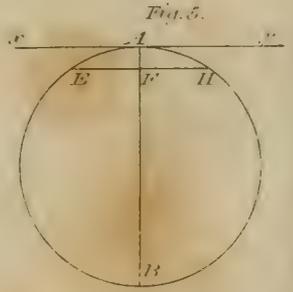
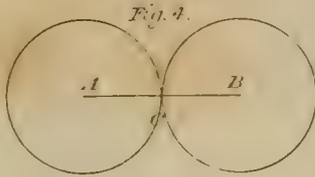
Gases.	True specific gravities.	Specific grav. obtained by exper.	Difference.
Oxygen	1·11110	1·1117	$\frac{1}{2000}$
Azotic.	0·97220	0·9728	$\frac{1}{1500}$
Hydrogen	0·06940	0·0694	0
Chlorine	2·50000	2·5000	0
Protoxide of azote.	1·52770	1·5269	$\frac{1}{1500}$
Deutoxide of azote.	1·04160	1·04096	$\frac{1}{1500}$
Protoxide of chlorine.	2·44440	2·4015	$\frac{1}{50}$
Ammoniacal	0·59027	0·590237	$\frac{1}{10000}$
Muriatic acid.	1·28470	1·284366	$\frac{1}{5000}$

In order to draw the particular attention of chemists to this subject, I need only mention that in the present state of the science it is of the very first importance. It is only by means of it that we can arrive at precise numbers for the atomic weights of bodies, and thus form an accurate basis upon which a true science can be reared. The methods followed by Dr. Wollaston,

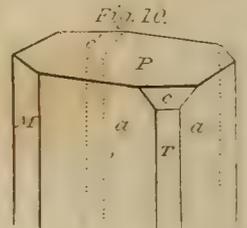
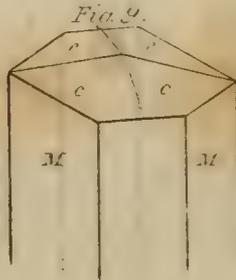
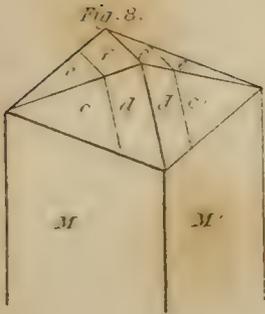




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Professor Berzelius, Mr. Dalton, and indeed every person who has hitherto turned his attention to the atomic theory, are obviously not susceptible of any great degree of precision. They have been guided entirely by the analytical researches without any general principle to direct their choice. Hence the very great diversity apparent in the numbers on which they have pitched, and the complicated nature of the numbers themselves; whereas it will be easy to show, when we have established the true specific gravities of the gases, that the numbers denoting the relative weights of the atoms of those elements that enter into the constitution of the gases are of the most simple kind, and that they are possessed of properties, which, if duly studied, would soon lead us a great way into the knowledge of the constitution of the vegetable and animal kingdoms. But I avoid entering upon this subject till a future opportunity.

(To be continued.)

ARTICLE II.

On Rain-Gauges. By Mr. H. Boase. (With a Plate.)

(To Dr. Thomson.)

SIR,

Penzance, July 12, 1820.

YOUR respectable correspondent Mr. Meikle (*Annals of Philosophy* for April, p. 269), appears to me to have too hastily concluded that “*the quantity of water received by the rain-gauge is totally independent of the general inclination of the rain.*”

First, Because common experience proves that more rain falls on a given surface in equal time during a calm than during a storm, other circumstances being the same. To this every superintendant of a rain-gauge, or gardener, or agriculturist, will bear uniform testimony.

Second, Because the principle of the parallelogram adopted by Mr. Meikle in this case proves too much; for if “*the perpendicular distance of the drops of rain varies with the sine of inclination, while it is the distance of the points in which the drops pass through a horizontal plane that is invariable,*” then a greater quantity of water will be received by the rain-gauge (other circumstances being equal) in proportion to the sine of inclination; for (adopting Mr. Meikle’s own diagram) let A B (Pl. CVII.) fig. 1, the vertical column of rain, be inclined to an angle of 30° as A D, the perpendicular distance of the drops will be reduced one half, consequently the whole assumed column A B will be received within the ellipsis *c e d f*. Suppose the diameter of the basin *a C b d* (or *a d b c*) divided into 10 parts (inches, miles, or leagues), the area of its surface will be $10^2 \times 7854 = 785400$; hence the ellipsis *c e d f* must be $10 \times 5 \times$

$\cdot 7854 = 39 \cdot 2700$; so that the basin D would receive twice as much water as the basin C, and the sloping would be twofold heavier than the vertical fall of the same rain.

Thirdly, Because a continued increase of the horizontal velocity would at last compress the supposed column into a dense sheet of water *no further compressible*; but the principle assumed requires that the compression should still proceed, which is impossible.

Thus, I conceive, Mr. Meikle's position falls, without drawing after it "*some of the simplest truths of geometry*," because those truths were inapplicable to the question.

Others of the simplest truths assure me that whatever composition of forces affects one drop of the assumed column of rain equally affects every other drop; and consequently whatever their perpendicular distance was, such it will be so long as the same cause exists.

For let A B C be supposed a *solid* column, or cylinder; and let it be inclined to the same angle, as before shown, of 30° as B E, it is obvious that its perpendicular diameter $a b$ remains unaltered, but its horizontal section $a f b c$ is twice as great as its perpendicular section, or the area of the basin $a d b c$, which, as before shown, is $= 78 \cdot 5400$, whereas the ellipsis is $\overline{20} \times 10 \times \cdot 7854 = 157 \cdot 0800$.* Hence it appears, that "the distance of the points in which the drops pass through a horizontal plane" *does vary* in proportion to the sine of inclination, unless it can be proved, in reference to the descending rain, that equal forces acting on equal drops produce *unequal effects*. Surely your respectable correspondent was rather too much *inclined* to sarcasm when, after remarking that "*admitting the opinion of these learned gentlemen to be correct, the quantity of rain which falls on any given space of ground during wind would be less than what came away from the area of the cloud*," he adds: "Query, *Is the rest annihilated, or what becomes of it that it does not reach the earth?*" The answer is plain, it does reach the earth, but is dispersed over a *greater surface*. The shower which falling vertically would have covered (suppose) 10 square leagues, if carried by a horizontal velocity equal to the mean downward velocity to an angle of 45° will be dispersed over $13\frac{1}{2}$ leagues, if, by a horizontal velocity as 2 to 1, at the angle of 30° over 20 leagues, and if by a force as 4 to 1, at the angle of 15° , over 40 leagues, and so on. If this hypothesis be true, it will, I think, afford a satisfactory solution of the phenomenon of rain-gauges at different heights indicating a different fall of rain.

If I have succeeded in proving that the perpendicular distance of the drops composing a given column of rain is *not varied by*

* It may be right to remark that the paths of the drops falling obliquely will not be straight, but parabolic lines. This will vary the angle in a small degree, but does not affect the principle.

its oblique descent, it appears to me that the phenomenon of different quantities of water being received by similar rain-gauges placed at different heights may be easily explained.

For let A B, fig. 2, represent the given column of rain, and let the assumed horizontal velocity of the wind be double the mean of the downward velocity, then the falling rain will describe the parabola A C. Suppose the basin, *a*, of the pluviometer divided into 10 parts, it is evident that the like basin, *b*, where the rain falls at an angle of 60° , will receive only 8.66 of those parts. Let another gauge be placed at *c*, where the angle of inclination is nearly 45° , or at *d*, 30° , or at *e*, $23\frac{1}{2}^\circ$ nearly, and the quantities received will be respectively $\frac{7}{10}$ ths, $\frac{5}{10}$ ths, and $\frac{4}{10}$ ths, or parts of the basin *a*. Hence then it follows that the proportion of water received by rain-gauges placed at different heights, *ceteris paribus*, is as *the sine of the angle of inclination of the falling rain*. Should hills, trees, or buildings, disturb the horizontal force, the angle of inclination will be varied, but the ratio remains the same. Our rain-gauges indicate very accurately the quantity of rain received by the surrounding surface, provided that surface is level, on the same plane and height as the gauge, otherwise they must differ. Thus let *d f* be a mount, having a tabular top, exactly equal to the assumed column of rain; it is clear that when the rain falls at an angle of 30° as at *d*, the top receives only five parts or one half, while the other falls on the side *f* upon half the area of the top; consequently a gauge placed on the side of the mount would indicate only half the quantity of water actually received by the surrounding surface. It is, perhaps, owing to this circumstance, that the different sides of the same hills are so unequal in fertility and the growth of trees, particularly on the sea coast, where high winds are more frequent, and the rain generally comes from one quarter of the compass.

I am, Sir,

Your most obedient servant,

H. BOASE.

P. S. Permit me to offer your readers a more simple mode of reducing the height of the barometer to the temperature of 32° . Multiply the difference of the thermometer above 32° by 3, and subtract the product from the second and third decimal figures of the observed height of the barometer.

<i>Example</i> : Barometer	30.000
Thermometer, 62° ; difference 30×3	= 00.090
	29.910

This indeed does not exactly agree with your method, except when the barometer is at 30 inches, but the difference is so minute, being only $\frac{1}{10000}$ th of an inch at 29 or 31, as not to be worth notice in ordinary observations.

ARTICLE III.

On the Mathematical Principles of Chemical Philosophy.

By Mr. J. B. Emmett.

(Continued from p. 46.)

SIR,

Hull, July, 1820.

Attraction in General.

By attraction is to be understood a tendency which any number of particles or masses of matter have, when at liberty, to approach towards each other, or, as it is defined by Sir I. Newton, definition 5: "Vis centripeta est, qua corpora versus punctum aliquod tanquam ad centrum undique trahuntur, impelluntur vel utcumque tendunt." We are, and most probably shall ever be perfectly ignorant of the nature of that which we denominate attraction; there are ample proofs of the existence of such a force, and though mathematical investigations lead to a development of its laws of action, they can never make us acquainted with its intimate nature; I, therefore, wish it to be minutely understood that the following researches relate only to the manner in which this force operates in producing the various phenomena of corpuscular action.

It is demonstrated, Princip. lib. 1, prop. 11 and 71, that every particle of matter in nature attracts every other particle, the force being in the inverse duplicate ratio of the distance; its actual operation in masses of terrestrial matter is experimentally proved by the phenomena of the tides, by the observations of Dr. Maskelyne (Phil. Trans. 1775), and in still smaller masses by the experiments of Mr. Cavendish. The ascent of water in capillary tubes is also a proof that the particles of water have a tendency towards those of glass; in each of these, it would appear that the attraction of the whole is made up of the sum of the attractions of the several particles; and it is easily demonstrable, from various chemical phenomena, that this centripetal or attracting force really resides in every ultimate atom or particle of matter; thus, when dry carbonic acid is mixed with dry ammoniacal gas, there is an immediate condensation, and solid matter is formed: the gaseous particles must have evidently tended to each other with a very considerable degree of force, since they now occupy a volume many hundred times smaller than that which they previously filled, and we are totally unacquainted with any mode by which this can be effected, excepting by the operation of an inherent tendency in the particles of matter, when at liberty to approach each other; and since we know that such a tendency really exists, and is operative between the larger masses of our system, there is nothing extravagant in supposing the above and all similar effects to result from the operation of such force; but to remove all doubts respecting it,

on examining the matter produced, a considerable part will be found in a crystalline form lining the interior of the vessels in which the experiment has been performed, and which will be found to have a very considerable cohesive force. There is, therefore, no impropriety in assuming that every particle of matter in nature attracts every other particle.

Since it is found that the attraction of the planetary bodies varies reciprocally in the duplicate ratio of the distance; and it is proved, Princip. lib. 1, prop. 71, that in order that this effect may result, the attraction of each of the particles of which these bodies are composed, must follow the same law; and since none of the researches that have been made into the various departments of physical astronomy indicate even the most minute operation of any other centripetal force, I shall suppose the ultimate atoms of ponderable matter to exert upon each other this force only, and attempt to demonstrate its capability of producing all the primary phenomena of corpuscular attraction; and here we must suppose the particles of matter to have magnitude, which is excessively minute; and, therefore, to have figure, and not to be, as some have imagined, mere physical points, which exert various forces upon each other.

On the Attraction of Cohesion.

Lemma 1.—If two spheres touch each other, an indefinitely small portion of the surface of each coincides with an equal one of that of the other.

Let $A B C$ (Pl. CVII), fig. 3, be a circle, $x y$ a tangent at A , draw the diameter $A C$, take $D B$ perpendicular to $x y$, and indefinitely near to $A C$; join $A B, B C$; then Princip. lemma 7, the arc $A B$ coincides with the chord $A B$; by similar triangles, $A C : A B :: A B : D B = \frac{A B^2}{A C}$, or since in the same circle,

$A C$ is constant, $D B$ is as $A B^2$; let $A D$ constantly diminish, and $D B$, its deflexion from the tangent, will diminish in the duplicate ratio of $A B$; therefore, when $A D$ is indefinitely near coincidence with $A C$, $D B$ will much more be indefinitely less than the least possible finite quantity; or at the point A , an indefinitely minute portion of the circumference of the circle will coincide with the tangent $x y$: and if an equal circle be drawn, touching the tangent $x y$, in A , but on the other side of it, an equal portion of its circumference will coincide with it, and, therefore, at A an indefinitely small portion of the circumference of each circle coincides with an equal one of that of the other. In the same manner it may be proved that the spherical surfaces which are formed by the revolution of the circles upon a straight line which joins their centers, and, therefore, passes through the point of contact, coincide with each other at the point of contact.

It may, perhaps, be objected, that in spheres these points of

contact are only mathematical points, and consequently there can be no coincidence. In mathematical reasoning, they must be regarded as having no value; for had they a finite value, the curvature of the circle would not be continuous and uniform, but it would be a polygon, having a finite number of sides; but in spheres composed of matter, when they are brought into the closest contact, the points of contact have magnitude, though indefinitely smaller than the least finite magnitude; for let $A B$, fig. 4, represent two spheres, which are urged by any force, in the direction of the line $A B$, which joins their centers; they move to a certain distance, when their surfaces meet in C , and all motion ceases. If the points of contact, C , be considered as physical points, since these have no magnitude, the spheres are not in contact, and can, therefore, be brought nearer to each other, but they cannot; since, therefore, they have arrived at the nearest distance from each other, and it is evidently the presence of solid matter which prevents the motion of each sphere, a quantity of matter can be comprehended under the magnitude of a mathematical point, which is absurd; consequently at C , the surfaces must touch and coincide with each other, the points of coincidence being indefinitely less than the least finite magnitude.

Lemma 2.—The surfaces of any other solids which are formed by the revolution of curves of finite curvature upon their axes will coincide with each other at the point of contact. For at that point every line which is drawn upon the surface of such solid, and passes through it, will coincide with a circle of a finite diameter. Hence through the point of contact of two such solids an indefinite number of circles may pass whose planes are inclined to each other, and which have severally the same curvature as the part of the solid which they touch at the point of contact, and each of these circles coincides with its tangent at the point of contact.

Lemma 3.—The area of the points of contact of equal spheres are as their diameters.

Let $A E B H$, fig. 5, be a sphere, xy a tangent at A , $A B$ a diameter, which is terminated at one extremity in A ; draw the plane $E F H$ parallel to the tangent; since the spherical surface $E A H = A F \times$ circumference $A E B H$, if in spheres of different diameters, the abscissæ $A F$ be equal, the surfaces cut off, will be as their circumferences, or as their diameters. Let now $A F$ continually diminish, and in different spheres the ultimate ratio of the evanescent surfaces which coincide with the tangential planes will be as the diameters of the spheres; since an equal portion of the surface of another equal sphere will coincide at A with the tangential plane xy ; but on the other side of it, in spheres of equal magnitude, the points of contact will be as their diameters.

Cor. If spheres of unequal diameters touch each other, the points of contact will be as the diameters of the smallest.

Prop. 1.—The ultimate particles of ponderable matter being spherical, and the centripetal force being reciprocally in the duplicate ratio of the distance, the attraction of cohesion results from the attraction of the portions of their surfaces which coincide with each other in contact.

Let A and B, fig. 6, be two ultimate particles of matter, of which every point exerts a centripetal force which is as $\frac{1}{\{\text{Distance}\}^2}$, c and d , the two indefinitely small portions of their surfaces which coincide with each other when in contact, the force which c and d exert upon, or by which they tend to, each other is as $\frac{1}{c d^2}$; let them be placed in contact; $c d = 0 \therefore \frac{1}{c d^2} = \text{infinity}$; i. e. the force with which c and d attract each

other when in contact is infinitely greater than when they are at any finite distance however small. Hence if this force be finite at any finite distance, it becomes infinite in contact; but if finite in contact, it vanishes at the least possible distance.

Cor. 1.—If centripetal force vary reciprocally as any power or root of the distance, the effect of cohesion will be produced; for since the force is as $\frac{1}{c d^n}$, when $c d$ vanishes, the force with which c and d mutually attract each other becomes $\frac{1}{0^n} = \text{infinity}$; but if the force were supposed to be as $c d^n$ in contact, the force with which the parts c and d mutually attract each other $= 0^n = 0$.

Cor. 2.—Hence the particles of matter cannot be plane figures of any sort, nor be terminated in any of their parts by plane surfaces; for since the attraction of one particle is finite at a finite distance, consequently that of any finite part is finite; therefore the attraction of two such plane surfaces is infinite in contact, which does not coincide with the observed phenomena.

Prop. 2.—The attraction of cohesion between two particles of matter is as the absolute force of attraction upon their surfaces \times the diameter, the spheres being equal; if unequal, it is as the same \times diameter of the smallest.

By lemma 3, when the spheres are equal, the points of contact are as their diameters; when unequal, as the diameter of the smallest; and if the centripetal force vary, the force with which they attract each other will vary in the same ratio. The force of cohesion will be as the points of contact \times absolute centripetal force.

Prop. 3.—The same phenomena of cohesion will result of whatever figures the particles of matter be assumed, provided they be such as may be formed by the revolution of curves of finite curvature, and which return into themselves upon their axes.

For by lemma 2, if two such solids be brought into contact, an indefinitely minute portion of the surface of each coincides with an equal one of that of the other; and by prop. 1, the attraction between these will be indefinitely greater when in contact with each other than when at the least finite distance.

Cor. Hence, in all cases, the force of cohesion vanishes when the attracting particles are removed to the least distance from contact.

Prop. 4.—According to whatever power of the distance the centripetal force vary, and whatever be the figure of the particles of matter, the force of cohesion can only be produced when they are in the closest possible contact.

Let AEB , fig. 7, be an attracting sphere, P , a corpuscle placed without it; from P draw the straight line, PAB , passing through the centre: in the circumference, AEB , take any points E, m , and join PE and Pm ; take Ee, mn , two evanescent portions of surface, and with centre, P , describe the circular arcs, EF, ef, mo, np , and let $Ff = op$. It is demonstrated, Princip. lib. 1. prop. 79, that the force with which the solids formed by the revolution of the figures, Ef, mp , upon PB , attract the corpuscle, P , will be as $DE^2 \times Ff \times$ ratio of the force with which a point in F attracts the corpuscle, and $\delta m^2 \times$

$op \times$ ratio of the force with which a point in o attracts P . Let the law of centripetal force be $\frac{1}{\{\text{Distance}\}^n}$, and since $Ff = op$, the force with which P is attracted by the solid Ef : that with which it is attracted by $mp :: \frac{DE^2}{PF^n} : \frac{\delta m^2}{P o^n}$, let E coincide with A ,

and the distance PA vanish, then the ratio $\frac{DE^2}{PF^n} : \frac{\delta m^2}{P o^n}$ becomes

that of $\frac{DE^2}{o^n} : \frac{\delta m^2}{P o^n}$, or of infinity : $\frac{\delta m^2}{P o^n}$; but in every other position

of E (P being in contact with A), PE will have a finite ratio to every other finite distance Pm ; hence the forces of all other parts of the sphere will have a finite ratio to each other; and if PA have a finite ratio to PB , the forces of all such figures as EF will also have to each other a finite ratio; and if PA vary, the variations in the force of each similarly formed solid Ef will have a finite ratio to each other, since $\frac{DE^2}{PF^n}$ representing the force, if PE vary, if its variations have to its original value a finite ratio, the force cannot be indefinitely increased or diminished by a finite variation of PA , excepting in the case when $PA = 0$. Consequently the phenomena of cohesion can only be produced when the particles are brought into actual contact with each other, and in every case arises from the mutual attraction of the portions of their surfaces which then coincide with each other only. The same may be proved in a similar manner of all other solids.

Prop. 5.—If equal cylinders be formed of particles of equally attractive matter, their cohesive force will be reciprocally as the diameters of these particles.

For by prop. 2, the cohesion will be as the diameters of these particles, and since in a given area the number of equal circles that can be described is reciprocally as the squares of their diameter, the number of points of contact in equal sections of these equal cylinders will also be reciprocally as the squares of the diameters of the particles; and by compounding these ratios, we obtain that of the inverse diameter of the particles.

Cor. 1.—If the centripetal force vary, the cohesion of the equal masses will be as the centripetal force directly, and the diameter of the particles inversely.

Cor. 2.—All that has been proved of spheres will equally apply to any other solids, which are formed by the revolution of any curves of finite curvature, and which return into themselves upon their axes.

Scholium.

Since the assumption of spherical atoms may be objected to by some, I have thought it proper to demonstrate generally that all solids, as spheroids, &c. excepting those that have plane surfaces, will present the same phenomena; yet the sphere is that which seems most properly adapted to all cases. We have a good example of the operation of magnetical attraction in producing an effect precisely similar to cohesion in the following experiment; present an iron sphere to one pole of a magnet, it will adhere with such force as will enable it to sustain a considerable weight; cover the pole with goldbeater's skin, or other exceedingly thin lamina of matter: the tendency of the sphere to the magnet will be considerable, but will be very little increased, by being brought into contact with the film, when it will be found to sustain much less weight than before. In this experiment we observe that when the lamina is interposed, the tendency of the whole sphere will be very little diminished, since its thickness is almost insensible, compared with the distance between the centre of the sphere and the centre of attraction of the magnet; the difference between the forces with which the sphere adheres to the magnet before and after the interposition of the lamina can arise only from the attraction of the points of contact when in contact, being in one case added to the whole tendency of the sphere, but being removed in the other; and magnetical attraction is reciprocally as the square of the distance.

It has been demonstrated, prop. 4, that the effects of cohesion will result, according to whatever inverse power or root of the distance the centripetal force be supposed to vary; and in prop. 1, it has been particularly demonstrated, when the force is sup-

posed to be reciprocally in the duplicate ratio of the distance, and which in masses produces gravity. Since then this latter force is known to exist, and we have not the most trivial indication of any other, we may fairly infer that this force alone is concerned in producing the attraction of cohesion, and that the results obtained by help of the above propositions minutely agree with what we observe to attain in nature, is proved by the following computation: Suppose two solid spheres, the diameter of each being one inch, to be placed in contact with each other; let the weight of each be 1000 grs. and the force of cohesion to be $\frac{1}{1000000}$ th of a grain; of the same matter let a cylinder of one inch in diameter be formed, its component particles being equal, and having a diameter = $\frac{1}{1000000}$ th of an inch; the weight of each particle becomes $\frac{1000}{\{1000000\}^3}$ gr. = $\frac{1}{1000000000000000}$ th of a gr. and by prop. 1, the cohesive force between two adjacent particles will be $\frac{1}{1000000000000000}$ th of a gr.; and by prop. 5, the cohesive force of the whole cylinder will be one grain. Let now an equal cylinder be formed of the same matter, the particles having each a diameter = $\frac{1}{1000000000000000}$ th of an inch; the weight of each particle will be = $\frac{1000}{\{1000000000000000\}^3}$ th of a grain, the cohesion between two adjacent particles will be $\frac{1}{1000000000000000000000000}$ th of a gr.; and the cohesion of the whole mass will be 1000000 grains = 130lbs. or about $1\frac{1}{8}$ th hundred weight; and if the particles were reduced to $\frac{1}{1000000}$ th part of what they were last supposed, the force of cohesion of this cylinder, whose diameter is one inch, would be increased 1000000 times. Whence it is manifest that in a mass of matter, the cohesive force may be very great, though the attraction of such a mass is absolutely insensible, the force being inversely as the square of the distance. This subject may be experimentally illustrated: Let chalk, calcined bones, white lead, or any other insoluble matter be reduced to a coarse powder, moisten this with water, and let it be dried with a very moderate degree of heat; when dry, it will form a mass, of which the parts cohere with very little force, the cohesion scarcely, if at all, exceeding their weight. Let it now be reduced to an impalpable powder, and treat it similarly, when it will be found that the cohesive force will be very great, the mass, when suspended by one of its parts, being able not only to sustain its own weight, but also a very considerable additional one. Since these

substances are highly insoluble, the only use of the water is to keep them in some measure separated, by which means they will have freedom of motion, and can arrange themselves in that order in which their mutual attraction has a tendency to place them.

Also by pressure coarsely powdered sulphur cannot be made to cohere into a mass, but when finely pulverized, or sublimed, the impalpable powder, by moderate pressure, will form a mass, having considerable hardness. By carefully examining these experiments, we find that by diminishing the diameter of the small masses or parts of which any mass of matter is composed, the cohesive force of the whole mass is increased, which it should be by prop. 5, but the cohesion of any two particles is diminished, which coincides with prop. 2, since however hard it may be a very moderate degree of friction will be found to remove some particles from its surface. We also find that the cohesive force vanishes when the particles are removed to the least possible distance from contact, as is proved by breaking the mass, which will not cohere again by being joined: this coincides with prop. 1 and 4.

Since some may yet suppose that some other force besides that which varies reciprocally as the square of the distance is concerned in producing the phenomena of cohesion, it may not be improper to show that a force which varies inversely as a higher power of the distance will not answer the conditions. Suppose it to vary reciprocally as the cube of the distance, and the force of the whole sphere or particle to be finite, at a finite distance from contact, then, Princip. lib. 1. prop. 81, Example 2, when the corpuscle P is placed in contact with A (see fig. 7, prop. 4), the whole force becomes infinite, which does not coincide with the observed phenomena, and in this case its influence must be observed in its effects upon the planetary motions. Let the force be such that at any finite distance from A the attraction of the whole sphere is insensible, or indefinitely small, in which case the attraction of any finite part of the sphere is but finite, when P is placed in contact with it, supposing the particle P to coincide with a finite part of the spherical surface; but when the corpuscle is in contact with the sphere, by lemma 1, the points of contact will be indefinitely less than any finite magnitude; consequently the force is indefinitely small; and since the attraction of the whole sphere = 0, when PA has a finite ratio to AB, by the hypothesis, the attraction of each of the figures $E f$, $m p$, which is not in contact with P, and consequently of their sum, or the whole sphere, excepting the point A, is = 0; therefore, upon this supposition the corpuscle is attracted only by the point A, which has been proved to attract it with a force which is less than any finite force. The same may be demonstrated, if the force be supposed to vary

inversely as the fourth power of the distance, by help of prop. 81 example 3, Princip. that force, therefore, which varies inversely as the square of the distance, appears to be the only one that can produce all the observed phenomena of cohesion.

ARTICLE IV.

On the Theory of Arbogast respecting the Separation of the Symbols of Operation from those of Quantity. By Mr. G. Harvey.

(To Dr. Thomson.)

SIR,

Plymouth, June 5, 1820.

THE celebrated Arbogast has well remarked in the preface to his learned work "Du Calcul des Derivations," that "le secret de la puissance de l'analyse consiste dans le choix et l'emploi heureux de signes simple et caracteristique de la chose qu'ils doivent représenter." Of the truth and propriety of this remark, an historical review of the progress of analysis will present many examples; of the advancement which particular departments have made by the judicious adoption of appropriate symbols; and the way in which other interesting branches of analytical inquiry have been retarded by not attending to this important particular.

In selecting or contriving a symbol to designate any analytical operation, we shall not communicate to the analysis the full powers of the symbol by regarding it merely as an instrument to facilitate our *present* inquiries; or only to estimate its *immediate* operations on the functions or quantities which it is destined to influence and controul; but in its adoption, to regard its capabilities in a *prospective* as well as a *present* point of view; and to contemplate the possibility of its producing, even from *its own evolutions*, new and enlarged ideas respecting analytical science.

I need not go far for examples to prove the consistency of these observations. Every one conversant with the progress of analytical inquiry will instantly recollect the facilities and advantages which have resulted to the differential calculus, and the splendid consequences which have been obtained, by the employment of the notation of Leibnitz; and to the theory of finite differences from the notation of Euler; not to mention the harmonious character which these possess, with the notation adopted by Lagrange, for his delicate and refined calculus of variations.

I have been led into this train of reflection by reviewing some of my MSS. on the method first announced by Arbogast respecting *the separation of the symbols of operation from those of*

quantity; and the elements of which, adapted to his "Signes des Derivations," may be seen in the second general article of his work; together with its application to the symbols of the differential calculus and finite differences in Articles 371 and 442.

The remarkable simplicity attendant on many of the investigations of analysis by the method just alluded to, and the concise and beautiful demonstrations it affords of many interesting and important formulæ, renders the inquiry peculiarly instructive and pleasing.

We well know that the generality of functions often disclose by the successive and varied effects of the symbols of operation, new, and remarkable laws; but to mark the changes which the *symbols* themselves undergo in these operations; and to draw from their characteristic forms, the elements of almost a new analysis, is an idea, original in its kind, due to Arbogast alone, and owes its origin to the happy adoption of the differential notation. It was this view of the subject doubtless which induced a learned and skilful analyst* to remark, that the system of notation resulting from the theory of Arbogast "seems to unite in the most perfect manner the properties of conciseness, simplicity, and elegance, and appears peculiarly well adapted to open new and enlarged views of the extent and meaning of analytical operations."

Mr. Woodhouse in his admirable work on the "Principles of Analytical Calculation," and Mr. Herschell in the treatise just alluded to, are the only analysts, as far as I am aware, who have directed the attention of our younger mathematicians to this important subject. And as it is the object of a periodical work like the *Annals of Philosophy*, not only to be the repository of the new and original views which men of genius are continually producing; but also to give a wide circulation, and an accelerated effect, to truths which have hitherto been confined to treatises accessible only to a few, I have conceived that some elementary remarks on the theory of Arbogast, and some illustrative examples, may not only be of immediate benefit to the young analyst, but may also induce him to extend his inquiries to the authors above-mentioned; and thus to diffuse in a wider degree the taste for analytical researches, which now seems so rapidly forming in this island.†

* Mr. Herschell in his Appendix to the translation of La Croix's Differential Calculus, p. 479.

† The future historian of analysis will probably trace to Mr. Woodhouse's treatise on analytical calculation, the dawn of those just and enlarged views of the nature of analysis in general, now so widely diffused among the self-taught mathematicians of this island. And to the same work we may attribute the popularity which the calculus of functions is so rapidly acquiring; and by means of which the student may be conducted "from the analysis of finite quantities into the analysis of infinite without formally announcing to him his arrival on their boundaries, or requiring him abruptly to pass them."—(Woodhouse, *ibid.* p. 72.)

Of the particular departments of analysis to which the symbols Δ , d , and δ , respectively belong, and also of the peculiar operations of which they are destined to be the representatives, the student may obtain ample information in the excellent translation of La Croix. And it shall now be my object to exhibit the application of these symbols to the theory of Arbogast, and to show how far it is capable to separate them from the symbols of quantity.

In the theory of finite differences, it is well known that if the series

$$u_{-n} \dots\dots\dots u_{-2} \quad u_{-1} \quad u \quad u_1 \quad u_2 \dots\dots\dots u_n$$

be assumed, that the *excess* of any one term over that which immediately *precedes* it is called the *difference* of the latter, and is denoted by the symbol Δ .

On this hypothesis is formed the series of equations,

$$u_1 - u = \Delta u$$

$$u_2 - u_1 = \Delta u_1$$

$$u_3 - u_2 = \Delta u_2$$

.....

$$u_n - u_{n-1} = \Delta u_{n-1}$$

and which by evident transpositions become

$$u_1 = u + \Delta u$$

$$u_2 = u_1 + \Delta u_1$$

$$u_3 = u_2 + \Delta u_2$$

.....

$$u_n = u_{n-1} + \Delta u_{n-1}$$

Or, separating the symbols of operation from those of quantity,

$$u_1 = (1 + \Delta) u$$

$$u_2 = (1 + \Delta) u_1$$

$$u_3 = (1 + \Delta) u_2$$

.....

$$u_n = (1 + \Delta) u_{n-1}$$

If now the value of u_1 in the first of these equations be substituted in the right hand member of the second; and this new value of the second substituted in the third, and so on; there will arise

$$u_1 = (1 + \Delta) u$$

$$u_2 = (1 + \Delta)^2 u$$

$$u_3 = (1 + \Delta)^3 u \tag{1.}$$

.....

$$u_n = (1 + \Delta)^n u$$

in each of which equations, the expression $1 + \Delta$ is to be regarded as having no other meaning than an abbreviated expression for its development, but which when expanded, and

the value of u restored, will furnish the absolute value of the function u_n , with all its orders of differences. For example, La Croix, at p. 4, of his "Traite des Differences," observes that

$$u_n = u + n \Delta u + \frac{n(n-1)}{1.2} \Delta^2 u + \frac{n(n-1)(n-2)}{1.2.3} \Delta^3 u \dots \dots$$

+ $\Delta^n u$; and it is evident, that if the conditional equation

$$u_n = (1 + \Delta)^n u$$

be developed, and the function u restored to the symbols of operation, the same result will be obtained.

Arbogast arrives at a similar conclusion, pp. 375, 377, "Du Calcul des Derivations," nearly as follows :

Let $E^0 u, E^1 u, E^2 u \dots \dots \dots E^n u$

represent consecutive states of the function u , so that

$$E^0 u = u$$

$$E^1 u = u_1$$

$$E^2 u = u_2$$

.....

$$E^n u = u_n$$

then since $E^1 u$, or $E u = u^1$, and that by the theory of differences $u_1 = u + \Delta u$, it follows that $E u = u + \Delta u$; and by separating the function u from the symbols of operation,

$$E = 1 + \Delta,^*$$

Whence

$$E^n = (1 + \Delta)^n;$$

and restoring the function u to the symbols of operation,

$$E^n \times u = (1 + \Delta)^n \times u$$

Or,

$$u_n = (1 + \Delta)^n u,$$

the same as above deduced.

By an equally ingenious process, the same able mathematician arrives at the value of $\Delta^n u$, as follows :

Since $E = 1 + \Delta$, it follows that

$$\Delta = E - 1, \text{ and } \Delta^n = (E - 1)^n.$$

Hence, by restoring the function u to the symbols of operation, we have

$$\Delta^n u = (E - 1)^n u;$$

the latter member of which developed by the binomial theorem, and u restored to the symbols of operation, gives

$$\Delta^n u = E^n u - n E^{n-1} u + \frac{n(n-1)}{1.2} E^{n-2} u \dots \dots \dots \pm E^0 u;$$

and since by the hypothesis $E^n u = u_n, E^{n-1} u = u_{n-1}$

$E^{n-2} u = u_{n-2} \dots \dots \dots E^0 u = u$, there arises by substitution,

$$\Delta^n u = u_n - n u_{n-1} + \frac{n(n-1)}{1.2} u_{n-2} \dots \dots \dots \pm u.$$

* The symbol = does not denote an absolute equality between E, Δ , and 1, but is simply used to connect two modes of expression arbitrarily made equivalent.— (See Woodhouse's Principles of Analytical Calculation, p. 123.)

A similar result may also be obtained from the last of the equations marked (1). For since the arbitrary equation

$$\Delta^n = \{ (1 + \Delta) - 1 \}^n$$

may be assumed, and that the latter member when developed becomes

$$(1 + \Delta)^n - n(1 + \Delta)^{n-1} + \frac{n(n-1)}{1 \cdot 2} (1 + \Delta)^{n-2} \dots \dots \dots \pm 1;$$

it follows, that by uniting the function u to the several symbols of operation, there will arise

$$\Delta^n u = (1 + \Delta)^n u - n(1 + \Delta)^{n-1} u + \frac{n(n-1)}{1 \cdot 2} (1 + \Delta)^{n-2} u \dots \dots \dots \pm u.$$

But since from the equations (1)

$$\begin{aligned} (1 + \Delta)^n u &= u_n \\ (1 + \Delta)^{n-1} u &= u_{n-1} \\ (1 + \Delta)^{n-2} u &= u_{n-2} \\ &\dots \dots \dots \end{aligned}$$

$$u = u$$

there will arise by substitution,

$$\Delta^n u = u_n - n u_{n-1} + \frac{n(n-1)}{1 \cdot 2} u_{n-2} \dots \dots \dots \pm u,$$

which is identical with the result obtained by Arbogast.

The conciseness of the above investigations demonstrates that many important advantages may be derived from the theory of Arbogast. And although, in the first point of view, it may seem impossible to pursue with certainty and precision any extended train of analytical inquiry, by employing symbols of operation when detached from the functions with which they were originally connected; yet it will be perceived, that it is of no consequence in what light we regard the symbol; whether as an instrument to generate successive changes in a function, or to transfer to it the momentary idea of quantity, when in its detached state from the function. In the latter point of view, it necessarily becomes subject to all the forms and laws of which quantities are susceptible. But it may be proper to caution the inquirer against his attaching a permanent idea of quantity to the symbol of operation, even in its separated state. It is only indeed in this latter condition that it is supposed to be subject to the rules and operations of quantity, and never decidedly loses the character it was originally destined to maintain in any stage of an investigation. In my next communication I shall endeavour to point out some further applications of the principle.

I am, Sir, your humble servant,
 GEORGE HARVEY.

The prisms are much longer in proportion than those of the mesotype, and the natural planes smoother and more brilliant, those of the mesotype being striated longitudinally, and affording comparatively imperfect reflections.

The primary form of the needlestone, fig. 9, is also a right rhombic prism, but measuring $91^{\circ} 10'$ and $88^{\circ} 40'$, M on M' being the acute angle.

It differs also in its chemical composition from mesotype, which, according to Berzelius, contains no lime, while the needlestone does contain it.

If the planes c, c', e, e' , be the result of a decrement by one row on the terminal edges of the prism, the height of the prism will be to one of those edges as 1 to 2.

Measurement of M on M'	88° 40'
M on C	116 30

I believe it was ascertained some time since by Dr. Wollaston that this substance differed from the mesotype both chemically and crystallographically.

Thomsonite, from Dumbarton.

This substance is found in the neighbourhood of Kilpatrick, near Dumbarton, and has for its primary form a right rectangular prism.

The crystals I have examined are of the form fig. 10, but they are not sufficiently perfect to afford the necessary measurements for determining the dimensions of the prism with accuracy. It is, however, nearly square, and the height equal to nearly four times the lesser terminal edge if the plane c , be produced by a decrement by one row on the greater edge of the terminal plane.

The measurement of M on P	is	90° 00'
M T		90 00
M a	about	135 20
a a'	about	90 40

The cleavages parallel to M and T are effected with great facility, and the planes afford very distinct reflections.

H. J. BROOKE.

ARTICLE VI.

Exposition of the Atomic Theory of Chemistry; and the Doctrine of Definite Proportions. By William James Macneven, M.D. Professor of Chemistry and Materia Medica in the College of Physicians and Surgeons of the University of the State of New-York.

1. IT has been known, ever since chemists acquired the art of analyzing bodies, that substances always enter into chemical combination in determinate proportions. Nitrate of potash, under all circumstances and in all situations, consists invariably of 54 parts nitric acid and 46 potash, per cent. Common salt of 46 muriatic acid and 54 soda; no matter whether the salt was taken from the sea, or from a spring, or artificially made, these proportions of the constituent parts are uniform. Carbonate of lime is always found to consist of 43.2 carbonic acid and 56.8 lime. Sulphate of barytes of 34.5 sulphuric acid and 65.5 barytes. The more rigorously this law has been examined by accurate analysis, the more conspicuous and decided have become the proofs of its reality. It is, therefore, universally admitted; indeed, it is obvious, that if there was not a law of nature to determine and preserve these fixed proportions, there could be no uniformity in compound bodies: but the uniformity and fixedness of the law necessarily depend on some ulterior cause which renders all other combinations impossible. Now, this cause, whatever it be, must constitute the principal basis of chemical philosophy, and well understood and rigidly applied, it would introduce mathematical precision into our chemical reasonings and conclusions. In ascending to the cause we can assign it no other residence than those elementary particles of matter which are so constituted as to be exempt from decay or change, though they are capable of being variously compounded together and separated again, so as to give origin to the perpetual transitions of elementary into organized matter; but the nature of the elements themselves is immutable; the forms only can vary. The indefatigable alchemist frequently exhausted every device and process, endeavouring, with keen research and prodigal talent, to alter the nature of matter, and transmute one species of metal into another, but he was eternally foiled in this chimerical attempt.

The productions of nature have not only succeeded one another in the same general order, but have been from the beginning invariably the same. An oak of the present time has the same general nature and the same properties as those of all other oaks that ever existed; we find the same texture in its wood and bark; a similar disposition in general in its root and branches; the leaves have the same form; the juices the same

astrigent power; the fruit is moulded to the same shape of an acorn, and has the same invariable property of never producing any other tree than an oak.

All this shows that the elements of bodies are permanent and unchangeable. Had they been liable to any gradual alteration or waste, the oaks of the present times, composed of those changed materials, would not be found to have the same qualities as the oaks of remote ages; and the order and course of nature, as well as the qualities of her productions would have been different from what they have uniformly appeared, and what we actually find them.

Such obvious reflections on the course of nature have, in the earliest ages of philosophy, suggested the supposition of a certain number of unchangeable elements, of which it was imagined all things were composed, and on the successive separations and reunions of which depended the decay and reproduction of all natural objects. This was the meaning of the opinion ascribed to Democritus, that all things were formed of atoms.*

We are consequently warranted by the phenomena in assuming that the ultimate particles of matter are so perfectly hard and minute as never to wear or divide. It is only by continuing entire that the particles may form bodies of the same nature and texture in all ages. Should they wear away, or break in pieces, the nature of all things depending on them would change incessantly, and, contrary to experience and fact, there would be no permanent species of matter. But since the ultimate particles, which henceforth we shall call atoms, are indivisible and indestructible, we may be permitted to deem them also simple.

2. In the chemical combination of different substances the *atoms* of those bodies unite together, and this is what causes them to be dispersed through the whole mass. Chalk is composed of lime and carbonic acid; now, how minute a portion soever of chalk we take, we shall find it to contain both lime and carbonic acid. How minute a portion soever of water we take, we shall find it to contain both oxygen and hydrogen. How minute a portion soever of saltpetre we take, we shall find it to contain both nitric acid and potash. Hence, it necessarily follows, that in a chemical compound of the simplest composition every atom of one ingredient is united to one or more atoms of the other.

3. The extent of this combination is limited. Thus, if a dilute solution of potash be added to an ounce of sulphuric acid, a point is observable, at which the potash and acid lose their peculiar characters, the one of turning vegetable blue colours green, the other of making them red, and a liquid is left of a bitter taste, affording a bitter salt, which crystallizes in short hexangular prisms. If an additional quantity of potash be now added, it

* Black's Lectures, vol. ii. p. 4.

effects no change in the nature of this salt, but remains distinguishable itself as potash; or if, instead of potash, there be added an excess of acid, it does not alter the nature of the salt, but is recognized, in like manner, by its own acid character. The quantity of one substance entering into chemical union is limited, therefore, by that of the other. To this phenomenon the older chemists, expressing the fact only, gave the name of *saturation*. And having further observed that an ounce of different acids require unequal quantities of potash for saturation, they established that the point of saturation varied with the substance, and that it was different in different bodies.

Modern chemists have given to this law the name of *definite proportion*, meaning thereby to express a fact which they have traced more extensively and correctly than their predecessors. Indeed all chemical compounds contain the same proportion of constituents with the most rigid accuracy, no variation ever taking place in this respect. The whole science of chemistry is founded on the permanency of chemical compounds.

4. Richter, in his geometry of the chemical elements, gave great extension to the doctrine of definite proportions, and enriched it by a vast number of new and important observations. He observed that when two neutral salts which mutually decompose each other are mixed together, the two newly formed salts still retain the same neutral state as the two original ones from which they were formed. He drew up the results of many experiments in tables exhibiting the weight of each base, capable of saturating 100 parts by weight of each acid, and the weight of each acid capable of saturating 100 parts by weight of each base. He found that the different bases follow exactly the same order in each of the tables, and, he observed further, that the numbers in each table constitute a series which have the same ratio to each other in all the tables. Suppose, for example, that in the table representing the muriates, the quantity of potash requisite to saturate 100 parts of muriatic acid were three times as great as the quantity of alumina requisite to produce the same effect; the same thing would hold in the sulphates, nitrates, and all the other genera of salts. Three times as much potash would be required to saturate 100 sulphuric, nitric, or any other acid, as would be requisite of alumina.

These facts explain how it happens that when two neutral salts decompose each other, the new-formed salts are also neutral, and why there is no excess of acid or base upon the one side or the other. The same proportions of bases that saturate a given weight of one acid, saturate all the other acids; and the same proportions of acids that saturate one base saturate all the other bases. Hence, numbers may be attached to each acid and base indicating the weight of it, which will saturate the numbers attached to all the other acids and bases. This is the foundation of Dr. Wollaston's sliding rule of chemical equivalents.

6. Mr. Dalton, in 1804, turned his attention to the subject, and was struck with the small number of proportions in which simple substances are capable of uniting, and the constancy of these proportions. Thus, if we represent the weight of carbon by 75, we find that carbonic oxide and carbonic acid, the only two compounds of carbon and oxygen, are composed as follows :

Carbonic oxide of 75 carbon + 100 oxygen
Carbonic acid of 75 carbon + 200 oxygen

So that the quantity of oxygen in carbonic acid is to that in carbonic oxide as 2 to 1.

If we represent the weight of nitrogen by 175 we find that all the compounds of nitrogen and oxygen are composed as follows :

Protoxide of nitrogen 175 nitrogen + 100 oxygen
Deutoxide of nitrogen 175 + 200
Hyponitrous acid . . . 175 + 300
Nitrous acid 175 + 400
Nitric acid 175 + 500

So that the quantity of oxygen in these compounds, supposing the nitrogen to remain always the same, is as the numbers 1, 2, 3, 4, 5.

Similar observations may be made respecting the composition of the metallic oxides, the chlorides, the neutral salts, and all chemical combinations with which we are acquainted.

The fortunate thought occurred to Mr. Dalton "that those proportional numbers represented the respective weights of the atoms of the combining bodies;" that bodies combine either 1 atom of one with one atom of another, or with 2 atoms, or with 3, 4, 5, or 6 atoms. According to this notion, if we represent the weight of an atom of carbon by 0.75, an atom of oxygen will be 1, and carbonic oxide will be a compound of 1 atom carbon and 1 atom oxygen, and carbonic acid of 1 atom carbon and 2 atoms oxygen. If the weight of an atom of nitrogen be 1.75, and that of oxygen 1, then the compounds of nitrogen and oxygen are composed as follows :

Protoxide of nitrogen . . 1 atom nitrogen + 1 atom oxygen
Deutoxide of nitrogen . . 1 + 2
Hyponitrous acid 1 + 3
Nitrous acid 1 + 4
Nitric acid 1 + 5

The simplicity and beauty of this opinion made a speedy and strong impression upon chemists in general. Its truth is now universally admitted.*

6. Mr. Higgins, Professor of Chemistry in Dublin, in a work published by him in 1789, made a near approach to the atomic

* Thomson; vol. iii. p. 19. Fifth Edition.

theory. He delivered the composition of sulphurous and sulphuric acids, the composition of water and the compounds of oxygen and nitrogen nearly as they are now received. But his own experiments were too few, and there were not data enough furnished by others to supply sufficient materials for the construction of the atomic theory. During the 21 years that elapsed between the publication of the comparative view of Mr. Higgins and the elements of chemical philosophy by Dalton, with more industry, the former might, perhaps, have secured to himself the reputation for which he is now so solicitous; but, if liberal and candid, he will confess that celebrity without labour is neither legitimate nor attainable in the walks of science, and that the fame of a discovery belongs rightfully to him, who is not only the most sedulous, but also the first to promulgate and apply it. Without being the discoverer of the atomic theory, no man in Britain has done so much for it as Dr. Thomson; and by Berzelius and Gay-Lussac its principles have been carried as much beyond where Dalton left them, as he exceeded the scanty suggestions furnished by Mr. Higgins. It is now every where received, and constantly improving.

7. The invariable permanency of chemical compounds cannot be owing to any thing else than the union of a certain determinate number of the atoms of one constituent with a certain determinate number of the atoms of the other. The proof of this position will be more easy and striking, if we first take an example from the union of gaseous bodies, the atoms of which, unrestrained by the force of cohesion, are at liberty to arrange themselves according to their chemical affinities.

Let the compound to be investigated be water, which is known to result from the condensation of oxygen and hydrogen gases, when they are ignited together. We shall denote the atoms of oxygen which unite by x , and those of hydrogen by y , and then an integrant particle of water will, in every case, be $x + y$.

The numbers x and y are easily found by making an accurate analysis of the different compounds into which various proportions of oxygen and hydrogen enter. Let us take water. It is known from experiment that 100 cubic inches of oxygen gas, weigh 33·888 grs. and that 100 cubic inches of hydrogen weigh 2·117 grs. If these volumes be mixed together and the electric spark passed through them, there will be a condensation of all the hydrogen and half the oxygen; so that 100 cubic inches, or 2·117 grs. of hydrogen can condense no more than 50 cubic inches, or 16·944 grs. of oxygen. But, if we mix 2 volumes of hydrogen, or 4·234 grs. with 1 of oxygen, and then pass the electric spark, there will be a complete condensation of both the gases, and a portion of water left, precisely equal to their joint weights; consequently the weight of oxygen in water is to the weight of hydrogen in water, as 16·944 is to 2·117, or as 33·888 to 4·234, all which, reduced to their lowest terms, are as

8 to 1. Now, if water be a combination of 1 atom of oxygen to 1 atom of hydrogen, it follows that the atom of oxygen weighs eight times as much as the atom of hydrogen.

8. We may give to this reasoning a more general form by putting y for any volume of hydrogen, and x for the same volume of oxygen; then there are in water, by experiment, $2y + 1x$; and since $1x$ weighs $16y$, the constituents of water may be expressed in weights of y ; thus, $2y + 16y = 18y$, the whole weight of water. Supposing this 100, we have $18y = 100$ and $y = 100 \div 18 = 5.555$, weight of $1y$, or one volume of hydrogen; but there are $2y$, or two volumes; consequently $5.555 \times 2 = 11.110$ gives the weight of hydrogen in 100 of water.

If we subtract the weight thus found from the compound, we have $100 - 11.110 = 88.890$, the weight of oxygen in 100 of water.

Now $88.890 : 11.110 :: 8 : 1$ nearly.

9. The weight of an atom of oxygen or hydrogen may also be deduced from the specific gravity of those gases, and it is the more usual way, for the weights of equal volumes are to each other as the specific gravities. Thus the specific gravity of hydrogen being 0.0694, and that of oxygen 1.111. If we take y to represent the weight of any number of atoms of hydrogen in a volume, we shall have this proportion $0.0694 : 1.111 :: y : 1.111 \times y \div 0.0694 = 16y$; and if $16y$ equal the weight of the atoms in a volume of oxygen, $y \times 2 = 2y$ is the expression for the weight of an equal number of the atoms of hydrogen; but $2y : 16y :: 1 : 8$. Therefore, the atom of hydrogen is but one-eighth of the weight of an atom of oxygen.

10. Another mode of obtaining the relative weights of the atoms from the specific gravity of their gases is to compare the specific gravity of each gas with the specific gravity and weight of the common standard atmospheric air; and as they are to one common third weight they will be to one another, thus:

$$\begin{array}{l} 1.000 : 1.111 \quad :: 30.5 : 33.888 \text{ weight of oxygen} \\ 1.000 : 0.0694 \quad :: 30.5 : 2.117 \text{ weight of hydrogen} \end{array}$$

The numbers 1.000 represent the specific gravity of atmospheric air; 1.111 that of oxygen gas; 30.5 the weight in grs. of 100 cubic inches atmospheric air; and 33.888 the weight of the same measure of oxygen gas.

11. The same law is observed by other elementary bodies in a gaseous state. They unite in such proportions that one volume of the gas of one combines with an equal volume of the gas of the other, or with two or with three volumes, &c. of that gas without any intermediate fraction; so that in these combinations one at least of the elements ought always to be considered as unity. According to the atomic theory, this means that an atom of one of the elements combines with one or more entire atoms of the other, for there can be no fraction of an atom.

The fact, that in every inorganic combination one of the elements enters as unity, is founded on numberless experiments, and is a deduction from the analytical researches of some of the ablest chemists, particularly of Berzelius, who verified it in all the inorganic bodies which he analyzed.

We have seen how strikingly this law is exemplified in the union of oxygen and hydrogen gases, and that the proportion in which they combine is definite.

12. Whereas the atoms of one kind of gas repel each other among themselves, and those of different gases attract each other, it follows that when they are mixed equably, as is the case when 2 measures of hydrogen and 1 of oxygen gas are put into a tube and fired by electricity, they will most readily unite atom to atom. Until very lately there was no combination of oxygen and hydrogen known, but that in which they exist in water; it was, therefore, concluded that this was the proportion in which they unite most readily, and with the greatest force. Of late, a new compound of oxygen and hydrogen has been formed by Thenard, consisting of one atom of hydrogen with more atoms of oxygen. This discovery tends fully to confirm the former opinion, viz. that the first formed combination is one of atom to atom.

13. It also follows from a consideration of the volumes in which oxygen and hydrogen gases combine, that a given bulk of hydrogen contains only one half the number of atoms that exist in the same bulk of oxygen gas.

Here it may not be superfluous to remind the student that the density of gases differs like that of other bodies, and that under the same volume they contain unequal quantities of matter. A remarkable example of the fact is afforded by ammoniacal gas, which can be expanded by repeated shocks of electricity to almost twice its original bulk. Consequently, in this new state its atoms are nearly twice as distant as they were at first, and any given measure of it contains now but about half the number of atoms which the same measure contained before.

14. Out of the consideration of this difference naturally arises that of the cause which keeps asunder the atoms of gases and of all other bodies; for, however strong we may deem their affinity, they can never touch. On the supposition that two atoms were in actual contact, their attraction for each other would not only be as great as possible, but as great as the attraction of any other body, for either of them, could possibly be; because, by the supposition, they cannot be nearer. Consequently since bodies chemically combined can be separated, they are not in actual contact, but their distance from one another may vary in different cases, and then the force of affinity will vary with the distance. All bodies are diminished in bulk by cold; it brings their particles nearer together, which would be impossible unless they had been at some distance before they were cooled. This

reduction of their volume by cold, favours the idea of caloric interposed between those particles being the cause of their not coming in contact.

15. If oxygen, hydrogen, and nitrogen gases be mixed in the same vessel, and the electric spark passed through them, a combination of oxygen and hydrogen takes place in preference to a combination of oxygen and nitrogen. The reason seems to be that there is a force of attraction or affinity inherent in every atom of matter, and that in different bodies it differs in intensity.

16. When gases are made to unite, that gas which enters into the compound in the smallest quantity in bulk is represented by 1, and then the bulk of the other constituents of the compound is 1, 2, 3, 4, &c. Here atoms and volumes are deemed to bear to each other the same exact ratio, as we have distinctly found in the example of oxygen and hydrogen gases. It is held that two atoms of one substance do not unite to two atoms of another, for this would resolve itself into atom to atom; nor two of one to three of another, which would amount to the union of an atom of one substance to one and a half atom of another. This cannot be, since the atom is indivisible. Were it possible to divide atoms, or to form combinations with their fractional parts, the compounds of various oxides would be so multifarious that all traces of chemical proportion would disappear, or be only occasional, whereas it is found to be perpetual and certain.

17. Knowing the weight of an atom of oxygen and of an atom of hydrogen, we have it in our power to determine the weight of an atom of the other substances which unite with oxygen, or with hydrogen, or with both. One hundred parts of sulphur unite with two well known proportions of oxygen, the first consisting of 100 oxygen, the second of 150 oxygen both in weight. Here the proportions of oxygen being to each other as $1 : 1\frac{1}{2}$, or as $2 : 3$, it was reasonable to suppose that the first proportion represented two atoms of oxygen, and the second of three atoms, and that there was another compound consisting of one sulphur with one oxygen. Such a compound, predicted by the theory, has been recently discovered, though not obtained in a separate state as yet. Hence it follows, that the weight of sulphur which enters into these combinations represents the proportional weight of an atom of that substance. Then the combinations of oxygen and sulphur will stand thus :

100 sulphur + 50 oxygen hyposulphurous acid
 100 sulphur + 100 oxygen sulphurous acid
 100 sulphur + 150 oxygen sulphuric acid

And if 100 represent an atom of sulphur, it is double the weight of an atom of oxygen; accordingly the weight of an atom of sulphur is represented by 16 in reference to hydrogen, or by 2 when oxygen is taken as 1.

18. The combination of sulphur with hydrogen leads to the

second loses half its carbonic acid in a moderate heat, whereas the first half is retained even in a red one. The carbonate consists of one atom base and one atom acid, but the bicarbonate of one atom base and two atoms acid. The union of one atom base and one atom acid is energetic, but that of one atom base and two atoms acid is easily broken.

21. When a body has the property of uniting with various doses of oxygen, we are best enabled to determine the number of atoms which constitute the compounds. Thus manganese unites with four doses of oxygen, and if the manganese be represented by 100, the oxygen of each respective oxide is found to be 14, 28, 42, 56. These numbers are in arithmetical progression, having the common difference 14, and are to each other as 1, 2, 3, 4. Hence, the first oxide is composed of one atom manganese and one atom oxygen; the second of one manganese + two oxygen; the third, of one manganese + three oxygen; and the fourth, of one manganese and four oxygen. It is observable that the fourth proportion of oxygen is very easily separated, the second and third are less and less so, and the separation of the first, or the total decomposition of the oxide is extremely difficult.

In like manner as mercury unites with two doses of oxygen and forms two oxides, the first composed of 100 mercury and 4 oxygen, and the second of 100 mercury and 8 oxygen; the first must be a compound of one atom mercury + one atom oxygen, and the second of one atom mercury + two oxygen.

There are two oxides of iron; the first composed of 100 iron and 28 oxygen; the second, of 100 iron and 42 oxygen. Now, as $28 : 42 :: 2 : 3$, it follows that the first is composed of 100 iron and 2 atoms oxygen, and the second of 100 iron and 3 atoms oxygen; and we may infer from analogy that there is a lower oxide consisting of 100 iron and 1 atom oxygen.

22. When once we know the gross number of atoms and the amount of each kind in a compound, their proportional weight is easily determined. Thus, if black oxide of mercury be composed of one atom mercury and one atom oxygen, and that 100 mercury combine with 4 oxygen, which experiment proves it does; then the weight of an atom of mercury is to the weight of an atom of oxygen as 25 to 1; and the weight of an atom of black oxide will be represented by $25 + 1 = 26$.

23. If water be composed of one atom oxygen and one atom hydrogen, and if the weight of the oxygen in water is to that of the hydrogen in water, as 8 : 1, then it follows that an atom of oxygen is eight times as heavy as an atom of hydrogen.

24. Such is the method of determining the weight of an atom of the different substances best known to us by accurate experiment. It enables us to calculate the proportions of the constituents of all compound bodies. It is a standard with which to compare our experiments and try their accuracy. Those that

have been conducted with the greatest care, and by the ablest experimenters, approach the closest to the conclusions of the theory. The moment it is unequivocally entitled to confidence, it becomes of the greatest assistance to the practical chemist; for, by determining one or two constituents of a compound, others will be had from calculation with as much certainty as we can get the inaccessible side of a triangle from two angles and one other side.

25. The proportions in which the elementary atoms unite are very limited; almost the whole of them have been examined without finding an exception to the law of their combination. Elementary atoms when they combine, form compound molecules of the first order; when these unite, they form compound molecules of the second order. The combinations of the latter are greatly diversified, varying in their elements as to the number of compound atoms which they contain. Berzelius examined a vast number of these substances, and as they are for the most part composed of oxides, he paid particular attention to the oxygen they contain.

From the numerous analytical researches of this eminent philosopher, aided by those of Gay-Lussac, and several other distinguished chemists, certain canons have been deduced which are strictly conformable to the atomic theory, and render its applications exceedingly beneficial to the operative chemist.

26. When gaseous bodies combine, they always unite in determinate proportions; and if we represent the bulk of the gas that enters into the compound in the smallest quantity in bulk by 1, then the bulk of the other constituent is either 1, 2, or 3, &c. Thus, muriate of ammonia is composed of 1 muriatic gas + 1 ammoniacal gas; carbonate of ammonia of 1 carbonic acid gas + 1 ammoniacal gas; nitrous gas of 1 nitrogen + 1 oxygen; water of 1 oxygen + 2 hydrogen; nitrous acid of 1 nitrogen + 2 oxygen; sulphuric acid of 1 oxygen + 2 sulphurous acid gas, because the sulphurous acid gas consists of 1 sulphur + 2 oxygen, carbonic acid of 1 oxygen + 2 gaseous oxide of carbon, for the latter is formed of 1 carbon + 1 oxygen; ammonia of 1 nitrogen + 3 hydrogen. This canon has been established by Gay-Lussac in a satisfactory manner.

The same result is observable in all the compounds of inorganic matter, one of the constituents of which is uniformly in the state of a single atom. This law greatly simplifies the doctrine of atomic combinations, as far as inorganic bodies are concerned, and reduces the whole to a state of elementary facility.

27. The quantity of acid requisite to saturate the different metals is directly as the quantity of oxygen which these metals require to convert them into oxides. Thus 100 parts of mercury require 4.16 parts of oxygen, and 100 parts of silver require 7.9 parts of oxygen, to convert them into oxides. Therefore, the

quantity of acid necessary to saturate 100 parts of mercury is to the quantity necessary to saturate 100 parts of silver as the number 4.16 to 7.9. This law was first pointed out by Gay-Lussac. Dr. Thomson expresses it in the following manner, the better to adapt it to the purposes of the chemist: "When different metallic oxides saturate the same weight of acid, each contains exactly the same weight of oxygen."

According to Berzelius, in order to saturate 100 parts muriatic acid, any metal whatever must be combined with 42 parts of oxygen. To saturate 100 parts of sulphuric acid, any metal whatever must be combined with 20 parts of oxygen.

This law necessarily implies that when an acid unites to a base, the oxygen in the acid is always a multiple of the oxygen in the base by a whole number; and generally by the number denoting the atoms of oxygen in the acid. Thus, sulphuric acid is composed of 1 atom sulphur = 16, and 3 atoms oxygen = 24, and 100 parts of sulphuric acid containing 60 oxygen (24 : 16 :: 60 : 40) will combine with and saturate a quantity of base which contains 20 oxygen. Now, 20, the oxygen in the base, multiplied by 3, the number of atoms in sulphuric acid, makes 60, the quantity of oxygen in 100 parts of sulphuric acid.

28. When sulphur combines with a metal, the proportion remains unchanged, though the sulphur be converted into an acid, and the metal into an oxide. Thus, the proportion of metal and sulphur in sulphate of copper is the same as in sulphuret of copper; for the protosulphuret of copper is composed of 1 atom sulphur + 1 atom copper; and the sulphate of the protoxide of copper is composed of

1 atom sulphur + 3 atoms oxygen (acid)

1 atom copper + 1 atom oxygen (protoxide or base.)

in which the sulphur and copper do not vary.

This law which is of great importance in practical chemistry, and very much facilitates the analysis of the metalline salts, was first pointed out by Berzelius.

29. The oxygen in a metallic protoxide is equal to half the sulphur in the sulphuret of the same metal, supposing the weight of the metal in both cases to be the same. This canon was first specified by Berzelius. It depends on the fact that an atom of sulphur is twice the weight of an atom of oxygen; and it is limited to those cases where the protoxide is a compound of 1 atom of metal and 1 atom of oxygen; and the sulphuret of 1 atom of metal and 1 atom of sulphur. This canon enables us to determine the constitution of the sulphurets by means of the oxides, and *vice versá*.

30. In combinations of two bodies, each containing a quantity of oxygen, the weight of oxygen in each body is either equal, or one contains twice, thrice, four times, &c. as far as eight times

the quantity of oxygen in the other. This law has been laid down by Berzelius, and indicates a most important regularity in the relative weights of the atoms of bodies.

31. Water is capable of combining both with acids and bases; when it unites with an acid it acts the part of a base, and contains the same quantity of oxygen that the base would contain. Therefore, the least quantity of water that can combine with sulphuric acid is 22.5 water to 77.5 acid; for $8 : 9 :: 20 : 22.5$. That is to say, 8, the atom of oxygen is to 9, the integrant particle of water, as 20 oxygen is to 22.5 water; and the water forming a base, in 100 sulphuric acid, it must contain 20 oxygen. This acid has, therefore, no more water than is barely sufficient for its formation: hence it is the strongest possible. Such compounds are called hydrates. This canon has also been laid down by Berzelius.

32. In combinations composed of more than two bodies containing oxygen, the oxygen of that constituent which contains the least of it is a common divisor of all the portions of oxygen found in the other bodies. This law, likewise laid down by Berzelius, depends on the fact that oxygen always unites by atoms, whence it follows that any quantity of oxygen will always be divisible by one atom of oxygen. Dr. Thomson observes that if we were accurately acquainted with the constitution of the earths, this law would be of great use to the mineralogist. It would enable him to distinguish between chemical combinations and mechanical mixtures.

33. When two combustible bases unite, they always combine in such a proportion that when oxidized, either the quantity of oxygen uniting with each will be the same, or the oxygen in the one will be twice, thrice, &c. that in the other. This is another law laid down by Berzelius, and depends on this fact that the two bodies must unite, atom to atom, or a certain number of the atoms of the one must combine with one atom of the other. Dr. Thomson applies this law to determine which of the metallic alloys are chemical combinations, and which are mechanical mixtures. There can be no doubt that copper and zinc combine chemically. Now the weights of the atoms of these metals are:

Copper	8.000
Zinc	4.315

Therefore, if they unite atom to atom, brass ought to be a compound of 100 copper and 53.93 zinc, $8 : 4.315 :: 100 : 53.93$. Actual analysis shows this to be very nearly the proportion of the ingredients.

34. Those are the canons of Berzelius, but founded on analysis. (Thomson's *Annals*, ii. p. 40.) By means of them he determines the proportion of oxygen in bodies, and the number of atoms of which they are composed. These laws give a facility and elegance to our chemical investigations scarcely to be

expected; and they must be admitted until some exception to them be discovered.

It is a circumstance much in favour of the atomic theory, that it assigns a mechanical and very satisfactory cause, why elementary atoms unite only in proportions which are multiples of each other. The compound molecules which contain oxygen combine likewise in a multiplex ratio, if we attend only to the oxygen they contain. This must be owing, in all probability, to a cause similar to that which occasions the like proportions between elementary atoms themselves.

The proportion of oxygen, in the oxides that unite, is a problem highly important to be verified. The investigation has accordingly occupied and still continues to occupy the attention of the ablest chemists with a result the most satisfactory.

Indeed but for the law which shows that the oxygen in an acid is always a multiple of the oxygen in the base by a whole number, no combination composed of several oxides could be calculated, nor any analysis verified in a decisive manner for the theory.

35. The employment of numbers facilitates the expression of chemical proportions, and, by determining the weight of the elementary atoms, figures exhibit the numeric result of an analysis in a manner at once simple and easily remembered. But, in order to draw up a table of the relative weights of the atoms of bodies, some one must be selected for comparison whose atom shall be denoted by unity.

There are only two elementary bodies possessed of the requisite qualities to serve as our unit. These are oxygen and hydrogen. Mr. Dalton has made choice of hydrogen, because it is the lightest of all known bodies. Sir H. Davy embraced it from him, but changed its value; and Mr. Brande, in his *Manual of Chemistry*, lately published, has likewise adopted the unit and computation of Dalton, but designates the relation of elementary parts by the term *proportionals*. This choice was not the happiest, for hydrogen has disadvantages from which oxygen is free. The weight of an atom of hydrogen is so small, that if we employ it as our unit, the number representing an atom of some of the metals becomes inconveniently great.

Besides, hydrogen enters much less frequently into compounds than oxygen, and, of course, the unit of comparison, when applied to hydrogen, does not nearly so much facilitate calculation as when it is applied to oxygen. Add, that oxygen constitutes among elementary bodies a particular class; and is, as it were, the centre round which chemistry turns. It exists in the greater number of inorganic bodies, and, without exception, in all the products of organic nature. For these reasons Berzelius preferred this unit, as most convenient and most agreeable to the scientific views of chemistry. He represents it by 100. (Thomson's *Annals*, ii. 451.)

Dr. Wollaston and Prof. Thomson, for similar reasons, have both adopted oxygen as the most convenient unit; nor can there be any hesitation in embracing their decision. Oxygen is, in fact, the substance by means of which the weight of the atoms of all other bodies is determined; hence, the great advantage, for the practical chemist, attending a convenient number for that body.

Much confusion has arisen in this department of the science, not only from the diversity of the unit, but also from the circumstance of different chemists having taken the same unit of different values. Berzelius, as already observed, takes it at 100; Wollaston at 10; and Thomson at 1. There is, indeed, no real difference between the three last, for any one of them may be converted into the other without an alteration of figures, by simply changing the place of the decimal point. Yet it is much to be desired that the same numbers were steadily employed by all persons, as they would soon be recollected by chemists who would thus have a ready recollection of every compound without the trouble of referring to a book.

36. Having observed in my lectures to the students of this college, that the doctrine of atoms was sooner made familiar to the imagination, when the numbers representing their proportional weights are reduced to their lowest terms, and being also more easily remembered when thus expressed, I have preferred the unit adopted by Dr. Thomson.

37. Gay-Lussac and Thenard, to whom, next to Berzelius, the corpuscular theory of chemistry is most largely indebted, have not failed, with many valuable improvements, to introduce a further perplexity by determining chemical proportions in volumes and not in atoms. Their method is indeed founded on a fact discovered by Gay-Lussac; namely, that bodies when in a state of gas unite either in equal volumes, or 1 volume of one combines with 2, 3, &c. volumes of the other; a fact which has been verified by several other distinguished chemists. Berzelius also prefers the computation in volumes; and though in the meaning of those very eminent chemists volumes are but another name for atoms, yet it injures the unity and simplicity of the doctrine to represent essential principles in such diversified forms and expressions. It is true that in the present state of our knowledge, the theory of volumes has the advantage of being founded on a well constituted fact, and it admits of our taking a demi-volume in calculation; while, in the theory of atoms, a demi-atom is an absurdity. But then it is a very forced and unnatural assumption to represent all bodies in a state of gas, even those which were never known to assume that form, or which can be supposed ever to exist in it, in order to determine the proportional weights of their constituent parts. Hence, as there is in reality no difference intended by the theory of volumes, but that of representing bodies in a gaseous, which

the theory of atoms represents in a solid form, I prefer the theory of atoms as subject, on the whole, to fewer difficulties.

38. Oxygen, 1.

39. It was proved in sections 7, 8, 9, that hydrogen is but one-eighth parts as heavy as oxygen. Hence, by dividing 8 into 1 we obtain the fractional proportion for hydrogen 0.125.

40. *Carbon.*—When carbon is burned in oxygen, the volume of the gas is not altered, but it is converted into carbonic acid gas. Hence, if from the weight of 100 cubic inches of carbonic acid gas = 46.313 grs. we subtract the weight of 100 cubic inches of oxygen gas = 33.688, the difference 12.641 grs. gives the weight of carbon in 100 cubic inches of carbonic acid gas. It shows that this gas is composed, per cent. of 27.29 parts carbon and 72.71 oxygen, for 46.313 carbonic acid is to 12.641, its proportion of carbon, as 100 carbonic acid is to 27.29, its proportion of carbon; and by subtracting the carbon, the remainder 72.71 gives the oxygen.

There is also another combination of oxygen and carbon necessary to be considered before we can determine the proportional number for carbon. It is carbonous oxide, a gas that may be formed by partially decomposing carbonic acid.

When a well dried carbonate, such as carbonate of barytes, or lime, is distilled with dry iron filings, the iron attracts a part of the oxygen of the carbonic acid of the carbonate, and the remainder, minus the portion of oxygen thus taken from it becomes carbonous oxide; a gas containing less oxygen, therefore, than carbonic gas. But if to the same carbonous oxide half its volume of pure oxygen gas be now added, and the electric spark be passed, it becomes carbonic acid gas again. In these two experiments the carbon has not been affected; it is the same in both, the oxygen only has been partially taken off in the first, it is restored in the second. Now, since there is the same weight of carbon = 12.641 grains in 100 cubic inches of both, deducting this from the weight of both, we get the weight of oxygen in each.

Thus $*46.313 - 12.641 = 33.688$ the oxygen in 100 carbonic acid; and $†29.158 - 12.641 = 16.517$ the oxygen in 100 carbonous oxide. But as 16.517 is very little more than half 33.688, it is proved that carbonous oxide contains but half the oxygen of carbonic gas, and, therefore, that there are twice as many atoms in the latter as in the former; and that if the first be a binary compound consisting of an atom carbon and an atom oxygen, the second is a ternary compound of an atom carbon and two atoms oxygen.

To find the proportional weight of the atom of carbon take the oxide of carbon which is binary, and as 16.517 weight of

* Weight of 100 cubic inches carbonic gas.

† Weight of 100 cubic inches carbonous oxide gas.

oxygen is to 12.641 weight of carbon; so is 1, weight of an atom of oxygen to .765 weight of the atom of carbon.

The weights 46.313 and 29.158 may be readily deduced from the specific gravity of those gases.

The specific gravity of carbonic gas = 1.519; that of carbonic oxide = 0.956: now

$$1 : 30.5 :: 1.519 : 46.313$$

$$1 : 30.5 :: 0.956 : 29.158$$

i. e. 1, the specific gravity of atmospheric air is to 30.5, weight of 100 cubic inches of atmospheric air, as 1.519 specific gravity: of carbonic gas is to 46.313 weight of 100 cubic inches of the same gas.

41. *Nitrogen*.—There are five compounds of nitrogen and oxygen gas, which it is necessary to examine, the better to determine the weight of the atom of nitrogen.

1. Protoxide of nitrogen or nitrous oxide gas. It is composed of two volumes of nitrogen and one volume of oxygen. Now one volume, say 100 cubic inches, of oxygen weighs 33.888 grs. and two volumes, or 200 cubic inches of nitrogen, weigh 29.652 $\times 2 = 59.304$ grs.; therefore the protoxide weighs the sum of their weights, 33.888 + 59.304 = 93.272 grs. To find the weight of each constituent in 100 grs. as the compound 93.272 is to 33.888, its proportion of oxygen; so is the compound 100 grs. to 36.3 grs. the oxygen in 100 of the protoxide. Consequently 100 - 36.3 = 63.7, the nitrogen in 100 protoxide.

Protoxide	63.7 nitrogen
	36.3 oxygen
	<hr style="width: 50px; margin: 0 auto;"/>
	100.0

2. Deutoxide, or nitrous gas, composed of one volume nitrogen and one volume oxygen.

The volumes of nitrogen and oxygen weigh as above 29.652 + 33.888 = 63.540, and to find the weight of each constituent in 100 of nitrous gas 63.540 : 33.888 :: 100 : 53.33 oxygen, and 100 - 53.33 = 46.67 nitrogen:

Now let the base, or nitrogen, be the same, = 63.7 in all the combinations of nitrogen and oxygen, and then as 46.67 nitrogen : 53.33 oxygen :: 63.7 nitrogen in the protoxide : 72.7 oxygen in the deutoxide or nitrous gas. It is evident from this, that the oxygen in the nitrous gas is double that in the protoxide, for 72.7 = 36.3 $\times 2$.

3. *Hyponitrous Acid*.—This acid, discovered by Thenard, and by him called pernitrous acid, is composed of 175 nitrogen and 300 oxygen by weight. Then to find the oxygen, the nitrogen being the same as that of the protoxide 63.7; as 175 : 300 :: 63.7 : 109.2. This shows that the oxygen in the hyponitrous

acid is three times that of the oxygen in the protoxide for $36.3 \times 3 = 109.9$.

4. *Nitrous Acid*.—This acid is composed of one volume nitrogen and two volumes oxygen. The volume nitrogen = 29.652 , as above, and two volumes oxygen, or $33.888 \times 2 = 67.776$. Therefore, the nitrous acid here weighs $29.652 + 67.776 = 97.428$. But to find the weight of each constituent in 100, we have this proportion $97.428 : 67.776 :: 100 : 69.56$, oxygen in 100 nitrous acid, and $100 - 69.56 = 30.44$ nitrogen in 100 ditto. Now in order to find the ratio in which the oxygen increases in these combinations, we have given the base 63.7 , and we get the oxygen in nitrous acid belonging to that base by the following proportion :

30.44 nitrogen : 69.56 oxygen :: 63.7 : 145 oxygen in nitrous acid. The oxygen in nitrous acid is, therefore, four times as much as in the protoxide for $36.3 \times 4 = 145.2$.

5. *Nitric Acid*.—This acid is composed of 1 volume nitrogen and 2.5 volumes oxygen, or by weight of 35.12 nitrogen and 100 oxygen. Then to find the oxygen when the nitrogen is 63.7 , we have the proportion as $35.12 : 100 :: 63.7 : 181.3$ oxygen in nitric acid to 63.7 nitrogen. The oxygen in this combination is five times that in the protoxide, for $36.3 \times 5 = 181.5$.

From all these combinations, it may reasonably be assumed that the oxygen enters with the nitrogen into a regular arithmetical increase from the protoxide upwards : thus,

Protox.	Deutox.	Hyponit.	Nitrous ac.	Nitric ac.
36.3 ;	72.7 ;	109.9 ;	145.23 ;	181.3 .

These numbers are to each other as 1, 2, 3, 4, 5, very nearly ; hence we may consider the protoxide as the lowest combination, or as a binary, in which one atom of oxygen unites with one atom of nitrogen, and from it the proportional weight of an atom of nitrogen may be obtained, as 36.3 oxygen : 63.7 nitrogen :: 1 atom oxygen : 1.75 atom of nitrogen.

42. *Phosphorus*.—According to the experiments of Lavoisier and Davy, 100 parts of phosphorus unite with and condense 154 parts oxygen, and form phosphoric acid, which consists, therefore in a hundred parts, of 39.38 phosphorus and 60.62 oxygen ; but $39.38 : 60.62 :: 1 : 1.54$, or else $100 : 154 :: 1 : 1.54$. The atom of phosphorus is represented, therefore, by the proportional 1.54 , or 1.5 . Another, but a more complicated method of arriving at the same result is that of examining the neutral salts which phosphoric acid forms with the different bases. We find then that phosphorus forms with oxygen three acids.

	Phos.	Oxygen.
Hypophosphorous acid	1.5	$+ 1$
Phosphorous acid.	1.5	$+ 2$
Phosphoric acid	1.5	$+ 3$

43. *Sulphur*.—There are two well known combinations of oxygen and sulphur, viz. the sulphuric and the sulphurous acids. It has been shown (sect. 17,) that the latter holds two-thirds the quantity of oxygen contained in the former. It has been also shown that the sulphuric acid contains 100 sulphur + 150 oxygen (ibid.) Hence, sulphurous acid must contain two-thirds of $150 = 100$ oxygen. Now, if we suppose oxygen to unite with sulphur according to a given arithmetical progression, we shall obtain its lowest combination by constantly taking the common difference from the last lowest term until the difference is too great to be deducted, or until nothing remains. Thus, two of the terms found are 150 and 100, their difference is 50; therefore $100 - 50 = 50$ will express the next term lower than 100. It is also the lowest for 50, the term last found $- 50$ common difference = 0; therefore, 50 is the quantity of oxygen uniting with sulphur when the union is the lowest that can be. It is, therefore, a binary, in which 1 atom of oxygen unites with 1 atom of sulphur, and it will be expressed 100 sulphur + 50 oxygen, from which we obtain the proportional weight of an atom of sulphur, as 50 oxygen : 100 sulphur :: 1 weight of an atom of oxygen : 2 weight of an atom of sulphur. For if there be the same number of atoms in 100 by weight of sulphur as in 50 by weight of oxygen, it is evident that the single atoms themselves must bear to each other the same proportion as their multiples do.

44. *Sodium*.—When 100 grs. sodium are thrown into water there is a decomposition of the latter, and 198.30 cubic inches of hydrogen gas are set free; the temperature being = 60° , and the barometric pressure = 30 inches. The hydrogen, as a constituent of water, requires half its volume of oxygen, that is $198.30 \div 2 = 99.15$ cubic inches, which is the quantity of oxygen that unites with the sodium to form soda. But 99.15 cubic inches of oxygen gas weigh 33.6; there soda is composed of 100 sodium + 33.6 oxygen, or taking the mean of several experiments 33.3.

The peroxide of sodium is composed of 100 sodium + 50 oxygen by weight. Now the oxygen in soda (33.3) is to the oxygen in the peroxide of sodium (50) as 2 is to 3 nearly; and if 2 and 3 are the lowest proportional terms to which 33.3 and 50 can be reduced, the 2 must represent two atoms, and the 3 three atoms. The lower numbers than 2 and 3, viz. 1 and 1.5 must be rejected, for there can be no fractional part of an indivisible atom; 2 and 3 are, therefore, the lowest proportionals. This gives soda as a deutoxide, and the peroxide of sodium as a tritoxide. From either of these we may deduce the proportional weight of an atom of sodium. Thus, for the deutoxide, or when 1 atom of sodium unites with two of oxygen.

As 33.3 oxygen : 100 sodium :: 2 atoms weight of oxygen :

6.006 weight of an atom of sodium. Hence 1 atom of sodium = 6.006.

Whatever be the state of oxidation of soda, the peroxide of sodium must be the next greater, for there can be no intermediate one between two and three atoms.

(To be continued.)

ARTICLE VII.

*Chemical Examination of Wodan-Pyrites.** By M. Stromeyer, Professor of Chemistry in Gottingen.

M. STROMEYER laid before the Royal Society of Gottingen, in a meeting held on March 16, 1820, the result of his experiments on the wodan-pyrites from Töpschau, in Hungary. It is known that Mine-commissioner Lampadius, of Freiberg, detected a new metal in this ore, differing as much from nickel as tellurium does from antimony, and to which he has given the name of *wodanium*. In consequence of this discovery, Mr. Inspector Breithaupt, of Freiberg, has been induced to give to this ore the name of *wodan-pyrites*. According to the experiments of Lampadius, it contains 20 per cent. of the new metal. Its other constituents are sulphur, arsenic, iron, and nickel, but no cobalt can be detected in it though it passes in Hungary for an ore of cobalt.

Through the goodness of Professor Monteiro, of Coimbra, M. Stromeyer had the good fortune to obtain a specimen of this very rare mineral, which this distinguished mineralogist received in a present from Lampadius himself during his residence at Freiberg. He sacrificed his specimen to a chemical analysis in order that by some further experiments the chemical properties of the new metallic substance detected in it by the Freiberg chemist might be more accurately known.

Though M. Stromeyer conducted his experiments with every possible care and attention, he was unable to discover in this ore a new metal possessing properties different from those of nickel, and distinguishing wodan-pyrites from other bodies. As there can be no doubt that the ore which he examined is the very same in which Lampadius detected his *wodanium*, it seems to follow as a consequence that wodanium must be placed in the same rank with *niccolanum* and *vestium*, two supposed metals, the existence of which was afterwards disproved.

In other respects this ore differs in its composition from all known ores of nickel. The discovery of it, therefore, though it

* Translated from Gilbert's *Annalen der Physik*, lxiv. 338, for March, 1820.

contains no new metal, serves to enrich mineralogy. According to the analysis of M. Stromeyer, 100 parts of this ore are composed of the following constituents :

Nickel	16·2390
Cobalt, with some manganese.	4·2557
Iron	11·1238
Copper	0·7375
Lead.	0·5267
Antimony	Trace
Arsenic	56·2015
Sulphur	10·7137
	99·7979

It is probable from this result, that in the Hungarian ore the nickel and cobalt, together with a portion of the iron, are united to the arsenic ; while, on the other hand, the manganese, copper, lead, antimony, and the rest of the iron, exist in the state of sulphurets. The considerable proportion of iron and sulphur renders it not unlikely that the arseniurets, as well as the other sulphurets, are held in solution together in the sulphuret of iron. Of course, if we regard only the nature of the mineral, the woda-pyrites might with propriety be denominated *iron pyrites*, and it approaches nearest to the glanz nickel of Prof. Pfaff.

As to the arseniuret of nickel in this ore, Prof. Stromeyer has shown that it is very probable that the nickel is united with twice as much arsenic as in *kupfernickel*.

ARTICLE VIII.

On the Evaporation of Spirits of different Degrees of Strength.
By William Ritchie, A.M. of the Academy, Perth.

(To Dr. Thomson.)

SIR,

IT is a well-known fact that the evaporation of strong spirits produces a greater degree of cold than the evaporation of weak spirits or water ; but, as far as I recollect, it has not been stated by any author that there exists a uniform ratio between the induced cold and the strength of the evaporating spirits. If you consider the following theorem worthy of a place in your *Annals of Philosophy* you will much oblige, Sir,

Your obedient servant,

WILLIAM RITCHIE.

Theorem.—The degrees of cold induced by the evaporation of spirits of different degrees of strength are proportional to the

strengths of these spirits, reckoning from the degree of cold induced by the evaporation of water.

Having made three very delicate hygrometers, according to Leslie's construction, I moistened the bulb of one of them with strong whisky, the bulb of another with a mixture of equal quantities of the same spirits and water, and the bulb of the third with water. I watched the descent of the fluid in the stem till each had gained its maximum of cold, and marked the cold induced by the water 40, by the dilute spirits 64, and by the strong spirits 88. Now the difference between 40 and 64 is 24, and between 40 and 88 is 48. Hence the following proportion: 24 : 48 :: strength of the dilute, : strength of the strong spirits. This I have tried with different proportions of spirits and water in different states of the atmosphere, and found the same property uniformly obtain. The experiment requires to be performed with great delicacy and care, as the spirits soon acquire their maximum after which the fluid in the stem begins to ascend.

This curious property may, perhaps, be explained by considering the composition of the evaporating fluids. In the case of the dilute spirits the evaporating film consists of an equal number of aqueous and spirituous particles. Now if the cold induced by a complete film of water be 40, the cold produced by the aqueous particles in the dilute film will be 20. Again, if the cold induced by the evaporation of a complete film of spirits be 88, the cold induced by the spirituous particles in the dilute surface will be 44. Hence the cold induced by the evaporation of both will be the sum of 20 and 44, or 64, agreeably to the actual experiment. Leslie's differential thermometer, therefore, besides the various uses to which that ingenious and profound philosopher has applied it, may also be used instead of the hydrometer to determine the strength or specific gravity of spirits. Considering the delicate structure of the instrument, and the difficulty of using it, we do not expect, however, that it will ever supersede the use of that invaluable instrument.

ARTICLE IX.

On Ferrochyzate of Iron. By Thomas Thomson, M.D. F.R.S.

IN a former number of the *Annals of Philosophy* I gave an account of some experiments which I had made to determine the composition of the beautiful blue pigment formed by pouring a solution of ferrochyzate of potash, or of ammonia, into the persulphate, pernitrate, permuriate, or any soluble ferruginous salt, containing the peroxide of iron. These experiments satisfied me, as I trust they would satisfy the readers of the *Annals*,

that the pigment is essentially a compound of one atom of ferrochyazic acid and one atom of peroxide of iron, supposing the weight of an atom of peroxide of iron to be 5. The pigment contains likewise a certain quantity of water, from which it cannot be entirely freed without undergoing at least partial decomposition. Probably there is a certain temperature (about 212°) in which, if it be kept, it would be reduced to a compound of one atom acid + one atom peroxide + one atom water. On that supposition its constituents would be as follows :

1 atom acid.	= 6.750	52.42
1 atom peroxide. ..	= 5.000	38.83
1 atom water.	= 1.125	8.75
	12.875		100.00

The quantity of water in the pigment which I subjected to analysis exceeded the above theoretical quantity by about two per cent. doubtless because I had not been at the requisite pains to dry it sufficiently.

I hinted in a preceding number of the *Annals of Philosophy* at some of the phenomena produced by the exposure of this pigment to a red heat ; but they are so curious and so instructive that I think them well deserving a more particular detail, and shall, therefore, make them the subject of the present paper. It will be recollected that the ferrochyazate of iron, which was the subject of the following experiments, was composed of

Ferrochyazic acid.	51.0	or	25.5
Peroxide of iron.	37.8		18.9
Water.	11.2		5.6
	100.0		50.0

Before proceeding to detail the effects produced on this salt by a red heat it may be necessary to lay before the reader the present state of my knowledge of the constituents of ferrochyazic acid. In vol. xii. of the *Annals of Philosophy*, p. 102, will be found an account of a set of experiments which I made on purpose to determine the composition of this acid. My analysis of the ferrochyazate of potash gave me the following quantities :

Acid	{	Iron.	15.0	
		Volatile matter.	30.9	= 45.9
Potash.				= 41.6
Water.				= 13.0
			100.5	

From this analysis it appears that nearly one-third of the acid consists of iron ; and that the weight of the other constituents of the acid in a grain of the salt amounts to 0.309 gr. When

one grain of the salt was heated to redness with peroxide of copper, I obtained volumes of carbonic acid gas and of azotic gas which bore to each other nearly the proportion of $2\frac{1}{2}$ to 1. From this I was disposed to conclude that the carbon and azote in the ferrochyzic acid bore to each other the same ratio as in hydrocyanic acid; namely, two atoms of carbon to one atom of azote; but in a paper which Mr. Porrett inserted in the *Annals of Philosophy* for Sept. 1818, he suggested the probability that the atoms of carbon and of azote in ferrochyzic acid bore to each other the ratio of 4 to 1; and he stated an experiment which corroborated that notion. This naturally led me to reconsider my experiments, and to endeavour to detect the cause of the deficiency of carbonic acid in all my trials. I repeated my experiments with an improved apparatus, but still found the proportions of carbonic acid gas and azote to each other nearly as at first.

After the publication of Mr. Porrett's new paper on Ferrochyzic Acid in the *Annals of Philosophy* for October, 1819, in which he gives experiments, from which he deduces that the volume of carbonic acid gas evolved is four times that of the azotic gas; but admits the quantity of iron to be no greater than I had found, I repeated the experiments again with all the requisite care; but my results were still two volumes of carbonic acid for one volume of azote very nearly; nor was I able to obtain so great a proportion of gas from a given weight of ferrochyzate of potash as Mr. Porrett got. I have been unable, therefore, to verify Mr. Porrett's conclusions, and consequently must still adhere to those which I gave in my first paper. On coming to London, Dr. Prout informed me that he had made many experiments on the analysis of the ferrochyzates, and had always obtained with his new apparatus two volumes of carbonic acid gas for one volume of azote. I repeated my experiments with his apparatus, which is susceptible of greater precision than my own. The result was as follows: five grains of crystallized ferrochyzate of potash gave out eight cubic inches of gas, when heated with peroxide of copper. Of these, 4.6 cubic inches are absorbed by potash, and consequently are carbonic acid gas; while 3.4 cubic inches remain unabsorbed, and consequently are azotic gas. Now if we take into our estimate the carbonic acid that must remain united to the potash of the salt, we shall find that the quantities of gas evolved were as follows:

Carbonic acid gas	6.8 cubic inches or 2 volumes
Azotic gas.	3.4 cubic inches or 1 volume

It seems decided from these experiments that the carbon and azote exist in ferrochyzic acid exactly in the same proportions as in cyanogen. As to the hydrogen and the iron, I have not made new experiments to determine their proportion.

I am still at as great a loss as ever to form an adequate idea

of the composition of this acid. The weight of an integrant particle of it seems to be 6.25 or 6.75. I begin to suspect that oxygen enters into its composition. Further experiments are still wanting to throw light on this obscure point.

I considered these preliminary observations as requisite to enable the reader fully to understand the phenomena which take place when ferrochyzate of iron is exposed to a red heat. To these phenomena I now proceed.

Ferrochyzate of iron is a tasteless powder, of a very deep and beautiful blue colour. It is partially decomposed by nitric and sulphuric acid, and probably likewise by muriatic acid; but I did not succeed in obtaining a complete decomposition by digestion in these acids, though continued for several days, and assisted by a moderate heat; but all the alkalies, the alkaline earths, and some of the metallic oxides, decompose it with great facility, depriving it of its ferrochyzic acid, and of course destroying the colour. Nothing remains behind, if the experiment be properly conducted but peroxide of iron.

When heated to the temperature of 400°, it catches fire, and burns for sometime without flame, giving out very concentrated fumes of ammonia. Twenty grains of ferrochyzate of iron, when thus treated, were reduced to 10.9 grs. This residuum effervesced strongly when put into muriatic acid, and dissolved, leaving 1.5 gr. of undecomposed ferrochyzate of iron; consequently 18.5 grs. of the salt were decomposed by the combustion. Now the constituents of these 18.5 grs. were as follows:

Ferrochyzic acid	9.435 grs.
Peroxide of iron	6.993
Water.	2.072
	18.500

The quantity of iron in 9.435 grs. of ferrochyzic acid amounts to 2.446 grs. which are equivalent to 3.494 grs. of peroxide. From this it is obvious that part of the iron was volatilized; for the whole iron in 18.5 grs. of ferrochyzate of iron (supposing it in the state of peroxide) would amount to 10.487 grs.; whereas the whole residue amounted only to 9.4 grs.

I conceive that during the combustion a portion of the carbon is converted into carbonic acid, which combines with the iron of the ferrochyzic acid, changed at the same time into protoxide. This accounts for the effervescence which takes place when the residue is thrown into muriatic acid. The evolution of the ammonia is not easily explicable, unless we suppose that the water of the salt undergoes decomposition at the same time with the ferrochyzic acid. It will be recollected that the pigment contains one atom of ferrochyzic acid for every atom of water present.

Such are the phenomena which take place when the ferrochy-

azate of potash is heated in the open air. Let us now see what happens when the pigment is heated in vessels excluding the action of the external air. The experiments which I am going to relate were frequently repeated; but I conceive it to be sufficient to give a single example, as the results in all the experiments were entirely similar. Into a copper tube, about 14 inches in length, and about 0.35 of an inch in internal diameter, close at one end, and having a tube partly of brass and partly of glass, fitted to the open end by grinding, I put 50 grs. of ferrochazate of potash, constituted, as stated in a preceding part of this paper. The pigment was secured in its place by filling the empty part of the tube with asbestos. The internal capacity of the whole apparatus, when empty, was 2.18 cubic inches; but the glass tube which constituted the greatest part of that capacity (being blown out towards the middle into a hollow ball) was partly filled with dry muriate of lime; so that the whole common air which existed in the apparatus did not exceed one cubic inch. The copper tube was surrounded with burning charcoal, which was urged by a pair of bellows so as to bring the pigment as speedily as possible to a strong red heat; and in this state it was kept for two hours.

After the conclusion of the process, I allowed the tube to get quite cold. I then shook out the whole residual matter from the 50 grs. of pigment. It still acted feebly as a pyrophorus, catching fire of its own accord upon coming in contact with the air, and giving out the smell of ammonia; so that even a red heat of two hours' continuance did not decompose the pigment completely. It has been long known, I presume, that when ferrochazate of iron is exposed to a red heat in a close vessel, and then exposed to the air, it burns strongly as a pyrophorus. I am disposed to ascribe this property to a small residue of potash which the pigment always retains, and which by the reaction of the other constituents, is converted into potassium. The pyrophorous quality, however, is considerably impaired when the red heat is kept up for two hours.

The weight of this residue after the spontaneous combustion was over amounted to 24 grs. When dissolved in muriatic acid, this residue did not leave any prussian blue, but only a small quantity of black matter, which possessed the usual characters of animal charcoal.

Now 50 grs. of ferrochazate of iron contain (as will be seen above) 18.9 grs. of peroxide of iron; and the iron in the ferrochazic acid present amounted to 6.61 grs. We see from this that part of the iron must have been driven off by the heat; for $18.9 + 6.61 = 25.51$ grs. which exceeds the whole weight of the residue by a grain and a half. I am disposed from this to suspect that part of the ferrochazic acid is driven off without undergoing decomposition.

The muriate of lime increased in weight during the continu-

ance of the experiment 14.3 grs. Now the whole water which the 50 grs. of the pigment contained amounted only to 5.6 grs.; therefore, 8.7 grs. of this increase must have been matter formed during the process. If we suppose the whole to be water, the oxygen of it must have been derived from the peroxide of iron which constituted the base of the salt. This peroxide amounted to 18.9 grs.; the oxygen of which amounts to three-tenths of the whole, or 5.67 grs. 5.67 grs. of oxygen, in order to be converted into water, must unite with a quantity of hydrogen amounting to one-eighth of the weight of the oxygen, or 0.708 gr. It is obvious from this that the whole of the water which could have been formed during the process from the oxygen present in the salt could only amount to 6.378 grs. Hence I conceive myself entitled to conclude that at least 2.322 grs. of the increase of weight which the muriate of lime experiences is occasioned by something different from water. What this something is we easily discover by performing the process without putting any muriate of lime into the tubes. The liquid which comes over is strongly impregnated with hydrocyanate of ammonia. How much of this salt is actually formed, I had not the means of ascertaining; but it must have considerably exceeded 2.322 grs.; for the iron in the residue was still in the state of oxide. Indeed from the action of muriatic acid upon the residue, it was obvious that it still contained peroxide of iron.

The tubes and the glass jars standing over mercury in which the gaseous products were received were coated with transparent colourless crystals, which had the smell of hydrocyanic acid, and which dissolved readily in water. When sulphuric acid was dropped upon them, an effervescence took place, and a strong odour of hydrocyanic acid was exhaled. When heated with soda, they gave out a strong smell of ammonia. Hence I consider them to have been crystals of hydrocyanate of ammonia.

The gas extricated amounted, under the mean pressure and temperature, to 46.5 cubic inches. It had the smell of hydrocyanic acid. When water was let up into it, the volume was reduced to 26 cubic inches. I consider the gas absorbed by the water, or at least the greatest part of it, to have been hydrocyanic acid vapour; at least the water which had absorbed it possessed all the characters of Scheele's prussic acid; which I have no doubt is merely an aqueous solution of the hydrocyanic acid of Gay-Lussac.

The 26 cubic inches of residual gas were destitute of taste and smell. When mixed with oxygen gas, or when brought in contact with a lighted candle, this gas detonates, or burns with a yellow-coloured flame. During the combustion, carbonic acid gas and water are formed. The specific gravity of this gas I found to be 0.8691, that of common air being reckoned 1.

To determine the constituents of this inflammable gas, I subjected it to a number of experiments, by burning it in a Volta's

eudiometer, with determinate quantities of oxygen gas. The following are the results which I obtained :

One hundred volumes of the inflammable gas required for complete combustion 42.1 volumes of oxygen gas. After the combustion, the bulk was diminished by 34.2 volumes, and the residue consisted of a mixture of 25.8 volumes of carbonic acid gas and of 82.2 volumes of azotic gas, which must have escaped the action of the combustion. We must, therefore, consider the gas to have consisted of

Azotic gas ..	82 volumes.	
Carbon	26 volumes.	{
Hydrogen ..	32 volumes.	{
		(From the 16 volumes of oxygen consumed above what entered into the carbonic acid.)

140

So that the constituents of it, if they had been merely mixed together without any chemical union, would have occupied 140 volumes instead of 100 volumes.

I am disposed to consider this gas as a mere mixture of 82 volumes of azotic gas with 18 volumes of a gas composed of

26 volumes carbon
32 volumes hydrogen

condensed into the third part of their volume. The specific gravity of such a gas would be 0.6743 ; and the specific gravity of a mixture of 18 volumes of such a gas and 82 volumes of azotic gas would be 0.9196. This rather exceeds the specific gravity given above. But as I made only a single experiment, I might not improbably have committed an error of six per cent. which constitutes the difference between the two numbers.

If any confidence can be put in the accuracy of these experiments, this inflammable gas constitutes a new species of carburetted hydrogen, differing in its composition from all those previously known. It seems to be a compound of 13 atoms carbon and 16 atoms hydrogen, or more probably of 3 atoms carbon and 4 atoms hydrogen condensed into one-third of their volume. It is intermediate between the common species of carburetted hydrogen gas and olefiant gas, and might also be considered as a mixture of equal volumes of these two gases ; for olefiant gas is a compound of two atoms hydrogen and two atoms carbon ; while carburetted hydrogen is a compound of one atom carbon and two atoms hydrogen. We have, therefore,

	Carbon	Hydrogen.
1 volume olefiant gas composed of	2 atoms	+ 2 atoms
1 volume carburetted hydrogen ..	1	+ 2
Constituting our inflammable gas.	3	+ 4

The specific gravity of olefiant gas	0·9708
carburetted hydrogen ..	0·555
	2)1·5258

Mean specific gravity = 0·7629

But it will be seen above that the specific gravity of our gas is only 0·6743, which is considerably less than the mean specific gravity of such a mixture. We must, therefore, consider the inflammable gas evolved when ferrochyzate of iron is exposed to a red heat as a peculiar compound, and not a mixture of carburetted hydrogen and olefiant gases. We may consider it as a compound of equal volumes of these two gases increased in volume about a ninth part.

Thus we have discovered a new gaseous combination of carbon and hydrogen; so that the known gaseous combinations of these two substances amount to three. I have little doubt that they will be found hereafter to unite in a still greater number of proportions. Indeed we require a considerable number of additional gases to be able to account for the great variations in the specific gravity and other properties of the combustible gases from coal and other similar bodies.

Thus it appears that when ferrochyzate of iron is kept at a red heat while air has no access to it, a decomposition takes place, and there are formed

Water,	Supercarburetted hydrogen,*
Hydrocyanic acid,	Azote.
Ammonia,	

The data which I have stated do not enable us to determine the respective quantities of each of these substances; consequently we cannot determine the nature of the decompositions which take place. Indeed I have no doubt that a portion of ferrochyzic acid, or at least of the iron which exists in that acid, was mixed with the other constituents, though, from the nature of the processes, I had not the means of detecting it. Indeed in one process in which all the liquid products made their way into the jars standing over mercury, traces of prussian blue were visible in the liquid, and the gaseous products, as might have been expected, were reduced in quantity.

The copious evolution of ammonia, when prussian blue is set on fire, constitutes one of the most curious and instructive phenomena. We see the great tendency, which azote and hydrogen have to unite in the proportions constituting ammonia. I shall not prosecute this subject further at present; but as the curious phenomena which appear during the decomposition of the ferrochyzates are by no means exhausted, I may, perhaps, resume the subject hereafter.

* By this provisional name I mean to distinguish the new inflammable gas extricated from prussian blue by heat.

ARTICLE X.

*Observations on the Means of removing Accumulations which impede Navigation.** By Joseph M'Sweeny, M.D.

THE utility of clearing the beds of rivers from accumulations of sand or gravel needs no comment: any thing which shall tend to diminish labour and expense in the removal of them will I hope be deemed worthy of consideration. The expense of clearing rivers by dredging machines impelled by steam engines must be very great, while the employment of men is objectionable on account of the work being both expensive and laborious. Here in Cork, below St. Patrick's bridge, men have been recently engaged in throwing gravel with shovels into a large boat resting on the accumulation when the tide was out. The labour of throwing any thing from the bed of the river over the elevated sides of a large boat may be easily conceived. The trouble of clearing the boat afterwards of the stuff raised must also be taken into account. I apprehend that the following expedient will save a great deal of labour: A large receiver open at one end is to be laid on the bed of the river, exactly similar to the contrivance used in salt works for weighing salt, and for pouring it off when weighed. It should, in like manner, be provided with chains uniting at a ring that it might be evenly suspended from any fixed point.

When the tide is out, this receiver can be easily filled with gravel, and when the tide returns sufficient to allow a broad flat-bottomed boat provided with a hook in the centre of the bottom to float over it, the ring of the receiver is to be put on the hook. The person who does this is now to get into the boat, and wait the further rise of the tide. As the tide rises, the buoyancy of the boat will raise a weight of gravel that would in ordinary circumstances sink it, provided what contains the weight be evenly suspended from the hook.

The boat is now to be propelled out of the channel of the river to any place most convenient for depositing the gravel here by pulling a rope attached to the closed end of the receiver so as to raise it, the persons in the boat can cause the gravel to slide down and escape at the open end.†

In this manner the surface of ground subject to be overflowed by the tide may be raised, and stone and other materials for embankments can be deposited in places where it is intended to erect them to keep off the water. In the formation of a break-water great trouble is experienced in raising out of the hold of a

* Read at the Cork Philosophical and Literary Society, on May 3, 1820.

† When fine sand, or any thing liable to be washed off, is to be conveyed, the receiver at this end may be closed up with a board moving on hinges and united by lynch pins. These pins should move so freely that they could easily be pulled up by ropes to allow the board to fall down that the sand might escape.

vessel large masses of stone to sink them. By adopting the plan suggested in this paper that inconvenience is obviated. In laying the foundation of a pier, immense blocks of stone attached by chains to the bottoms of large boats may be brought during high water, and may be deposited accurately in the place intended by the subsidence of the tide.

JOSEPH M'SWEENEY.

ARTICLE XI.

Proceedings of Philosophical Societies.

LINNEAN SOCIETY.

June 6.—A continuation of Dr. Horsfield's paper, on the Birds of Java, was read.

June 30.—A paper, by Mr. Brown, on a new Genus of Plants called *Rafflesia*, was read.

The plant from which this genus is established was discovered in Sumatra in 1818, by the late Dr. Joseph Arnold, who accompanied Sir Stamford Raffles in his first journey into the interior of the island.

This plant has lately excited the curiosity of botanists, from the extraordinary dimensions of its flower, which is equally remarkable in structure.

The flower springs directly from a horizontal root. The bud is covered with many round imbricate dark-brown floral leaves, or bracteæ, and has very much the appearance of a cabbage, which it also nearly equals in size.

The expanded flower was ascertained by measurement to be full three feet in diameter; the tube it was supposed would hold 12 pints, and the weight of the flower was reckoned to be about 15 lbs.

As the proper floral envelope is simple, it is, though coloured, regarded rather as calyx than corolla. Its substance is fleshy, and of a thickness proportioned to its size; the tube is short, the faux is produced into an entire annular corona, and the limb is deeply divided into five equal rounded spreading segments.

The few flowers yet examined proved to be male.

The antheræ are numerous, sessile, nearly spherical, cellular, and bursting by a single pore at top. They are disposed in a simple series under the projecting margin of the apex of a fleshy column which occupies the centre of the flower, is included in the tube, and whose disc is furnished with numerous slightly curved hornlike processes.

The author of the paper, in treating of the affinities of this singular plant, compares it chiefly with *Asaræa* (or *Aristolochæa*),

and *Passiflorea*; but does not pretend to determine absolutely to which of these two families it is most nearly related.

He is inclined to consider it as being parasitical on the root to which it is attached; he does not, however, speak with confidence even on this point, for the satisfactory determination of which the examination of additional specimens in various stages appears to be necessary.

ARTICLE XII.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS
CONNECTED WITH SCIENCE.

I. *Singular Rain.*

ON Nov. 2, 1819, about half-past three in the afternoon, there fell a pretty heavy shower of rain at Blankenberge, in the Netherlands, which lasted about a quarter of an hour. The wind was westerly. This rain had a dark-red colour. The rain indeed lasted the greatest part of the day, but the red colour disappeared after the interval above-mentioned, and the rain assumed the ordinary appearance. Some of this extraordinary rain was brought to Bruges, and examined by MM. Meyer and Von Stoop, who concluded from their experiments that it owed its red colour to a solution of *muriate of cobalt*.

They concentrated 144 ounces to 4 ounces. No precipitate fell, and no crystals made their appearance in it. When sulphuric acid was poured into this concentrated liquid, muriatic acid was driven off in a very perceptible manner. When nitrate of silver was let fall into it, a precipitate of *hornsilver* immediately fell. Hydrosulphuret of potash threw down a black precipitate, which, when heated to redness, assumed the metallic appearance. The liquid, after being precipitated by nitrate of silver, was treated with an alkaline hydrate; a purple-coloured precipitate fell, which, being reduced in the usual way, gave three grains of a brittle, greyish-white metal, which was attracted by the magnet, and which communicated a fine blue colour to borax, when treated with it before the blow-pipe. Such were the evidences upon which these chemists concluded that the rain in question contained *muriate of cobalt*.—(Gilbert's *Annalen*, lxiv. 335.)

II. *Existence of Muriate of Potash in Rock Salt.*

M. Vogel was induced by Dr. Wollaston's discovery of the existence of *muriate of potash* in sea water to examine whether the same salt might not be detected in common salt, whether obtained from mineral springs, or dug out of the earth in the state of a solid water. He examined salt from Hallein and from

Berchtesgaden. The simple solutions of these bodies in water were not precipitated by nitromuriate of platinum; but when these solutions were concentrated till a considerable proportion of the common salt was extracted, they then yielded a yellow precipitate, when treated with this nitromuriate, indicating the existence of potash. The same experiments succeeded with the salt brine from Rosenheim, in the upper part of Bavaria. He examined likewise the mother liquor, which drops from salt extracted from salt springs, and obtained likewise a precipitate with nitromuriate of potash. These experiments leave no doubt of the existence of potash in common salt extracted from brine springs, and show a striking analogy between these springs and sea water.—(Gilbert's Annalen, lxiv. 157.)

III. Benzoic Acid.

Hitherto benzoic acid has been found only in benzoin, storax, balsam of Peru and Tolu, vanilla, cinnamon, and the urine of several graminiverous animals, as cows, horses, camels, rhinoceros. Vogel has lately made a curious addition to the number of plants which furnish it. He found it crystallized in the *Tonquin bean*, between the skin and the kernel. It is well known that this bean, as it is called, is the produce of a tree a native of Guiana, which has received various names from botanists. By Aublet, it is denominated *Coumarouna odora*; by Gärtner, *Baryosma Tongo*; and by Wildenow, *Dipteryx odorata*. The only useful purpose, as far as I know, to which this bean is put, is to give an agreeable odour to snuff.

The crystals which Vogel found in these beans melt at a moderate heat into a transparent liquid, which, on cooling, suddenly shoots out into stars, and then assumes the form of a crystallized mass. If the temperature be increased, it sublimes, and is deposited under the form of fine brilliant needles. The smell of these needles is quite similar to that of the Tonka bean, the concentrated solution of these needles in alcohol reddens litmus paper, and becomes milky when mixed with water. When saturated with ammonia, these needles form a salt which throws down iron with a brown colour. In short, they possess all the characters of crystallized benzoic acid.

M. Vogel has likewise detected benzoic acid in the flowers of the *trifolium melilotus officinalis*. His method was to digest the flowers in absolute alcohol raised to the boiling temperature. On cooling, it let fall a fatty substance, and in two or three days crystals of benzoic acid made their appearance in the liquid in the state of long crystals. To separate these crystals from the fatty matter, the whole was digested in boiling water, and the liquid passed through the filter. The liquid containing the benzoic acid passed through the filter, and when slightly evaporated yielded the benzoic acid in crystals. M. Vogel thinks that the quantity of benzoic acid in the blossoms of this plant is so

great that it may be extracted from them economically for the purposes of sale.—(Gilbert's *Annalen*, lxiv. 161.)

IV. *Self-registering Rain-Guage.*

Mr. Donovan, Professor of Chemistry, *Materia Medica*, and Pharmacy, to the Apothecaries' Hall of Dublin, with whose merits as a chemist and philosopher my readers are well acquainted in consequence of his very valuable work on galvanism, and his important chemical papers, of which due notice has been given in the *Annals of Philosophy*, has just completed a very curious and ingenious rain-guage possessed of the following properties :

“ This machine is calculated to keep an exact register of the quantity of rain that falls during any period of time in the absence of the observer. Nothing more is required than to put a card into the machine and wind it up. At the end of every week, the card is to be removed, and a new one put in : on it will be found registered the following particulars :

“ It will show at what precise hour and minute of the day, and upon what day, the first cubic inch of water fell during any series of weather.

“ It will show how many cubic inches of rain fell during the whole week, and the precise hours at which each fell, what day of the month, and whether it was night or day. Hence we have also an exact measure of the heaviness of the showers.

“ It will show when the rain commenced (at least so as to be worth noting), how long it continued ; what intervals there were of tolerably dry weather ; and when the rain ceased.

“ The descent of each inch of rain will also be announced by a bell, which will act as a notice at night when access cannot be had to the detailed account on the register. The examination of the register card need not be weekly ; it may be consulted at any time.

“ The machine will tell not only the individual cubic inches of rain with their times, but it will show the total quantity at one view.

“ At the end of the week, the card is to be put on a file, and thus, without any trouble of writing or watching, may be kept a precise register, that can at any time be consulted with ease, of the weather with regard to rain.

“ The machine can be adjusted in such a manner that instead of cubic inches of rain it will register its results in ounce measures, which, being the sixteenth of a pint, would be more generally appreciated.

“ It requires winding once a week, and a new card ; this is all the trouble it gives ; but it may be constructed very easily so as to require these operations but once a month. It is not liable to go out of order ; for however complicated its functions may appear, its individual parts are simple.”

V. *Completion of the great Metallic Vase at Mr. Thomason's Manufactory, Birmingham.*

The public are indebted to the late Sir William Hamilton for the beautiful collection of antique vases which enrich the mansions of the nobility and wealthy; for having less pleasure in the possession of these treasures than in gratifying the good taste of his countrymen in making them public, he distributed them with a most liberal hand to those who felt their beauty and appreciated their importance; hence he presented to the late Earl of Warwick the chef-d'œuvre of Grecian sculpture by the chisel of Lysippus.

Our ingenious countryman, Mr. Thomason, of Birmingham, conceived the idea of making a fac simile of this great vase entirely of metal. He accordingly commenced this great undertaking in the 54th year of the reign of King George III. and has lately completed it. Two hundred and eleven medals of different subjects, including one of King George IV. all made at the manufactory, were sealed up in an antique urn, and deposited in the centre of the pedestal upon which the vase was raised by the efforts of about 50 of the workmen in celebration of his present Majesty's accession to the throne.

The character and history of the Warwick vase are so generally known that we shall confine ourselves to the description of the metallic one.

In 1814 the late Earl of Warwick, who liberally patronized the fine arts, permitted Mr. Thomason and his artists to have free access to the original vase to model it in wax, which occupied several months. From these models casts were made in lead to serve as *patterns* to form the whole, which whole is made in two distinct metals, the field being of one metal, and the handles, vines, masks, panther skins, and leaves, composed of another; this original thought gave Mr. Thomason the opportunity of adopting two novel modes of oxidation, thereby producing the most beautiful effect of light and shade, the oxidating of the field being accomplished by a combination of the sulphates and nitrates urged on by powerful heat, which has produced the desired appearance of the rouge antique marble. The masks, handles, and parts, in relief, are oxidated by the acetates, and resemble the verd antique bronze. The harmony of these two colours is at once grand and imposing.

This vase, which is 21 feet in diameter, and weighs several tons, from its being made of imperishable materials, will not only record and perpetuate the fame of our country, but hand down the name of Mr. Thomason to remote posterity. It affords also a true pledge that a rapid improvement of taste has taken root in the great manufacturing town of Birmingham, and that we need not apprehend being surpassed in fine and classical workmanship by our competitors abroad.

VI. *On the Naturalization of tender Exotics.* By Mr. Murray.

Dr. Macculloch has given to the Caledonian Horticultural Society a paper on the Naturalization of tender Exotics, which has been published in their "Memoirs," and crowned with their gold medal.

I can discover nothing in this paper which had not been before treated of by Baron de Humboldt, and is to be found in his "Personal Narrative." It proceeds upon the assumption that plants may, by a gradual transition, be made to endure finally a more rigorous clime, and that tropical vegetation may ultimately luxuriate and be naturalized in climates the most remote from the confines of the torrid zone.

Guernsey is singled out as an appropriate nursery for this intermediate transplantation, and it is suggested that the seeds of such tropical plants as have been transposed to this island will give birth to a race more hardy than their primogenitors. But the question will always return, what is there peculiar in the atmosphere of Guernsey, or other insular tracts of country, which there imparts to the exotics of a torrid clime all the "pomp and circumstance" of tropical vegetation? Dr. Macculloch adverts to the *uniformity* of the climate, and this seems to me to be the grand cause why we witness in Guernsey the luxuriance of exotics, which we in vain attempt to recognize in lower latitudes. Of the cause of this uniformity we are not informed, and it may, perhaps, not be deemed irrelevant if I attempt to account for it. (See the next notice.)

In the Memoirs of the Caledonian Horticultural Society, a facetious writer proposes to make the *cloudberry* leave its alpine seat, and hop, step by step, into the plain below. In the former case it is more than probable that the transition from a uniform to a very variable clime would operate as fatally as that from high increments of temperature to frigid media; so in the latter instance there is more to be taken into the account than a mere transit from an alpine elevation to the level of the sea in reference to temperature. It may not only be more uniform, but there is less density, and in point of hydrometric relations a greater siccidity: while the electrical state of the medium is very different.

That a gradual and well managed introduction will accomplish much, striking analogies would lead us to conclude, but it is evident a great deal is to be taken into calculation, and that we are not to feel disappointed if our endeavours are sometimes baffled.

At p. 77 of the Edinburgh Philosophical Journal, No. V. (for July) we have a remarkable evidence of what may be done in this way by the detail which Mr. Macnab there gives us of a suspended plant of *Ficus Australis*, which had grown *without earth* for eight months. Mr. Macnab also pointed out to me in the same stove a species of *Solandria* which bids fair to yield a

similar result. I have myself had *two leaves* of the "cotyledon calycina" in my *pocket book* for *three months*. Young *perfect plants* supplied with roots have sprung from some of the indentations of the leaf, and *continue to grow*.

VII. *On the Uniformity of Insular Climate.* By Mr. Murray.

The temperature of springs has been pronounced an index of the mean temperature of a country. The sea preserves a uniform temperature, and is not subject to variable heat or cold: the consequence of all this will be, that the air incumbent on the bosom of the ocean will participate of this mean temperature, and by mingling with the atmosphere pendant over small islands by a tendency to an equilibrium impart to it a uniformity not enjoyed by inland tracts of country. This is in some measure participated in by such soils as border upon the sea coast; an earlier vegetation and maturity always characterize them. They are not checked by those sudden transitions which mark other situations. An island in the ocean may be compared to a garden-pot plunged into a bed of equal temperature. In inland countries vegetation must be more affected by the rapid variations which result from radiation and other causes, and which have no compensating curb. Numerous are the facts which I have collected, and which go to establish this opinion. It would be unnecessary to adduce them here. In the "Isola Bella," and "Isola madre," are plants exposed, and always unprotected, which even in southern Italy would be sought for in vain under such circumstances, as the *arundo saccharifera*, *coffea arabica*, &c.

VIII. *On the Vegetation of Bulbous Roots in Water.* By Mr. Murray.

I made last winter some experiments on bulbs growing in water chiefly with reference to a transition into varied media, and a short detail of some of them here, while they afford information with regard to the functions of the roots, may not be deemed uninteresting. The bulbs were those of the hyacinth.

The Dutch keep the hyacinth when newly placed on the bulb glass for some time in the dark to induce good roots, and for this purpose by way of comparison with other bulb glasses I used coloured ones. The fibres of the root seem generally to attain a greater longitude in coloured glasses, in the green especially. In the blue, the fibres are marked by the singularly strong ones which subsequently surround the margin.

The very strong swollen fibres which characterize the latter stage of the plant is, perhaps, connected with the evolution of the flower at that period when it should declare itself. At any rate there are two distinct series of roots, and the primitive, more minute fibres shrink when the others emerge.

A bulb fed with a solution of carbonate of ammonia continued healthy, and flowered well; but the roots did not greatly elongate.

One hyacinth bulb was injured to repeated additions of salt water till sea water was finally used: it continued to grow, but was not so luxuriant as the others.

A bulb fed with diluted pyrolignous acid did not seem materially affected by the new medium, and the fibres seemed to have decomposed the acid matter in contact.

Repeated renewals of the water seem to ensure health and luxuriance, and more rapid growth. The probability is, that there is an appropriation of the air contained diffused through the water, which is in time exhausted, and, according to M. Gay-Lussac, the air contained in water contains excess of oxygen.

IX. *Oxidation by Solar Light.* By Mr. Murray.

When light is decomposed, the chemical rays which may be thus insulated are twofold; namely, *oxidating* and *hydrogenating*. I was much struck with the effects induced by the solar rays on the *iron railing* which is on the summit of the *leaning tower of Pisa*, in Tuscany. Where exposed to the action of the solar influence, it is *highly oxidated*, while such parts as are from the inclination of the tower, &c. not so exposed, have undergone little alteration.

X. *Lute.* By Mr. Murray.

In the vicinity of Naples I found that the Neapolitans use a cement for earthenware and other pipes composed of *powdered pumice* and *quicklime*, and was assured it formed a most excellent lute.

I examined with some care the magnificent ruins of ancient Rome. The mortar seems in almost all cases composed of quicklime and *pozzolana*, and this last is a congeries of volcanic scoræ, the chief part of which is pumice stone. Vitruvius not only adverts to *pozzolana* as having been employed in the mortar of ancient edifices, but points out its property of hardening under water. He even names its locality. It was obtained near the "Bagno di Nerone," in the Bay of Baia. To this ingredient most probably are to be attributed the hardness and durability of the mortar of the ancients.

XI. *Solubility of Phosphorus in Water.* By Mr. Murray.

I believe it has not been considered that phosphorus is soluble in water, and yet we well know that it communicates to that fluid its peculiar odour, and renders it poisonous to such animals as drink it, especially when the phosphorus has remained in it for some time.

If phosphorus, however, be allowed to rest for some days in

distilled water, and the fluid then passed through filtering paper, it will exhibit *minute luminous jets* if poured on hot water in the dark.

XII. *Agave Americanum*. By Mr. Murray.

The *Agave Americanum* is said to flower only once in a century. This much we know at any rate, its flower is a rare phenomenon in the British isles. Not a few, however, may be seen in flower on the sides of the high way from *Terracina* to *Capua*; on the road to Naples. I think it obvious, from an attentive examination of many of these plants, that this *Agave* flowers only *once*, and that having provided for its perpetuity by an offset, the whole energies of the plant are expended in the flower, which being matured it perishes. The most magnificent flower of this species I ever saw was growing on an exposed rock in one of the Borromean islands, called *Isola madre*, in the "Lago-maggiore," north of Italy. I measured the flower stem, or *pedunculus*, and found it *twenty-eight and half feet in altitude*, and its *circumference*, where it emerged from the leaves, *two feet ten inches*. It was a superb spectacle.

I was inclined to consider this plant as *indigenous* to Italy, but Sir James E. Smith tells me he is not of this opinion. It is, however, common in that country. It crowns the *walls* of the city of Genoa where it even flowers, and I have seen whole fields near *Pontercule* fenced in entirely with the *Agave Americanum*.

The plant sketched in *fresco* on one of the walls of Pompeii, supposed to be an *Agave*, and which would assign a distant date to its introduction into Italy, does not appear to me (and Dr. J. F. Schouw joins in the opinion) ever to have been intended for the plant in question.

XIII. *Crystallization of Platina*. By G. B. Sowerby, Esq.

I am not aware that any thing decided is known respecting the crystallization of this substance. Bournon, it is true, has mentioned some grains of platina which have a distinct form, but these appear to have been a deposition of platina upon some other substance which has afterwards been decomposed and gone, for they are hollow and mammillated upon their outer surfaces. I have some of these, and I have reason to think that they have been formed over palladium, for some externally mammillated portions of platina which accompany them are still adherent to native palladium. But in looking over a small parcel of platina very lately, I discovered several pieces which had a perfectly lamellar structure, and a distinct cleavage; and one of them which showed the four faces forming the solid angle of an octahedron. I have preserved this little fragment, as being perfectly demonstrative for the inspection of any who may think the subject sufficiently interesting.

Should you think this notice worthy of insertion in your journal, it is much at your service.

XIV. *Meteorite.* By G. B. Sowerby, Esq.

In your last number (for August) you mention a meteorite which fell at Jonsac, or Tonsac, on June 13, 1819. From an examination of a small portion of this stone now in my possession, for which I am indebted to the liberality of M. Brochant de Villiers, I am enabled to inform you that it differs much in outward appearance from all the meteorites I have ever observed, except those which fell at Stannern, in Moravia, a few years ago. Like them its internal parts are much lighter in colour, and its crust is perfectly vitrified and shining.

XV. *Temperature of Taroslavl, in Russia.*

The last winter was remarkable for its severity throughout the north of Europe. An English gentleman who wintered in the above town states, that Reaumur's thermometer generally stood at from 12° to 20° below the freezing point (equal from 27° to 45° of Fahrenheit), that it often sunk even much lower, and once reached the extraordinary point of 32° , equal to 72° Fahrenheit, below the freezing point.

Taroslavl, or *Yaroslavl*, as it is called in the maps, is one of the largest towns in Russia. It is situated on the Volga, about 130 miles N.N.E. from Moscow, in latitude about $57\frac{1}{2}^{\circ}$ North, or nearly on the same parallel as Inverness.

ARTICLE XIII.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

Dr. Renwick is about to publish the Continuation of the Narrative of Miss Margaret M'Avoy's Case, with general Observations upon the Case itself, &c. and with additional Proofs of her Blindness, &c.

Shortly will be published, lithographed in 4to. a Series of Designs for private Dwellings, comprising perspective Elevation, adapted to geometrical Measurement, and Plans of the several Stories, with explanatory References. By J. Hedgeland.

Mr. Accum has in the Press, a Treatise on Domestic Chemistry, containing Instructions for making good and wholesome Bread, Beer, Wine, Vinegar, Pickles, &c.

Prof. Leslie has in the Press, Geometrical Analysis, and the Geometry of Curve Lines: also a Treatise on Heat, theoretical and practical.

Travels in England, Wales, and Scotland, in 1816, by Dr. Spiker, Librarian to the King of Prussia, are translating for the Press.

Lectures on the Philosophy of the Human Mind, by the late Dr. Thomas Brown, are printing in three octavo Volumes.

JUST PUBLISHED.

A Compendium of the Ornithology of Great Britain, with a Reference to the Anatomy and Physiology of Birds. By John Atkinson, F.L.S. &c. 8vo. 8s.

A new Method of solving Equations with Ease and Expedition, by which the true Value of the unknown is found without previous Reduction. By T. Holdred. 4to. 7s.

An Essay on Involution and Evolution. By Peter Nicholson, 8vo. 6s.

Pomarium Britannicum, an Historical and Botanical Account of Fruits known in Great Britain. By Henry Phillips, royal 8vo. 1l. 1s.

Observations on the Nature and Extent of the Cod Fishery carried on off the Zetland and Orkney Islands. By A. Edmonston, M.D. 8vo. 2s.

Observations on a general Iron Rail-Way, showing its great Superiority over all the present Methods of Conveyance. 8vo. 1s. 6d.

Observations on Variolous Inoculation and Vaccination. By J. F. Hulbert. 12mo.

Popular Observations on Regimen and Diet. By J. Tweed. 12mo. 5s.

Lectures on the Philosophy of History, with Notes and illustrative Engravings. By the late Rev. Ezekiel Blomfield. 4to. 1l.

The Heraldic Origin of Gothic Architecture in Answer to all foregoing Systems. By Rowley Lascelles, Esq. Royal 8vo. 7s.

An Account of the Improvements on the Estates of the Marquis of Stafford, in the Counties of Stafford and Salop, and on the Estate of Sutherland, with Remarks. By James Loch, Esq. 8vo. 12s.

ARTICLE XIV.

NEW PATENTS.

John Read, of Horsmanden, Kent, for an improvement on syringes. July 11, 1820.

Samuel Fletcher, of Walsall, Staffordshire, sadlers' ironmonger, for an improvement on, or additions to, saddles, saddle-straps, saddle-girths, and saddle-cloths, by the application of certain known materials, hitherto unused for that purpose. July 11, 1820.

James White, of Manchester, for certain new machinery, adapted to preparing and spinning wool, cotton, and other fibrous substances, and uniting several threads into one; and also certain combinations of the said machinery with other machines, or with various parts only of other machines already known and in use. July 11, 1820.

John Grafton, of Edinburgh, for a new and improved method of distilling the products of coal, and carbonising coal, in the process of making gas used for the purpose of illumination. July 11, 1820.

William Davis, of Bourne, near Minchin Hampton, Gloucestershire, for improvements in machinery for shearing or cropping woollen and other cloths requiring such process. July 11, 1820.

Matthew Bush, of Battersea-fields, for an improvement on a machine now in use for printing silks, linens, calicos, woollens, and other similar fabricks, by means of which improvement, shawls and handkerchiefs can be printed with one or more colour or colours, and whereby linens, calicoes, silks, woollens, and other fabrics of the like nature, for garments, can be printed with two or more colours. July 20, 1820.

John Hudswell, of Addle-street, London, for an improvement in the manufactory of wafers. July 20, 1820.

William Dell, of Southampton, for an improvement in gun-barrels, July 20, 1820.

Robert Bowman, of Manchester, for improvements in the construction of looms for weaving various sorts of cloths, which looms may be set in motion by any adequate power. July 20, 1820.

ARTICLE XV.

Magnetical and Meteorological Observations.
By Col. Beaufoy, F.R.S.

Bushey Heath, near Stanmore.

Latitude 51° 37' 44" 27" North. Longitude West in time 1' 20.93".

Magnetical Observations, 1820. — Variation West.

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
July 1	8h 45'	24° 27' 51"	1h 30'	24° 37' 49"	7h 55'	24° 34' 08"
2	8 40	24 27 42	— —	— — —	7 40	24 35 12
3	8 35	24 27 37	1 25	24 39 00	7 35	24 33 38
4	8 40	24 28 55	1 20	24 37 54	7 45	24 34 35
5	8 40	24 28 12	1 20	24 38 18	7 50	24 33 10
6	8 40	24 28 09	1 20	24 38 27	7 35	24 34 27
7	8 35	24 30 02	1 25	24 39 28	— —	— — —
8	8 40	24 29 16	1 25	24 36 04	7 45	24 34 54
9	8 35	24 29 11	1 40	24 37 48	7 45	24 34 32
10	8 35	24 30 47	1 40	24 40 08	7 55	24 32 18
11	8 35	24 26 37	1 15	24 38 32	— —	— — —
12	8 35	24 28 10	1 20	24 38 32	7 55	24 34 22
13	8 40	24 27 40	1 20	24 39 12	7 50	24 33 27
14	8 35	24 27 29	1 15	24 38 19	— —	— — —
15	8 45	24 28 47	1 30	24 38 49	7 50	24 32 13
16	8 45	24 29 35	1 25	24 39 32	7 45	24 32 52
17	8 45	24 29 05	— —	— — —	7 45	24 35 23
18	— —	— — —	— —	— — —	— —	— — —
19	8 35	24 28 28	1 15	24 37 01	7 45	24 34 27
20	8 35	24 28 20	1 20	24 38 20	— —	— — —
21	8 35	24 29 26	1 25	24 37 32	7 40	24 35 00
22	8 40	24 27 47	1 15	24 38 04	7 35	24 33 04
23	8 45	24 28 48	1 40	24 39 52	7 40	24 34 16
24	8 40	24 30 41	1 50	24 39 02	7 50	24 32 33
25	8 40	24 29 47	1 20	24 39 17	7 45	24 32 17
26	8 35	24 28 20	1 20	24 39 57	7 50	24 33 44
27	8 45	24 28 26	1 20	24 41 41	7 55	24 35 00
28	8 40	24 30 34	1 25	24 42 14	— —	— — —
29	8 40	24 28 55	1 20	24 40 43	7 45	24 34 13
30	8 35	24 28 48	1 45	24 42 18	7 40	24 33 25
31	8 40	24 30 05	1 25	24 38 18	7 45	24 32 43
Mean for Month.	} 8 39	24 28 41	1 25	24 39 00	7 46	24 33 26

Meteorological Observations.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
July		Inches.				Feet.		
1	Morn....	29.744	56°	60°	NE by N		Fine	50°
	Noon....	29.767	60	58	NE		Cloudy	63
2	Even....	29.769	57	63	N by W		Very fine	} 49
	Morn....	29.688	59	64	W by S		Cloudy	
3	Noon....	29.635	—	—	W by S		Rain	63
	Even....	29.563	57	81	NNW		Showery	} 48
4	Morn....	29.500	55	74	NW		Cloudy	
	Noon....	29.473	58	71	NW		Showery	51 $\frac{3}{3}$
5	Even....	29.447	54	73	WNW		Showery	} 51 $\frac{3}{3}$
	Morn....	29.490	56	72	NNW		Cloudy	
6	Noon....	29.524	58	64	N by E		Cloudy	51
	Even....	29.550	54	65	NNE		Cloudy	47 $\frac{1}{2}$
7	Morn....	29.638	55	64	NNE		Cloudy	63 $\frac{1}{2}$
	Noon....	29.652	57	62	NNE		Cloudy	50 $\frac{2}{3}$
8	Even....	29.664	55	64	NE		Cloudy	65
	Morn....	29.688	56	66	NE		Fine	} 50 $\frac{2}{3}$
9	Noon....	29.690	62	62	NE		Cloudy	
	Even....	29.693	58	65	NNE		Fine	} 52 $\frac{1}{3}$
10	Morn....	29.678	57	71	NNE		Cloudy	
	Noon....	29.683	62	63	NNE		Fine	51 $\frac{3}{4}$
11	Even....	—	—	—	—		—	65 $\frac{2}{3}$
	Morn....	29.732	56	66	NNE		Cloudy	54 $\frac{1}{3}$
12	Noon....	29.732	59	60	NNE		Cloudy	69
	Even....	29.715	55	62	NNE		Cloudy	} 51 $\frac{3}{4}$
13	Morn....	29.713	55	68	Var.		Sm. rain	
	Noon....	29.703	64	60	E		Cloudy	54 $\frac{1}{3}$
14	Even....	29.700	58	68	E		Cloudy	69
	Morn....	29.688	60	65	ESE		Cloudy	} 51
15	Noon....	29.663	68	56	Var.		Fine	
	Even....	29.635	59	65	E		Very fine	50 $\frac{2}{3}$
16	Morn....	29.618	60	65	E		Fine	69
	Noon....	29.600	67	55	E		Very fine	} 53
17	Even....	—	—	—	—		—	
	Morn....	29.500	58	68	E		Fine	52 $\frac{3}{4}$
18	Noon....	29.468	66	60	E		Very fine	67
	Even....	29.422	59	68	E		Cloudy	} 58
19	Morn....	29.379	59	68	E		Cloudy	
	Noon....	29.376	61	62	E		Cloudy	56
20	Even....	29.379	58	65	NE		Cloudy	75 $\frac{2}{3}$
	Morn....	29.377	60	66	NE		Cloudy	60 $\frac{1}{2}$
21	Noon....	29.415	65	64	NE		Cloudy	68
	Even....	—	—	—	—		—	} 54
22	Morn....	29.500	64	73	S by W		Cloudy	
	Noon....	29.507	70	62	Var.		Cloudy	—
23	Even....	29.519	64	65	SSW		Cloudy	—
	Morn....	29.507	67	66	ESE		Fine	—
24	Noon....	29.485	73	59	SSW		Thun. rn.	—
	Even....	29.452	67	68	E		Thunder	—
25	Morn....	29.174	67	79	SE		Showery	—
	Noon....	29.086	—	84	SSW		Rain	—
26	Even....	29.044	61	80	SW		Fine	—
	Morn....	28.938	—	85	SE		Rain, th.	—
27	Noon....	—	—	—	—		—	—
	Even....	—	—	—	—		—	—

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
July		Inches.				Feet.		
19	Morn....	29·000	62°	73°	WSW		Fine	53
	Noon....	29·054	66	63	W by N		Showery	70
	Even....	29·163	60	76	SSW		Th. show.	} 54
20	Morn....	29·240	61	76	SSW		Very fine	
	Noon....	29·258	70	55	Var.		Very fine	72 $\frac{1}{4}$
	Even....	—	—	—	—		Thun. rn.	} 57 $\frac{1}{2}$
21	Morn....	29·400	60	84	NNW		Fog	
	Noon....	29·457	67	64	SW		Thun. rn.	69 $\frac{1}{2}$
	Even....	29·460	63	75	NW		Showery	} 51
22	Morn....	29·483	59	67	W		Very fine	
	Noon....	29·474	61	58	W		Cloudy	64
	Even....	29·462	58	64	WNW		Showery	} 54
23	Morn....	29·460	55	74	NW		Cloudy	
	Noon....	29·500	58	70	NW		Cloudy	62 $\frac{1}{3}$
	Even....	29·500	58	71	W by N		Cloudy	} 53
24	Morn....	29·531	60	71	NW		Cloudy	
	Noon....	29·531	70	59	W by N		Fine	71
	Even....	29·500	65	62	W		Cloudy	} 58
25	Morn....	29·418	64	67	W		Fine	
	Noon....	29·412	66	53	WNW		Fine	67 $\frac{3}{4}$
	Even....	29·455	56	67	WNW		Showery	} 51
26	Morn....	29·563	58	66	W by N		Fine	
	Noon....	29·591	67	57	W by N		Fine	69
	Even....	29·593	63	76	WSW		Fine	} 57 $\frac{1}{2}$
27	Morn....	29·604	63	73	W by S		Fine	
	Noon....	29·586	69	58	W by S		Fine	72 $\frac{1}{3}$
	Even....	29·580	63	64	W		Fine	} 56
28	Morn....	29·583	66	67	WNW		Fine	
	Noon....	29·600	70	63	W		Showery	7 $\frac{1}{4}$
	Even....	—	—	—	—		—	} 59
29	Morn....	29·628	66	63	SE		Very fine	
	Noon....	29·635	73	55	SW		Cloudy	74 $\frac{1}{4}$
	Even....	29·635	69	62	WNW		Fine	} 59 $\frac{2}{3}$
30	Morn....	29·643	68	65	SE		Cloudy	
	Noon....	29·610	77	55	Var.		Cloudy	77 $\frac{1}{4}$
	Even....	29·540	68	63	SE		Cloudy	} 58
31	Morn....	29·405	67	72	SW by S		Very fine	
	Noon....	29·405	77	55	Var.		Very fine	80
	Even....	29·395	72	68	S		Fine	

Rain, by the pluviometer, between noon the 1st of July, and noon the 1st of August, 1·936 inch. Evaporation, during the same period, 3·950 inches. On the 30th, at 10 in the evening, it commenced lightning from the S.W. to the S.E. and continued until half-past one in the morning. During half the time, the flashes were incessant. There was little wind, and the quantity of rain that fell was only 0·29 inch.

ARTICLE XVI.

METEOROLOGICAL TABLE.

1820.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.	Hygr. at 9 a. m.
		Max.	Min.	Max.	Min.			
7th Mo.								
July 1	N	30·21	30·18	65	40	—		69
2	W	30·18	30·02	65	45	—	14	70
3	N W	30·02	30·01	65	50	23	17	77
4	N	30·14	30·02	65	50	—		77
5	N	30·19	30·14	65	40	—		72
6	N E	30·20	30·18	65	44	24		73
7	N W	30·22	30·20	68	50	—		75
8	N	30·24	30·21	68	48	—		72
9	Var.	30·21	30·18	70	50	—		74
10	S E	30·18	30·13	74	41	36		72
11	N E	30·13	30·01	73	43	—		71
12	E	30·01	29·89	74	53	—		74
13	N E	29·90	29·89	67	51	31		73
14	N E	29·98	29·92	73	57	—		73
15	N E	29·99	29·98	75	47	—		76
16	Var.	29·98	29·65	80	58	—	86	74
17	Var.	29·65	29·37	72	50	25	1·08	82
18	S W	29·41	29·36	70	48	—	26	98
19	S W	29·68	29·41	73	48	—	06	90
20	S W	29·89	29·68	77	55	—	—	83
21	N W	29·94	29·89	72	50	34	05	85
22	N W	29·96	29·94	70	53	—		76
23	N W	29·99	29·96	63	52	—	—	78
24	N W	30·00	29·89	73	58	—		79
25	N W	30·02	29·89	73	50	55		77
26	N W	30·06	30·02	73	57	—		71
27	N W	30·04	30·02	76	56	—		77
28	Var.	30·07	30·04	78	55	45		76
29	S E	30·07	30·06	79	52	—		74
30	S E	30·06	29·88	84	54	—	76	74
31	S	29·88	29·84	86	54	44		79
		30·24	29·36	86	40	3·17	3·38	98—69

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Seventh Month.—1. Fine. 2, 3. Showery. 4—6. Overcast. 7—9. Cloudy. 10—13. Fine. 14. Cloudy. 15. Fine. 16. A heavy thunder shower about eleven, a. m.: an unusually heavy shower of rain about half-past one, p. m. 17, 18. Rainy. 19. Cloudy and fine. 20. *Cirrus: Cirrocumulus:* cloudy: showers, with some thunder. 22. Cloudy and fine. 23. Overcast. 24—26. *Cirrocumulus:* cloudy. 27. *Cirrocumulus: Cirrostratus: Cirrus.* 28. *Cirrus: Cirrocumulus.* 29. *Cirrus:* clear. 30. Fine day: a thunder storm commenced about ten, p. m. and continued about three and a half hours: the lightning extremely vivid and frequent: the thunder mostly distant and nearly continuous: at half-past eleven, after a very loud clap of thunder, heavy rain began with us.

RESULTS.

Winds: N, 4; NE, 5; E, 1; SE, 3; S, 1; SW, 3; W, 1; NW, 9; Var. 4.

Barometer: Mean height

For the month.	29·973 inches.
For the lunar period, ending the 1st:	30·035
For 13 days, ending the 1st (moon south)	30·160
For 14 days, ending the 15th (moon north)	30·084
For the lunar period, ending the 31st.....	29·966
For 13 days, ending the 28th (moon south)	29·838

Thermometer: Mean height

For the month.....	61·129°
For the lunar period, ending the 1st.	53·034
For the lunar period, ending the 31st	64·916
For 31 days, the sun in Cancer.	61·887

Evaporation. 3·17 inch.

Rain)..... 3·38

The mean of the hygrometer is for the latter 17 days of the month 79·35°, but the mean deduced for the like space from a new one now substituted for it is 64·88°: it appears, therefore, that the old one, the inaccuracy of which has been heretofore stated, will require a deduction of 14 degrees from its later results. It appears to have been employed from about Midsummer, 1819; and the error, which has apparently arisen from the stretching of a too slender piece of whalebone, has probably increased from that time to the present.

ANNALS

OF

PHILOSOPHY.

OCTOBER, 1820.

ARTICLE I.

On the Specific Gravity of Gases. By T. Thomson, M.D. F.R.S.

(Concluded from p. 177.)

IN the preceding part of this essay, I have endeavoured to establish the exact specific gravities of the four simple gases, chlorine, oxygen, azote, and hydrogen, and likewise of the gaseous compounds, which they form with each other. As these specific gravities constitute the true foundation of the atomic theory, all the weights of the more compound bodies being connected with them, and depending on them, I considered it worth while to be at every pains both experimentally and theoretically to arrive at the truth; and I entertain considerable hopes that I have approached very near absolute precision, if I have not altogether reached it. Hydrogen is the lightest of the gases; its specific gravity is 0.0694, if we reckon that of common air 1.0000.

Azote is 14 times heavier than hydrogen, or	$0.0694 \times 14 = 0.9722$
Oxygen 16	$0.0694 \times 16 = 1.1111$
Chlorine 36	$0.0694 \times 36 = 2.5000$

If we reckon the specific gravity of oxygen, unity, as is most convenient for the purpose of the atomic theory, then the specific gravities of these four gases will be as follows:

Oxygen	1.000
Hydrogen.	0.0625 or $\frac{1}{16}$ th of 1.0000

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Azote	0.875	or $\frac{14}{16}$ ths of 1.0000
Chlorine	2.25	or $\frac{36}{16}$ ths of 1.0000

Now if we reckon 1.000 the weight of an atom of oxygen, then an atom of each of the other three gases will be equal to the preceding numbers multiplied by 2.

The atom of hydrogen.	0.125	or 0.0625	× 2
azote	1.75	0.875	× 2
chlorine.	4.5	2.25	× 2

There cannot be a doubt that, reckoning the weight of an atom of oxygen to be unity, that of the other three gases is just double their specific gravities; consequently a given volume of oxygen gas contains twice as many atoms as the same volume of hydrogen, azote, or chlorine. Of course when we compare together the volumes of these gases, and wish to convert them into atoms, we must reckon half a volume of oxygen, as equivalent to an atom; and one volume of hydrogen, azote, and chlorine, to be an atom. Thus water is composed of half a volume of oxygen gas united to one volume of hydrogen gas. It is, therefore, a compound of one atom oxygen and one atom hydrogen. In like manner protoxide of azote is a compound of half a volume oxygen gas and one volume azotic gas; it consists, therefore, of one atom oxygen united to one atom azote. Deutoxide of azote being formed of one volume oxygen gas and one volume azotic gas must be a compound of two atoms oxygen and one atom azote. Muriatic acid being a compound of one volume hydrogen gas and one volume chlorine gas, and a volume of each of these gases being equivalent to an atom, it is obvious that muriatic acid is a compound of one atom hydrogen and one atom chlorine.

The specific gravities of steam, protoxide of azote, and ammonia, are as follows:

Steam	0.625
Protoxide of azote	1.5278
Ammonia.	0.5900

Or reducing them to what they would be supposing the specific gravity of oxygen unity:

Steam	0.5625
Protoxide of azote	1.375
Ammonia.	0.53125

If we multiply the first two of these last numbers by 2, and the last by 4, the products will be the weight of the atom of each of these gases; for

$$\begin{aligned}
 0.5625 \times 2 &= 1.125 = \text{an atom of steam} \\
 1.375 \times 2 &= 2.75 = \text{an atom of protoxide of azote} \\
 0.53125 \times 4 &= 2.125 = \text{an atom of ammonia}
 \end{aligned}$$

It is obvious that steam is a compound of

1 atom oxygen	= 1.000
1 atom hydrogen	= 0.125
	<hr/>
Atom of steam	= 1.125

Protoxide of azote is composed of

1 atom oxygen	= 1.00
1 atom azote	= 1.75
	<hr/>
Atom of protoxide	= 2.75

Ammonia is composed of

1 atom azote	= 1.750
3 atoms hydrogen	= 0.375
	<hr/>
Atom of ammonia	= 2.125

Hence it follows obviously that the volumes of which these gaseous compounds are constituted, when they unite together, are reduced to one half of their original volume. We see from this that they unite with considerable force, and that this force reduces their elasticity to one half of what it had been before the union.

The specific gravities of muriatic acid gas and of deutoxide of azote are as follows :

Muriatic acid	1.284
Deutoxide of azote	1.0416

Or, reckoning oxygen 1,

Muriatic acid	1.156
Deutoxide of azote	0.9375

Now if we multiply each of these numbers by 4, we obtain the weight of an atom of each of them. For

$$1.156 \times 4 = 4.625$$

$$0.9375 \times 4 = 3.75$$

It is obvious that muriatic acid is composed of

1 atom chlorine	= 4.5
1 atom hydrogen	= 0.125
	<hr/>
Atom of muriatic acid	= 4.625

Deutoxide of azote is composed of

2 atoms oxygen	= 2.00
1 atom azote	= 1.75
	<hr/>
Atom of deutoxide	= 3.75

A little consideration will satisfy the reader that when the gaseous bodies of which these compounds are constituted unite together they undergo no change whatever in their bulk. Probably, therefore, the force of affinity which occasions the union is considerably weaker than the affinity by which the four preceding gaseous compounds are kept united.

I shall proceed in the remaining part of this essay to endeavour to establish the exact specific gravities of the gaseous compounds formed by the union of the simple gases with the solid bodies carbon, sulphur, and phosphorus. These specific gravities will be found of great importance, because they will put it in our power to determine with precision the atomic weights of carbon, sulphur, and phosphorus; bodies which act so very prominent a part in chemistry, and which enter into so vast a number of important combinations.

I. CARBON.

Carbon forms gaseous compounds with oxygen, hydrogen, and azotic gas, and there is a gaseous compound known composed of carbon, oxygen, and chlorine.

Carbon and oxygen form .. carbonic acid,
carbonic oxide.

Carbon and hydrogen form olefiant gas,
carburetted hydrogen.

Carbon and azote form. .. cyanogen,
The triple compound is .. chlorocarbonic acid or phosgene gas.

1. *Carbonic Acid Gas*.—A quantity of very pure carbonic acid gas was prepared by dissolving calcareous spar in muriatic acid. Its specific gravity taken three times with the necessary precautions to ensure accuracy was as follows:

By first trial.	1.5266
By second trial	1.5268
By third trial	1.5268
Mean	<u>1.52673</u>

The first person who attempted to determine the specific gravity of this gas was Mr. Cavendish, who fixed it at 1.57;* but his data were obviously inaccurate; though we have not the means of rectifying them. He filled a bladder capable of holding 100 ounce measures with the gas, and weighed it. The gas was then squeezed out, and the bladder being weighed again was found to weigh 34 grs. less than at first. Now if we suppose 100 ounce measures to be equal to 189.8 cubic inches, and 100 cubic inches of common air to weigh 30.5 grs. it is obvious that Mr. Cavendish's experiment will give us the specific gravity of

* Phil. Trans, 1766, p. 141.

carbonic acid gas, 1.587. But these data are too loose to enable us to come to precise conclusions.

Lavoisier gives us the result of his experiments on the specific gravity of carbonic acid gas in his *Elements*.* He found it to be 1.4993. Biot and Arago, in the year 1806, published the result of a set of experiments undertaken to determine the specific gravity of different gases. They fix the specific gravity of carbonic acid gas at 1.5196.† Thus we have four different determinations of the specific gravity of this gas, each founded on experiment. According to

Cavendish, it is	1.587
Lavoisier	1.4993
Biot and Arago.	1.5196
Thomson	1.52673

Now, if we except the determination of Cavendish, which, from the reasons above stated, does not seem entitled to much confidence, it is obvious that each experimenter has found the specific gravity of this gas higher than his predecessor. Lavoisier, who was first in order, makes it the lightest; Biot and Arago, who followed him at an interval of nearly 20 years, found it heavier. The experiments in my laboratory were made 14 years later, and we found the gas heaviest of all. Now this is just what ought to have happened, supposing that every succeeding experimenter employed greater care than his predecessor to exclude common air from his carbonic acid gas as completely as possible; for common air being much lighter than carbonic acid gas, it is obvious that the intermixture of a very small portion of it would produce a sensible change on the specific gravity of the carbonic acid gas subjected to experiment. Had Lavoisier's carbonic acid been mixed with a very little more than five per cent. of common air, the specific gravity of such a mixture would have been just what he states his carbonic acid to be. Now whoever has paid the least attention to the manner of making experiments on gases 30 years ago will have no hesitation in admitting that Lavoisier's carbonic acid gas might very likely be contaminated with even a greater proportion of air than we have here supposed. The carbonic acid gas of Biot and Arago was probably contaminated with a mixture of $1\frac{1}{2}$ per cent. of common air.

Even the carbonic acid gas, whose specific gravity was taken in my laboratory, was not absolutely pure. It was collected over common river water. Now such water always contains some common air, part of which is disengaged when any other gas is placed over it. Of course a small portion of air would of necessity mix itself with the carbonic acid gas, and of consequence diminish its specific gravity somewhat. Upon trying the good-

* English Translation, p. 569.

† Mem. de l'Institut, 1806, p. 320.

ness of our carbonic acid gas, we found that when 200 volumes of it were absorbed by water, one volume of unabsorbable gas remained. But this proportion I believe to be excessive; because when water is saturated with carbonic acid gas, the whole of the common air which it previously contained is driven off; so that part of this residue was actually separated from the water. I am disposed to think that the carbonic acid gas which we experimented on was composed of a mixture of $505\frac{1}{6}$ volumes of pure carbonic acid and one volume of air; or that it contained nearly $\frac{1}{566}$ th part of its volume of common air. If this opinion be well founded, the true specific gravity of carbonic acid gas is 1.5277. The result which we obtained is only $\frac{1}{1469}$ th part less than this number—a result which is certainly far within the limits of unavoidable error from the experiment. I shall now state the reasons why I consider 1.5277 as the true specific gravity of carbonic acid gas.

It has been demonstrated that carbonic acid gas is a compound of carbon and oxygen, and that when oxygen gas is converted into carbonic acid gas, its volume is not altered. Therefore to ascertain the weight of carbon which a given volume of carbonic acid gas contains, we have only to subtract the specific gravity of oxygen gas from that of carbonic acid gas, the remainder represents the weight of carbon united with 1.1111 of oxygen.

Specific gravity of carbonic acid gas.	1.5277
Specific gravity of oxygen gas	1.1111
	<hr style="width: 100px; margin-left: auto; margin-right: 0;"/>
Carbon	= 0.4166

The number 0.4166 must represent the specific gravity of a volume of carbon, supposing it in the gaseous state, and carbonic acid gas must be considered as a compound of one volume oxygen gas and one volume of carbon condensed into one volume, or of two atoms of oxygen and one atom of carbon.

If we represent the specific gravity of oxygen by unity, then the specific gravity of carbon will become 0.375; for it is obvious that 1.1111 : 1 :: 0.4166 : 0.375; but $0.375 \times 2 = 0.75$. Now 0.75 represents the weight of an atom of carbon—certainly very nearly—I am of opinion accurately.

Carbon agrees with hydrogen, azote, and chlorine, in the respect that the weight of its atom is just double that of its specific gravity, reckoning the specific gravity of oxygen unity. We have found that the weights of the atoms of oxygen, azote, and chlorine, are multiples of the weight of the atom of hydrogen; for the weight of the atom of hydrogen is 0.125, and

$$0.125 \times 8 = 1.00 = \text{atom of oxygen}$$

$$0.125 \times 14 = 1.75 = \text{atom of azote}$$

or $0.125 \times 36 = 4.5 =$ atom of chlorine. I conclude from analogy that the atom of carbon is likewise a multiple of the atom of hydrogen. Now $0.125 \times 6 = 0.75$. So that if 0.75 be the weight of an atom of carbon, we see that it is just six times heavier than the atom of hydrogen.

Were we to determine the weight of an atom of carbon from the specific gravity of carbonic acid gas as found in my laboratory we should obtain the number 0.7481166. Now this number is only $\frac{1}{1000}$ th less than 0.75. I have given reasons which are perfectly sufficient to satisfy every practical chemist that the specific gravity of carbonic acid gas, as determined by us, is rather below the truth. So that the weight of an atom of carbon, as determined experimentally, cannot differ more than $\frac{1}{1000}$ th part from the theoretical number 0.75. No person the least accustomed to consider the simplicity of the laws of nature can, under such circumstances, entertain the least doubt that the true number which represents the weight of an atom of carbon is 0.75. The admission of it will bring carbon under the same law as hydrogen, azote, and chlorine, and will introduce a simplicity into its combinations that is truly delightful.

I may add that Dr. Wollaston in his very useful sliding rule has already adopted 0.75 as the weight of an atom of carbon. Dr. Prout has done the same in his paper on the "Relation between the Specific Gravities of Gaseous Bodies and the Weight of their Atoms." (*Annals of Philosophy*, vol. vi. p. 321.) Even Berzelius, who seems never to have thought of any relations subsisting between the atomic weights of different bodies, but to have trusted entirely to experiment, has adopted the number 0.7533, which does not differ so much as $\frac{1}{100}$ th part from 0.75. Indeed the number 0.75 for the weight of an atom of carbon has been in general use in this country for several years. But if 0.75 be the weight of an atom of carbon, 0.75 must represent the specific gravity of vapour of carbon, reckoning the specific gravity of oxygen gas unity, or 0.4166, if we reckon common air unity, as is usually done; and the specific gravity of carbonic acid gas must be just equal to that of oxygen gas and of carbon added together; that is to say, it must be 1.5277, the number which I have pitched upon above.

In giving the history of the different experiments to determine the specific gravity of carbonic acid gas, I did not notice the result obtained by M. T. de Saussure; namely 1.5266. This result is almost the same with ours, differing only by unity from our two last trials, in the fourth decimal place. The small difference is in favour of M. de Saussure being on the side of accuracy. His result is nearer the truth than ours; but the difference is so small that it seemed hardly entitled to notice. Saussure's gas must have been a mixture of 657.6 volumes of carbonic acid gas and one volume of common air—a mixture so

near purity, that very great precautions indeed must be taken before carbonic acid gas can be obtained in a state of greater purity.

2. *Carbonic Oxide*.—The carbonic oxide gas upon which the following experiments were made was obtained by mixing together equal weights of clean iron filings and pounded chalk previously dried by exposure to a red heat, and exposing the mixture to a strong heat in an iron bottle. The specific gravity of the gas thus obtained was as follows :

By first trial	1.9694
By second trial	1.9700
By third trial	1.9700
Mean	<u>0.9698</u>

We have two sets of experiments to determine the specific gravity of this gas, one by Clement and Desormes, and another by Cruikshanks ; both made soon after the discovery of the gas ; and in neither of them, as is obvious from the result of the experiments, was the gas operated upon pure. The specific gravity of this gas, according to

Clement and Desormes, is.	0.9409
Cruikshanks.	0.9569

These numbers are considerably lower than ours ; but as the gases by means of which they were determined were certainly impure, it seems unnecessary to make any observations upon the specific gravities deduced from them. Even our gas, though very great precautions were taken in preparing it, was not perfectly pure. If the chalk be not absolutely free from water, which is not easily accomplished, some hydrogen gas is always evolved, which, acting upon the carbonic acid gas, which is driven off along with the carbonic oxide, seems to give origin to a portion of carburetted hydrogen gas. But it is easy to demonstrate from other considerations what the specific gravity of pure carbonic oxide must be.

It has been sufficiently ascertained by experiment that when one volume of pure carbonic oxide gas is mixed with half a volume of oxygen gas, and an electrical spark passed through the mixture, the two gases unite, and form exactly one volume of carbonic acid gas. It is obvious from this, that if from the specific gravity of carbonic acid gas we subtract half the specific gravity of oxygen gas, the remainder must give us the specific gravity of carbonic oxide gas.

Sp. gr. of carbonic acid gas.	= 1.5277
Half sp. gr. of oxygen gas	= <u>0.5555</u>
Sp. gr. of carbonic oxide gas	= 0.9722

We see from this that the true specific gravity of this gas when in a state of purity is 0.9722. The result which we obtained is less than the truth by $\frac{1}{40}$ th part. This is a greater error than we could have committed if our gas had been free from impurity. But if we suppose the gas upon which we tried our experiments to have been a mixture of 171 volumes of pure carbonic oxide gas and 1 volume of carburetted hydrogen gas, such a mixture would have the very specific gravity which we found. Now though I have prepared carbonic oxide gas by heating iron filings and chalk more than 40 times, I have never obtained it more free from carburetted hydrogen gas than the specimen employed for ascertaining its specific gravity.

I consider it then as demonstrated, that carbonic oxide and carbonic acid gases are composed as follows :

$$\begin{array}{r} \text{Carbonic oxide of half volume of oxygen gas} = 0.555\dot{5} \\ \text{one volume carbon} \dots\dots = 0.416\dot{6} \\ \hline = 0.972\dot{2} \end{array}$$

So that the diminution of volume is one-third.

$$\begin{array}{r} \text{Carbonic acid of 1 volume oxygen} = 1.111\dot{1} \\ \text{1 volume carbon} \quad 0.416\dot{6} \\ \hline 1.527\dot{7} \end{array}$$

So that the diminution of volume is one-half.

Or substituting atoms for volumes, carbonic oxide is a compound of

$$\begin{array}{r} 1 \text{ atom oxygen} \dots\dots\dots = 1.00 \\ 1 \text{ atom carbon} \dots\dots\dots = 0.75 \\ \hline \text{Atom of carbonic oxide} \dots\dots\dots = 1.75 \end{array}$$

Carbonic acid is a compound of

$$\begin{array}{r} 2 \text{ atoms oxygen} \dots\dots\dots = 2.00 \\ 1 \text{ atom carbon} \dots\dots\dots = 0.75 \\ \hline \text{Atom of carbonic acid} \dots\dots\dots = 2.75 \end{array}$$

So that in both of these gases the weight of the atom is double the specific gravity, reckoning the specific gravity of oxygen unity; for in that case we have the specific gravities of these gases as follows :

$$\begin{array}{r} \text{Carbonic oxide } 0.875 \text{ and } 0.875 \times 2 = 1.75 \\ \text{Carbonic acid } 1.375 \text{ and } 1.375 \times 2 = 2.75 \end{array}$$

Thus these two gases exactly agree with hydrogen, azote, chlorine, steam, and protoxide of azote.

3. *Olefiant Gas*.—Olefiant gas is obtained by mixing together

four parts of sulphuric acid and one part of alcohol by weight, and exposing the mixture in a retort to the heat of a lamp. The gas comes over, and may be received over water. The only precaution is to take care that all the common air in the retort be driven off before we begin to collect the olefiant gas. If the mixture of sulphuric acid and alcohol be kept hot after it has become black, there is always evolved a considerable portion of carbonic acid gas mixed with some sulphurous acid gas. The sulphurous acid is immediately absorbed by the water; but to get rid of all the carbonic acid, the best way is to wash the olefiant gas in some milk of lime. All these precautions were attended to in the experiments made in my laboratory to determine the specific gravity of olefiant gas. The following were the results obtained:

By first trial	0.9709
By second trial	0.9709
By third trial	0.9709

We have two previous sets of experiments to determine the specific gravity of this gas. The Dutch chemists who discovered the gas found its specific gravity 0.909. In a set of experiments which I made on it in 1810, I determined its specific gravity to be 0.9745. Both of these results differ from what has been just given; but from the great pains taken to obtain the gas pure, there can be no doubt that our present results are much nearer the truth than those of the Dutch chemists, and at least as near as my own former ones.

As we know the specific gravity of hydrogen gas and carbon vapour, the two constituents of this gas, and as we are in possession of accurate experiments respecting the proportion in which these two elements are combined in it, we can easily determine its true specific gravity by calculation.

It has been ascertained by experiment that one volume of pure olefiant gas requires for complete combustion three volumes of oxygen gas, and that the residue after combustion amounts to two volumes of carbonic acid. Now the two volumes of carbonic acid required for their formation two volumes of oxygen gas. The other volume of oxygen gas consumed must have combined with hydrogen and formed water, and it must have combined with a quantity of hydrogen which, had it been in the gaseous state, would have amounted to two volumes. Two volumes of carbonic acid gas contain just two volumes of carbon. We see from this experimental result that a volume of olefiant gas is composed of

2 volumes carbon }
hydrogen. } condensed into one volume.

Therefore, to obtain the specific gravity of olefiant gas, we

have only to add together twice the specific gravity of hydrogen gas, and twice the specific gravity of carbon vapour.

Specific gravity of hydrogen	0.0694	and	0.0394	$\times 2 =$	0.1388
carbon ..	0.4166	and	0.4166	$\times 2 =$	0.8333
olefiant gas.	= 0.9722				

Thus we see that the true specific gravity of olefiant gas is 0.9722. This specific gravity neither agrees with my former determination nor with our late results; but if we take the mean of the two sets of experiments, we obtain a number which approaches very near the truth.

Sp gr. by my former experiments ...	0.9745
recent experiments ...	0.9709
Mean	= 0.9727

This differs from the truth only $\frac{1}{1034}$ th part, and is, therefore, greatly within the limits of unavoidable error from experiment. I have no doubt that the excess of my first specific gravity, which amounts to $\frac{1}{414}$ d part, was owing to the gas having been imperfectly freed from carbonic acid. A mixture of 239.5 volumes of olefiant gas and one volume of carbonic acid gas would have exactly the specific gravity which I obtained. It is not so easy to account for the error in defect of the last experiment, which amounts to $\frac{1}{747}$ th part, unless we suppose that a small portion of carburetted hydrogen gas had been generated. A mixture of 318 $\frac{1}{2}$ volumes of olefiant gas and 1 volume of carburetted hydrogen would have precisely the specific gravity of 0.9709, which was the result obtained.

Upon the whole, there does not seem the least reason for hesitating to admit 0.9722 for the true specific gravity of olefiant gas. It is a compound of two atoms of hydrogen and two atoms of carbon. Hence the weight of an atom of it is 1.75. Now this number is just double the specific gravity of olefiant gas, reckoning the specific gravity of oxygen gas unity; for its specific gravity in that case will be reduced to 0.875; and $0.875 \times 2 = 1.75$.

It deserves attention that the specific gravities and the weights of the atoms of azotic gas, carbonic oxide gas, and olefiant gas, are all the same. The specific gravity of each is 0.9722, and the weight of an atom of each is 1.75, or 14 times the weight of an atom of hydrogen.

We might indeed consider olefiant gas as a compound of one atom hydrogen and one atom carbon. This would reduce the atomic weight to 0.875, which is precisely equal to the specific gravity, reckoning oxygen unity. This was the view of its constitution which I entertained when I published the fifth edition

of my System of Chemistry; but I neither consider it as so simple nor so satisfactory as the view now given.

4. *Carburetted Hydrogen*.—I have often obtained by distilling coal and whale oil, gases which possessed very nearly the chemical properties of carburetted hydrogen gas; but the specific gravity of the gases obtained by distilling these bodies varies so much that there cannot be the least doubt of their being mixtures of various gases, some of which, in all probability, we are still unacquainted with. The only method with which I am acquainted by means of which we can procure pure carburetted hydrogen gas in a state of purity is to collect the gas which is evolved when we stir the bottom of stagnant ditches filled with water in the hot weather of summer. By this simple method we can collect it in abundance. It is usually as first collected contaminated with carbonic acid; but this gas is easily removed by means of milk of lime. I have sometimes also found a mixture of common air, but this is not always present.

The following are the results of the trials made in my laboratory to determine the specific gravity of this gas collected in the way just mentioned:

By first trial.	0.5602
By second trial.	0.5558
By third trial.	0.5570
Mean.	<hr style="width: 10%; margin: 0 auto;"/> 0.5576

am not aware of any previous determination of the specific gravity of this gas, except the one given by me 10 years ago, in a paper which I published on the gaseous combinations of carbon and hydrogen in the first volume of the Memoirs of the Wernerian Society. I found the specific gravity of the pure gas 0.5554. Mr. Dalton had indeed stated the specific gravity of this gas to be 0.600; but from the imperfection of his apparatus, all the specific gravities of gases which he gives can be considered only as rude approximations; but not at all precise enough to found important points of doctrine on. The difference between the experiments made in my laboratory last year and my former result amounts to $\frac{1}{253}$ d part of the whole. I believe the reason to be, that in my first experiments I ascertained the quantity of common air mixed with carburetted hydrogen gas, and made allowance for it; whereas that precaution was not taken in the experiments of last summer. A mixture of 209.6 volumes of pure carburetted hydrogen gas and one volume of common air would have had the specific gravity 0.5576. Now I am ignorant of a good method of detecting so small a mixture of common air as this in a gas of so difficult combustion as carburetted hydrogen gas.

But it is easy from the data established in the preceding part of this paper to demonstrate the absolute specific gravity of pure

carburetted hydrogen gas. One volume of this gas requires for complete combustion two volumes of oxygen gas, and there remains after the explosion exactly one volume of carbonic acid gas. Now one half of the oxygen must have gone to the formation of carbonic acid; the remaining volume of oxygen must have combined with hydrogen, and formed water, and it must have combined with a quantity of hydrogen which, if it had been in the gaseous state, would have amounted to two volumes. Carbonic acid gas contains exactly its own volume of vapour of carbon. It is obvious from this that a volume of carburetted hydrogen gas is composed of

1 volume vapour of carbon }
2 volumes hydrogen gas .. } condensed into one volume.

Consequently its specific gravity must be equal to twice the specific gravity of hydrogen gas + the specific gravity of carbon vapour.

Sp. gr. of hydrogen = 0.0694	and	0.0694 × 2 = 0.1388
carbon .. = 0.4166	0.4166
carburetted hydrogen.		= 0.5555

It is clear from this (if our data be accurate) that the specific gravity of pure carburetted hydrogen gas is 0.5555. Now this differs less than $\frac{1}{5000}$ th part from the result of my experiments made in 1810—a degree of precision as near as can be expected in experiments of this nature. The latter experiments were less accurate, because the gas examined was much purer than what I employed the first time; and though not absolutely pure, too much so to enable us to detect with certainty the proportion of common air which it contained.

We see from what has been stated that carburetted hydrogen is a ternary compound, consisting of two atoms of hydrogen united to one atom of carbon. Hence the weight of an integrant particle of it is 1.000, or the same as that of oxygen; for

2 atoms hydrogen.	= 0.25
1 atom carbon	= 0.75
	1.00

= an atom of carburetted hydrogen.

If we make the specific gravity of oxygen gas unity, then the specific gravity of carburetted hydrogen gas will become 0.5 and $0.5 \times 2 = 1.000$; so that the weight of the atom of carburetted hydrogen is just double its specific gravity. In this respect it agrees with olefiant gas, and with the other gases enumerated in the preceding part of this paper.

Carburetted hydrogen gas differs from olefiant gas merely in

having one atom less of carbon. The gaseous constituents of carburetted hydrogen are condensed into one-third of their bulk, and those of olefiant gas into one-fourth of their bulk, before combination. This demonstrates the great affinity that exists between carbon and hydrogen, and the intimacy of the union which they form.

5. *Cyanogen*.—This gas, which was discovered by Gay-Lussac, is evolved when dry prussiate of mercury is exposed to heat in a small retort. I have not succeeded well in obtaining this gas by means of an Argand's lamp; but when a few pieces of red-hot charcoal are applied to the bottom of the retort, the gas comes over in great abundance. It must be received over mercury; for water absorbs it too copiously to allow us to collect it in sufficient purity for experiment over that liquid. A sufficient quantity of it having been collected, and care having been taken to exclude all common air as completely as possible, its specific gravity was found to be as follows:

By first trial	1·8039
By second trial	1·8040
Mean	= 1·80395

The only person who preceded us in taking the specific gravity of this gas is Gay-Lussac, who states it at 1·8064.

Gay-Lussac's very ingenious and satisfactory analysis of this gas puts it in our power to determine its true specific gravity from the data contained in the preceding part of this essay. One volume of it for complete combustion requires two volumes of oxygen gas. After combustion, there remain two volumes of carbonic acid gas and one volume of azotic gas. It is obvious from this that it is composed of

2 volumes carbon vapour }
1 volume azotic gas . . . } condensed into one volume.

Consequently its specific gravity must be equal to twice the specific gravity of carbon vapour + the specific gravity of azotic gas.

$$\begin{aligned} \text{Sp. gr. of carbon ..} &= 0\cdot416\dot{6} \text{ and } 0\cdot416\dot{6} \times 2 = 0\cdot833\dot{3} \\ \text{azotic gas} &= 0\cdot972\dot{2} \text{} = 0\cdot972\dot{2} \\ &\hline &1\cdot805\dot{5} \end{aligned}$$

Thus we see that the specific gravity of pure cyanogen is 1·8055. Our result is $\frac{1}{1127\cdot4}$ th part below the truth. The gas on which we experimented must have been a mixture of 1127·4 volumes of pure cyanogen and one volume of air. Now when we collect gases over mercury, it is exceedingly difficult to obtain them free from some little mixture of common air, owing

to the thin film of air which adheres with obstinacy to the inside of the glass. Indeed unless the mercury were previously boiled in the jars, I question whether it would be possible to procure the cyanogen purer than we got it. Gay-Lussac's result is within less than $\frac{1}{2000}$ th part of the truth. His error is in excess, mine in deficiency. If we were to take the mean of the two, we should get 1.805175 for the specific gravity of cyanogen—a number which does not differ much more than $\frac{1}{3000}$ th part from the truth. I consider this to be as near an approximation to absolute precision as we can well attain in such experiments.

Cyanogen is a ternary compound consisting of two atoms of carbon and one atom of azote; so that the weight of an integrant particle of it is 3.25; for

$$\begin{array}{rcl} 2 \text{ atoms carbon.} & \dots\dots\dots & = 1.50 \\ 1 \text{ atom azote.} & \dots\dots\dots & = 1.75 \\ & & \hline & & 3.25 \end{array}$$

= an atom of cyanogen.

If we make the specific gravity of oxygen gas unity, then the specific gravity of cyanogen will be reduced to 1.625. Now $1.625 \times 2 = 3.25$; so that the weight of an atom of cyanogen is precisely double its specific gravity. In this respect it agrees with the greater number of the gases mentioned in the preceding part of this essay.

The gaseous constituents in cyanogen are reduced to one-third of their original volume. This proves the strong affinity which exists between them, and the intimacy of the combination which they form.

6. *Chlorocarbonic Acid.*—This gas was discovered by Dr. John Davy. It may be formed by mixing together equal volumes of carbonic oxide and chlorine gas, and exposing the mixture to the direct rays of the sun inclosed in a glass jar. The colour disappears, and the bulk of the mixture is reduced to one half. This was the process employed to form it in my laboratory. In consequence of the difficulty attending the process, we were satisfied with a single trial, which gave us the specific gravity of this gas 3.4604.

I am not aware of any previous attempt to determine the specific gravity of this gas experimentally; but it is obvious that its true specific gravity is just equal to the specific gravity of chlorine gas + the specific gravity of carbonic oxide gas.

$$\begin{array}{rcl} \text{Sp. gr. of chlorine gas} & \dots\dots\dots & = 2.5 \\ \text{carbonic oxide gas} & \dots\dots & = 0.9722 \\ & & \hline & & 3.4722 \end{array}$$

= specific gravity of chlorocarbonic acid.

Our determination is about $\frac{1}{300}$ th part less than the truth, and

indicates a gas composed of 207.5 volumes of chlorocarbonic acid and one volume of common air. This is as good an approximation to accuracy as could have been looked for in such an experiment.

We see that chlorocarbonic acid is a compound of one atom chlorine + one atom carbonic oxide. Hence the weight of an integrant particle of it is 6.25; for

$$\begin{array}{r} 1 \text{ atom chlorine.} = 4.50 \\ 1 \text{ atom carbonic oxide.} = 1.75 \\ \hline \phantom{1 \text{ atom carbonic oxide.}} = 6.25 \end{array}$$

= an atom chlorocarbonic acid.

If we reckon the specific gravity of oxygen gas unity, then the specific gravity of chlorocarbonic acid will be 3.125. Now $3.125 \times 2 = 6.25$; so that the atomic weight of this gas, like so many others, is just double its specific gravity.

Whoever considers attentively the preceding discussion will, I think, entertain no doubt that the specific gravity of carbon vapour is 0.4166, and that the weight of an atom of carbon is 0.75.

II. SULPHUR.

Sulphur does not enter into so many gaseous compounds as carbon, or at least we are acquainted at present with only two permanently elastic fluids, into which it enters as a constituent. These are

1. Sulphurous acid composed of sulphur and oxygen.
2. Sulphuretted hydrogen composed of sulphur and hydrogen.

These two, however, will put it in our power to determine the weight of an atom of sulphur, with as much precision as the atom of carbon.

1. *Sulphurous Acid*.—This acid gas is easily obtained by mixing together a mixture of mercury and concentrated sulphuric acid in a retort, and heating the mixture by an Argand's lamp. The mercury is oxidized at the expense of the sulphuric acid; which, being deprived of one-third of its oxygen, flies off in the state of sulphurous acid gas. It must be collected over mercury. A sufficient quantity of pure gas being prepared in this way, its specific gravity was found to be as follows:

By first trial	2.2221
By second trial.	2.2221
By third trial.	2.2223
	2.22216
Mean	2.22216

We have two previous sets of experiments to determine the specific gravity of this gas.

According to Kirwan, it is. 2·265
 Davy. 2·1930

These two results do not agree with each other, the one being higher and the other lower than ours. We were at such great pains to procure the gas perfectly pure, and the specific gravities were taken with such scrupulous attention to accuracy, that I am disposed to consider our result as approaching as near accuracy as our apparatus and method of proceeding would admit; near enough I am persuaded to enable us to determine the weight of an atom of sulphur with perfect precision.

When sulphur is burned in oxygen gas, that gas is converted into sulphurous acid gas without undergoing any alteration in its bulk. It follows from this, that in order to determine the proportions of oxygen and sulphur of which this gas is composed, we have only to subtract the specific gravity of oxygen gas from that of sulphurous acid gas. The remainder will be the weight of sulphur, that of the oxygen in the gas being denoted by its specific gravity. This is the same thing as if we said that a volume of sulphurous acid gas is composed of a volume of oxygen gas and a volume of vapour of sulphur condensed into one volume; so that the remainder gives us in fact the specific gravity of vapour of sulphur.

Sp. gr. of sulphurous acid gas.	2·22216
oxygen gas	1·11111
	1·11105

It appears then from our experiments that sulphurous acid gas is composed of

Oxygen	1·11111	or	1·00000
Sulphur	1·11105		0·99995

Now these two numbers 1 and 0·99995 differ from each other by only $\frac{1}{100000}$ th part—a quantity greatly within the limits of unavoidable error from experiment. It is impossible to doubt that in reality the two numbers are precisely equal, and that the very small deficiency was owing to some small error committed in taking the specific gravity. If we admit the equality of the numbers, then the weight of an atom of sulphur will be a multiple of that of an atom of hydrogen, as is the case with the atom of oxygen, azote, chlorine, and carbon; and the weight of an atom of sulphur will be precisely double the specific gravity of sulphur vapour, as we have seen to be the case with so many other gaseous substances; but if we adhere absolutely to the result of experiments without admitting any error whatever, according to the method of Berzelius; then none of these coincidences hold—the whole simplicity disappears—and, what is most provoking, we have only to admit an error amounting to

$\frac{1}{20000}$ th part of the whole, and the whole perplexity vanishes, and the numbers resume that simplicity and accordance with each other which marks a law of nature.

If there be any person who has a reluctance to yield his assent to the above conclusions, and to admit that the specific gravity of sulphur vapour and of oxygen gas are precisely equal, I flatter myself that the following observations will remove his remaining doubts.

In the year 1803, I demonstrated by a set of conclusive experiments that the oxygen in sulphurous acid is precisely two-thirds of the oxygen in sulphuric acid.* This conclusion is now universally admitted by every chemist without exception. Let us now observe the gradual progress of the attempts made by chemists to determine the exact composition of sulphuric acid. Sulphuric acid is composed, according to

	Sulphur.	Oxygen.
Lavoisier, of	100 +	40·845
Chenevix, of	100 +	62·601
Bucholz, of	100 +	135·300
Klaproth, of	100 +	136·190
Richter, of	100 +	138·100
Berzelius, of	100 +	146·85
Ditto, by later experiment,	100 +	148·44
Thomson, from the specific gravity of sulphurous acid,	100 +	149·9925

In these results it is obvious that there is a constant approach towards 150 as the quantity of oxygen united to 100 of sulphur. Indeed in the last result deduced from the specific gravity of sulphurous acid gas, as stated in this essay, the oxygen is only $\frac{1}{20000}$ th less than 150. Is it possible for any one that considers the above series of results with attention not to conclude that the true composition of sulphuric acid is

Sulphur	100
Oxygen	150

Even Berzelius has advanced considerably towards this proportion. His first experiments gave him sulphuric acid composed of

Sulphur	100·00
Oxygen	146·85

His last experiments, made on purpose to correct the former ones, gave him the acid composed of

Sulphur	100·00
Oxygen	148·44

* Nicholson's Journal, vi. 92.

But it is impossible that his experiments, how carefully soever made, can ever give him the exact truth, because the data which he constantly employs in all his calculations are inaccurate in several particulars. For example, he considers sulphate of barytes as a compound of

Sulphuric acid.	33·5
Barytes	66·5
	<hr/>
	100·0

Whereas its real composition is

Sulphuric acid.	33·89831
Barytes	66·10169
	<hr/>
	100·00000

But if sulphuric acid be a compound of

Sulphur.	100
Oxygen.	150

Then sulphurous acid is a compound of

Sulphur.	100
Oxygen.	100

Consequently the specific gravity of vapour of sulphur and of oxygen gas are the same, and the true specific gravity of sulphurous acid gas is 2·2222.

We are acquainted with three combinations of sulphur and oxygen; namely, hyposulphurous acid, sulphurous acid, and sulphuric acid; the composition of which is as follows:

	Sulphur.	Oxygen.
Hyposulphurous acid.	100 +	50
Sulphurous acid.	100 +	100
Sulphuric acid	100 +	150

No one who considers these compounds can hesitate to conclude that hyposulphurous acid is a compound of one atom sulphur + one atom oxygen; sulphurous acid of one atom sulphur + two atoms oxygen; and sulphuric acid of one atom sulphur + three atoms oxygen. Hence it follows that the atom of sulphur is twice as heavy as the atom of oxygen; so that an atom of sulphur weighs 2. Now 2 is a multiple of 0·125, the atom of hydrogen; for $0·125 \times 16 = 2$.

If we reckon the specific gravity of oxygen gas unity, then the specific gravity of the vapour of sulphur will be 1; and $1 \times 2 = 2$. We see then that the weight of an atom of sulphur is just twice the specific gravity of its vapour, as is the case with so many other bodies.

2. *Sulphuretted Hydrogen.*—This gas may be obtained pure

by pouring muriatic acid on pounded sulphuret of antimony in a retort, and exposing the mixture to a moderate heat. It may be procured likewise by mixing together equal weights of clean iron filings and flowers of sulphur with a sufficient quantity of water, and heating the mixture in a retort till it becomes black. Considerable heat is evolved during the combination of the iron and sulphur, which keeps the mixture boiling long after it is removed from the fire. It was by this last process that the sulphuretted hydrogen prepared in my laboratory for determining its specific gravity was prepared. Its specific gravity was found as follows :

By first trial	1·1780
By second trial	1·1796
By third trial	1·1796
	<hr/>
Mean	1·17906

There have been various previous experiments to determine the specific gravity of this gas ; though I have reason to believe that no preceding chemist has come so near the truth as we have done.

Kirwan found it	1·106*
Thenard	1·231†
Gay-Lussac and Thenard	1·1912‡
Davy	1·1967

Kirwan's gas was no doubt contaminated with common air, or perhaps even hydrogen gas ; for I have found that when sulphuretted hydrogen gas is obtained by the process which he employed, it usually contains a mixture of hydrogen gas.

We have a simple method of determining the specific gravity of this gas from the data established in the preceding part of this paper. When sulphur is heated in hydrogen gas, that gas is gradually converted into sulphuretted hydrogen gas without any alteration in its volume. It follows from this that sulphuretted hydrogen gas is a compound of

1 volume hydrogen gas	} condensed into one volume.
1 volume vapour of sulphur	

Consequently the specific gravity of this gas is equal to the specific gravity of hydrogen gas + the specific gravity of vapour of sulphur.

Sp. gr. of hydrogen gas	0·0694
vapour of sulphur	1·1111
	<hr/>
	1·1805

* On Phlogiston, p. 3.

† Ann. de Chim. xxxii. 267.

‡ Recherches Physico-Chim. i. 191.

Thus we learn that the true specific gravity of sulphuretted hydrogen gas is 1.1805. Our determination differs from this only by $\frac{1}{1106.39}$ —a difference certainly within the limits of unavoidable error from experiment. It, therefore, serves to confirm and establish the accuracy of the calculated specific gravity.

A volume of sulphur vapour is obviously equivalent to an atom. As sulphuretted hydrogen is composed of one volume sulphur vapour + one volume hydrogen gas, it follows that it is a compound of

1 atom sulphur	= 2.000
1 atom hydrogen	= 0.125
	2.125

Hence the weight of an atom of it is 2.125. If we reckon the specific gravity of oxygen gas unity, then the specific gravity of sulphuretted hydrogen gas will be 1.0625. Now $1.0625 \times 2 = 2.125$; so that the atomic weight of this gas is just double its specific gravity. In this property, it agrees with almost all the gases which we have had occasion to mention in this essay.

The volume of the two constituents of this gas is reduced to one-half when they unite together. This shows the strong affinity which exists between them and the intimacy of the combination which takes place.

III. PHOSPHORUS.

None of the compounds of phosphorus and oxygen are gaseous; but it forms two gaseous combinations with hydrogen. These will be sufficient to enable us to determine with precision the weight of an atom of phosphorus, and to show that it agrees with carbon and sulphur in having an atom whose weight is a multiple of the atomic weight of hydrogen.

1. *Phosphuretted Hydrogen Gas.*—Though this gas had been noticed by Gengembre and Kirwan as early as the year 1783, it can scarcely be said to have been known as a chemical substance till I published a set of experiments on it in 1816.* I pointed out the method of procuring it in a state of purity, determined its properties, and ascertained its composition. This gas is procured in a state of purity by putting phosphuret of lime into pure water. Phosphuretted hydrogen gas immediately begins to be evolved, and continues to make its escape for more than 24 hours. It is best to receive it over mercury; for water, unless it be quite free from air, speedily alters its nature. The specific gravity of a quantity of pure gas obtained in this way in my laboratory was found to be as follows:

* *Annals of Philosophy*, viii. 87.

By first trial.	0.9040
By second trial	0.9025
Mean	<u>0.90325</u>

The only previous determination of the specific gravity of this gas, as far as I know, is the one which I gave in my paper On Phosphuretted Hydrogen already alluded to. I stated its specific gravity to be 0.9022. Our new determination differs only $\frac{1}{76}$ th part from the preceding one. This difference is but little when we take into consideration the peculiar difficulties which attend the taking of the specific gravity of this gas. Unless the flask be very completely exhausted indeed of common air, combustion takes place when the phosphuretted hydrogen gas is let into it. This combustion, by the deposition of phosphoric acid which it occasions, has a tendency to increase the specific gravity of the phosphuretted hydrogen gas beyond the truth. 0.9022 was not the average of the six results which I obtained from an equal number of experiments—it was the experiment which appeared to me the most successful.

But we have a method of determining the specific gravity of this gas which appears to me perfectly satisfactory. When phosphorus is heated in hydrogen gas, a portion of the hydrogen gas is changed into phosphuretted hydrogen, but no alteration takes place in the volume of the hydrogen. It follows from this that phosphuretted hydrogen is composed of

1 volume hydrogen gas	} condensed into one volume.
1 volume vapour of phosphorus	

This is the same thing as to say that it is a compound of one atom of hydrogen and one atom of phosphorus. Now we have found the atoms of all the other bodies examined; namely, azote, oxygen, chlorine, carbon, and sulphur, to be multiples of the weight of an atom of hydrogen. We have found likewise that the atomic weight of gaseous bodies is double their specific gravity, provided we reckon the specific gravity of oxygen unity. Let us apply these properties to the discovery of the weight of an atom of phosphorus, and of the specific gravity of phosphuretted hydrogen gas.

I found the sp. gr. of this gas	0.9022
The specific gravity of hydrogen is ..	<u>0.0694</u>
	0.8328

We see from this that phosphuretted hydrogen is composed of

Hydrogen	694 or 1
Phosphorus	8328 12

Therefore an atom of phosphorus is 12 times heavier than an atom of hydrogen. It is of course a multiple of 0.125; and $0.125 \times 12 = 1.5 =$ the weight of an atom of phosphorus.

The specific gravity of vapour of phosphorus = 0.8328. This, if we reckon the specific gravity of oxygen unity, will be reduced to 0.75, and $0.75 \times 2 = 1.5 =$ the weight of an atom of phosphorus. It is obvious from all this that the true specific gravity of phosphuretted hydrogen gas is 0.90277, and that it is composed of

Hydrogen.	0.06944
Phosphorus	0.83333
	0.90277

For these numbers answer exactly to the requisite conditions: $0.06944 \times 12 = 0.83333$; and $1.1111 : 1 :: 0.90277 : 0.75$.

Now 0.9022, my old determination, differs from this only $\frac{1}{1825}$ th part. The new determination 0.90325 differs only $\frac{1}{1910}$ th part. Both of these discordancies are considerably within the limits of unavoidable errors from experiment. If we were to take the average of the two sets of experiments, we should obtain for the specific gravity of phosphuretted hydrogen gas 0.902735. Now this number accords with the calculated specific gravity in its four first decimal places, and indeed differs from it only by $\frac{1}{1944}$ th part, a quantity quite inappreciable.

I think then that it is demonstrated that the specific gravity of phosphuretted hydrogen gas is 0.90277; and that an atom of phosphorus weighs 1.5.

2. *Bihydrouret of Phosphorus*.—This gas was first particularly examined by Sir H. Davy in 1812, though it had often been noticed before by chemists. He obtained it by heating crystallized phosphorous acid. It may be obtained likewise by exposing pure phosphuretted hydrogen to the direct rays of the sun. It was in this last way that the quantity of it necessary for trying its specific gravity was obtained in my laboratory. Only one experiment was made, on account of the difficulty of obtaining this gas in quantity. The specific gravity was found 0.9653.

From the experiments of Davy, and indeed from my own, there cannot be the least doubt that this gas is composed of

2 volumes hydrogen gas	}	condensed into one volume.
1 volume vapour of phosphorus		

Hence its specific gravity must be equal to twice the specific gravity of hydrogen gas + the specific gravity of vapour of phosphorus.

Twice the sp. gr. of hydrogen gas. =	0·1388
Sp. gr. of vapour of phosphorus .. =	0·8333
	0·9722

Thus we see that its true specific gravity is 0·9722. The difference between this quantity and the specific gravity which we found is about $\frac{1}{140}$ th part—a quantity which will not be considered as great for a first experiment. In all probability, the whole of the phosphuretted hydrogen gas had not been decomposed by the action of the solar rays. A mixture of about one-ninth of phosphuretted hydrogen gas would have given hydroguretted phosphorus the specific gravity which we found.

It is obvious that this gas is a compound of two atoms hydrogen and one atom phosphorus. Hence the weight of an integrant particle of it is 1·75; for

2 atoms hydrogen.	= 0·25
1 atom phosphorus.....	= 1·5
	1·75

Its specific gravity, supposing that of oxygen to be unity, will be 0·875, and $0·875 \times 2 = 1·75$. Thus we see that its atomic weight is just twice that of its specific gravity. In this respect, it agrees with the other gaseous bodies which we have examined in this essay.

IV. IODINE.

Iodine is easily converted into a vapour; but it would be difficult to determine the specific gravity of this vapour experimentally. There is no great difficulty, however, in demonstrating from the specific gravity of hydriodic acid gas, and its known composition what the specific gravity of vapour of iodine must be. The same gas puts it in our power to determine the weight of an atom of iodine with accuracy.

Hydriodic acid gas is obtained when we bring iodine and phosphorus in contact with each other. A great deal of heat is evolved, and the hydriodic acid gas is expelled in considerable quantity. It must be received over mercury, and its specific gravity must be taken as soon as the gas is collected, because mercury has the property of decomposing it when they are left in contact of each other. The following were the specific gravities of this gas as ascertained in my laboratory :

By first trial.....	4·3758
By second trial.....	4·3756
By third trial.	4·3756
	4·37566
Mean	4·37566

The only person who took the specific gravity of this gas before our experiments were made was M. Gay-Lussac. He found it 4.443. But I have not the least doubt that our experiments, from the great care employed in collecting the gas pure, are much nearer the truth than his.

It has been shown by Gay-Lussac that hydriodic acid gas is a compound of equal volumes of hydrogen gas and vapour of iodine united together without any diminution of volume. Let x = specific gravity of vapour of iodine : then

$$\frac{0.0694 + x}{2} = 4.37566$$

From this it is easy to deduce the specific gravity of vapour of iodine, or $x = 8.68188$.

It is obvious that hydriodic acid is a compound of one atom of hydrogen and one atom of iodine. To find the weight of an atom of iodine we have only to reduce its specific gravity to what it would be on the supposition that the specific gravity of oxygen is unity, and multiply that specific gravity by 2. The product will be the weight of the atom. The specific gravity of the vapour thus reduced becomes 7.8137, and $7.8137 \times 2 = 15.6274$; so that the weight of an atom of iodine deduced from the specific gravity of hydriodic acid, as determined in my laboratory, is 15.6274.

We have seen that it is a fundamental law in chemistry that all the atomic weights of bodies are multiples of the atom of hydrogen, or of 0.125. It is obvious that 15.6274 is not a multiple of 0.125; but it requires only a very slight alteration to become a multiple. We have only to change it into 15.625. I conclude, therefore, that the true weight of an atom of iodine is 15.625. The half of this number is 7.8125; and $1 : 1.1111 :: 7.8125 : 8.68053047 =$ specific gravity of vapour of iodine. The true specific gravity of hydriodic acid gas will be obtained by adding the specific gravity of hydrogen gas to this number, and dividing the sum by 2.

$$\begin{array}{r} 8.68053047 \\ 0.06944444 \\ \hline 2)8.74997492 \\ \hline 4.37498746 \end{array}$$

Thus the true specific gravity of hydriodic acid gas is 4.375. Now the specific gravity which we found differs only $\frac{1}{66\frac{1}{8}}$ th part from this calculated specific gravity. There cannot, therefore, be the least doubt that the true specific gravity of hydriodic acid is 4.375, and that the weight of an atom of iodine is 15.625. This is a multiple of the atom of hydrogen; for $0.125 \times 125 = 15.625$. Iodine, therefore, agrees with the other bodies exa-

mined in this essay in every respect. Its atom is double the specific gravity of its vapour, and it is a multiple of the atom of hydrogen.

V. FLUOBORIC ACID GAS.

This acid gas was discovered by Gay-Lussac and Thenard. The method of procuring it was much simplified by Dr. John Davy. He showed that in order to obtain it, we have only to mix together in a retort one part of finely pounded anhydrous boracic acid, two parts of pounded fluor spar, and 12 parts of sulphuric acid. When heat is applied to this mixture, fluoboric acid gas is extricated. It must be received over mercury. This was the method employed to procure the fluoboric acid gas examined in my laboratory. Its specific gravity was found as follows :

By first trial.	2·3694
By second trial	2·3694

The only person who examined the specific gravity of this gas before us was Dr. John Davy : he found it 2·3709. These two sets of experiments nearly agree ; the difference between them being only $\frac{1}{330}$ th part. The nature of the constituents of this gas is still too imperfectly known to enable us to correct our experimental results by calculation. But I thought it worth while to record the result of our trials in this essay. They were carefully made, and the time will come when they will have their utility.

In the preceding essay, I think I have established the true specific gravities of 20 gases, deduced from them the real atomic weights of eight simple bodies, and established two very important general laws. 1. That the atomic weights of the seven other bodies are multiples of the atom of hydrogen ; and, 2. that the weight of an atom of a gas is twice its specific gravity (or, in some rare instances, four times its specific gravity) reckoning the specific gravity of oxygen gas unity.

It will probably be agreeable to the reader to see the true specific gravities of these gases, and the specific gravities of them which we determined by experiment. It will enable him to judge of the degree of accuracy to which our experiments were carried.

Gases.	True specific gravities, air being unity.	Specific grav. from experiment.
Hydrogen	0·0694	0·0694
Oxygen.	1·1111	1·1117
Azote	0·9722	0·97286
Chlorine ..	2·5000	2·5000
Vapour of iodine .	8·6805	8·68188
Vapour of carbon.	0·4166	0·41503

Gases.	True specific gravities, air being unity.	Specific grav. from experiment.
Vapour of sulphur	1·1111	1·11046
Vapour of phosphorus	0·8333	0·83339
Protoxide of azote	1·5277	1·5269
Deutoxide of azote	1·0416	1·04096
Muriatic acid gas	1·28472	1·28436
Hydriodic acid gas	4·3750	4·37566
Protoxide of chlorine	2·4444	2·4015
Ammoniacal gas	0·59027	0·5931
Carbonic acid	1·5277	1·5266
Carbonic oxide	0·9722	0·9694
Chlorocarbonic acid	3·4722	3·4604
Olefiant gas	0·9722	0·9709
Carburetted hydrogen	0·5555	0·555
Cyanogen	1·8055	1·80395
Sulphurous acid	2·2222	2·2216
Sulphuretted hydrogen	1·1805	1·17906
Phosphuretted hydrogen	0·90277	0·902735
Bihydroguret of phosphorus	0·9722	0·9653
Fluoboric acid	—	2·3694

The following table exhibits the weight of the atom of each of these bodies :

Hydrogen	0·125
Oxygen	1·000
Azote	1·75
Chlorine	4·5
Iodine	15·625
Carbon	0·75
Sulphur	2·0
Phosphorus	1·5
Protoxide of azote	2·75
Deutoxide of azote	3·75
Muriatic acid	4·625
Hydriodic acid	15·75
Protoxide of chlorine	5·5
Ammonia	2·125
Carbonic acid	2·75
Carbonic oxide	1·76
Chlorocarbonic acid	6·25
Olefiant gas	1·76
Carburetted hydrogen	1·00
Cyanogen	3·25
Sulphurous acid	4·000

Sulphuretted hydrogen	2.125
Phosphuretted hydrogen	1.625
Bihydroguret of phosphorus	1.75

It is obvious from the preceding tables that there are four gases which have the same specific gravity and the same atomic weight. These are

	Sp. gr.	Weight of atoms.
Azote.	0.9722	1.75
Carbonic oxide.	0.9722	1.75
Olefiant gas	0.9722	1.75
Bihydroguret of phosphorus	0.9722	1.75

The specific gravity of carburetted hydrogen gas is half that of oxygen gas, and the weight of its atom the same as that of oxygen. The specific gravity of vapour of sulphur is the same as that of oxygen gas, and the weight of its atom double that of oxygen. The specific gravities and atomic weights of protoxide of azote and carbonic acid gases are the same.

ARTICLE II.

A Mode suggested for passing Calculi from the Bladder by the Aid of Atmospheric Pressure.

(To Dr. Thomson.)

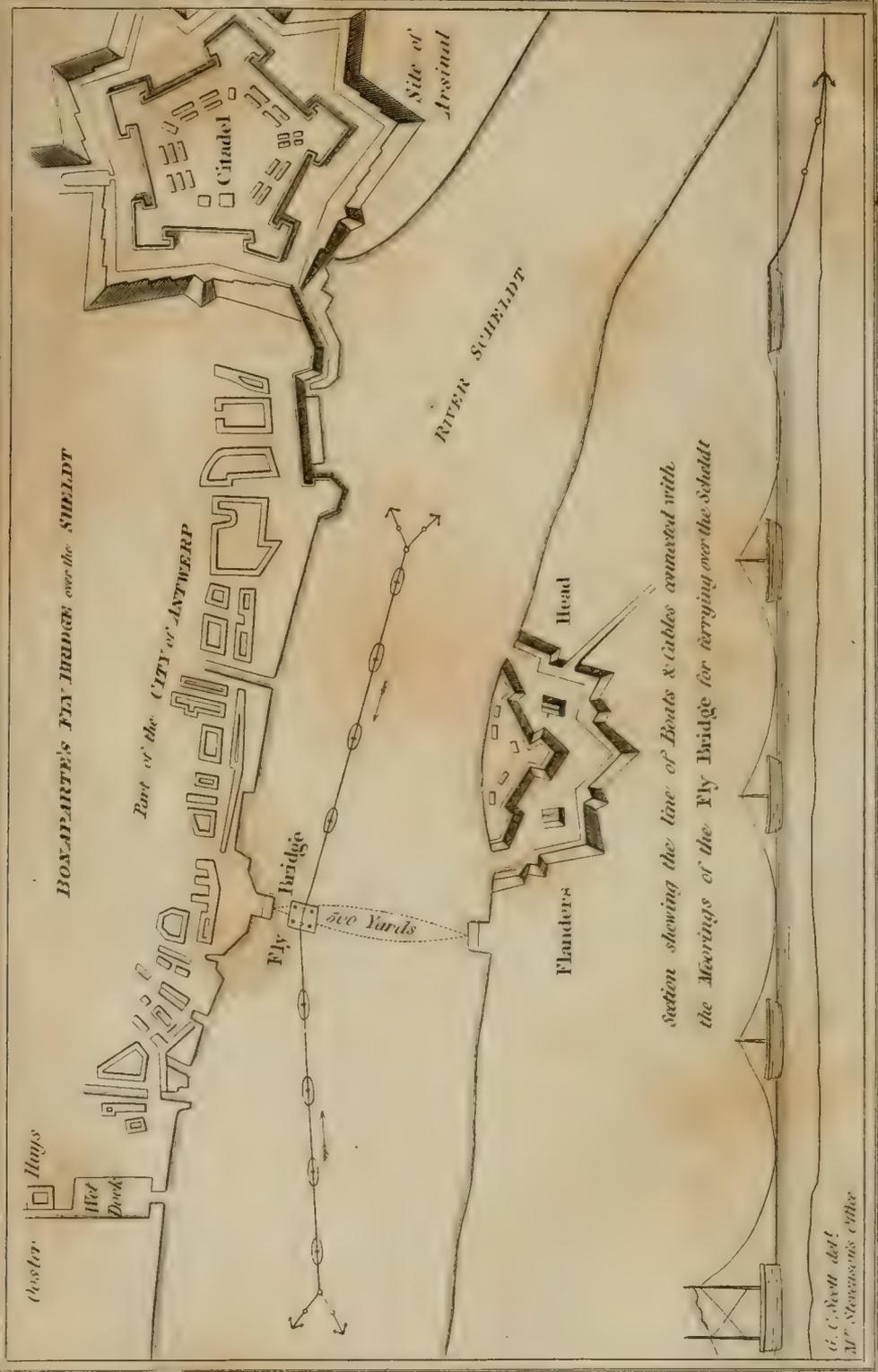
SIR,

Stockgrove, Aug. 3, 1820.

THE writer of this has lately seen in one of the public journals the description of a new instrument called by its inventor* a Dilator, by which any part of the urethra may be dilated without affecting other parts of it by means of an elastic substance charged with air, for the removal of stricture, and to facilitate the passage of calculi from the bladder. Now whatever may be the merit of the instrument itself, or whatever chance there may be (and of this there is reasonable hope) of its leading to new and useful expedients, the inventor has at least the merit of directing the public attention to mechanic remedy in this class of complaints. Chemistry has done much of late towards pointing out the true principle on which alone medical treatment can be of any use. Let mechanic science also by the invention of new, or by the improvement of old instruments assist the progress of relief. The object of this paper is to suggest the plan of another instrument for the removal of the calculus, after the dilator has prepared a passage for it, of sufficient power, it is presumed, to draw away any calculi which the dilated calibre of

* Mr. Arnott, of Bedford-square, London.





G. C. Swell del.
Mr. Stevenson's engr.

the urethra may allow to pass, and which are not impeded by adhesion. I propose to effect this by means of a tube, or canula, so constructed as to allow an exhausted receiver of a portable size to be fixed to it, having its stop-cock to regulate the pressure; through this canula the calculus may be searched for, and when in contact with it, and the power of the vacuum applied to it, the powerful suction so excited would, it is presumed, be sufficient to draw the attached calculus forward as far as the previous enlargement would allow; and, according to the size of the stone, either draw it quite out, or at least so far on in the passage as to be within the reach of a simple operation.* As the end of the proposed instrument must accomodate itself to all surfaces, rough as well as smooth, the having it so formed as best to answer the purpose will be a great desideratum: the body of the canula ought probably to be metallic, but its end or lower termination may be formed of the elastic gum, or some substance of that nature, and in shape and structure perhaps imitative of some processes to be found in the animal kingdom where suction is produced by muscular action.

A surgeon of eminence in London some time since succeeded in removing a small calculus (said to be of the size of a small walnut) from the bladder of a gentleman, by the following method: he made an opening from the perineum into the membranous part of the urethra, and after enlarging the passage with the dilator, he reached and extracted the stone with the common stone forceps. The wound soon healed, nor did the dilatation occasion much pain or other inconvenience. Besides the case of regularly formed calculi, might not the instrument now proposed be used also in removing loose granular concretions lodged in the bladder? In some cases, might it not with advantage be substituted for the forceps, especially when they are to act through a long and narrow passage? Might not an instrument formed on this principle be used for drawing away aqueous fluid, or pus, situated in parts remote from the surface?

PHILOTECNOUS.

ARTICLE III.

Letter from Robert Stevenson, Civil Engineer, to Prof. Thomson, &c. &c. relative to Bonaparte's Fly Bridge on the Scheldt at Antwerp. (With a Plate.)

I TAKE this opportunity of communicating some observations from my *note book*, when lately at Antwerp, on a tour through

* It seems possible, and with much advantage, if it could be done, to join together the two instruments; in that case when in use, the attached calculus would follow so closely in the wake of the bulb of the dilator as to pass before the dilated parts could close upon it, and a less force be sufficient to move it along.

Holland and Flanders, relative to a convenient mode of crossing navigable rivers, used in several parts of the continent with much facility, especially, during the late war, by Bonaparte's troops in crossing the Scheldt; a mode which in my humble opinion is also most suitable for several of the principal rivers and great cattle ferries of this country.

The Scheldt at Antwerp is about 500 or 600 yards in breadth, being more than the broadest parts of the Thames above Gravesend; and you are to understand that this part of the Scheldt being also navigable, the free intercourse of shipping must not be obstructed. The banks of this great river upon each side are too low for a fixed bridge upon any economical mode of construction, although it were found practicable in other respects. But as the ardent mind and urgent measures of the *ci-devant* Emperor could not submit to the tediousness and uncertainty of the common ferry boat, he caused a very large flat, or vessel, to be moored at Antwerp, constructed in such a manner that the impulse of the current acting on the rudder caused the vessel to cross the stream in an oblique direction in the course of a few minutes by a very simple and obvious principle. This mode has likewise been used with much effect on some of the principal rivers in Germany and Italy, but has not, so far as I know, been introduced in this country.

As shown by Plate CVIII, representing a part of the river Scheldt, a wooden pier or platform is projected across the muddy beach to a point beyond the low water-mark on each side of the river. Connected with the outer extremity of these piers, the *hulk* of a decked vessel is attached, and forms a kind of floating pier, or platform, projecting across the mud, so constructed that the vessel, called the *Fly Bridge*, and the pier, rise and fall with the perpendicular motion of the tidal or speat waters of the river; and the deck or platform of the bridge, and the outer extremity of the pier, are thus always kept upon the same level for the conveniency of passengers embarking and debarking; and that the *Fly Bridge* may lie with safety alongside of the floating piers, the bridge is furnished with a *folding-leaf* on each side, which forms a roadway of communication between it and the pier.

Having thus prepared the means for passengers, carriages, and cattle, communicating on board with ease and facility, I observed that the part of this apparatus termed the bridge consists of a large deck or platform of timber forming an area upwards of 60 square feet laid upon two boats, or vessels, after the manner of the *Dalswinton Steam Boat*, described in the *Annals of Philosophy*, vol. xiii. p. 279. The mooring of such a *float* as this, which would be unnecessarily large for the ordinary purposes of a ferry, is one of the most important and essential parts of this contrivance, and is accomplished in the following very simple and effectual manner: An anchor, or anchors, are laid down in

the middle of the river at a suitable distance up and down the stream, from the intended track of the Fly Bridge across the river, to admit of hawsers being stretched of sufficient length to allow the bridge to pass from bank to bank to the floating piers. To these anchors placed up and down the river, as shown in the Plate, two decked boats are made fast with a piece of chain cable. One end of the mooring hawser being made fast to one of these boats, the other end, having what seamen term a *spring* upon it for more readily *shearing* the Fly Bridge, is made fast to one of the masts of the Fly Bridge, as will be seen in the section of the range of boats and bridge delineated on the Plate. To give a Fly Bridge a sufficient scope of cable to cross a ferry of 500 yards in width, her mooring hawser, including both up and down the river, must be about 1200 yards in length, which, in situations like the Scheldt, would form such a curve or *catenarian* as would infallibly touch the bottom of the river, and prevent the Fly Bridge from crossing. To guard against this, however, the hawser is supported, and borne off the ground, upon three decked boats intermediate between the Fly Bridge, and each of the boats made fast to the anchors by chain cables, which prevent this long hawser from sweeping or touching the ground in its passage across the river, as will be better understood by examining the section of the range of boats before alluded to. The Fly Bridge, with its connecting boats, are, therefore, made to describe circles proportionate to their distance respectively from the mooring anchors, the operation of the whole being similar, if we may be allowed the comparison, to points fixed upon the moveable bar of the mariner's quadrant, in which the position of the mooring anchors may be supposed to occupy the joint of the quadrant bar, while the Fly Bridge traverses the track of the Nonius. When, therefore, this vessel, or Fly Bridge, is to cross the river, the *lashings* are let go with which it is made fast to the floating pier on either side, and the rudder is so set that the current acts upon it, and the boat immediately performs her passage, and *swings* across the river to the floating pier on the opposite side to which the length of the hawser is adjusted, and a passage is thus effected within the course of a few minutes with the greatest promptitude and precision, the rapidity of the passage being always in proportion to the strength of the current. When the tide changes from flood to ebb, and *vice versâ*, the Fly Bridge rides or swings from the anchors in the direction of the current so as to perform her passage during both the tides of ebb and flood with equal facility, as will be seen by examining the Plate.

It is said that Bonaparte established this Fly Bridge, or ferry apparatus, for the more speedy and certain transportation of his troops across the Scheldt when his army was in Flanders, and its capacity and extensive accommodations were such that it also answered the purpose of a barrack, or halting place having

ample conveniency for carrying 500 men with their park of artillery, ammunition, baggage, and horses, at one operation. There were also cooking places on the deck of this vessel, and large rooms below; and when the troops were making forced marches, a courier was dispatched to order preparations on board of the Fly Bridge, which, on these occasions, generally became one of the stages for refreshment.

Among the many ingenious and bold designs which have been formed for crossing the rivers and ferries of Great Britain where a bridge of the ordinary construction is not applicable, this seems particularly to deserve the notice and attention of the *county gentlemen* as road trustees. Here I might instance many of our navigable rivers where the introduction of such a contrivance would be of the greatest importance to large districts of country, where a facility in crossing extensive streams is most zealously inquired after, even at the expenditure of very large sums of money, as, for example, at certain situations communicating with the Murray Frith, the friths of Forth and Tay, the rivers Tweed, Humber, Thames, Mersey, Clyde, &c. &c. besides many places in Ireland.

The Fly Bridge, or boat apparatus, which I saw at Antwerp, was of very large dimensions, and extremely rude in its construction. It will naturally be inquired by those who are interested in the navigation of rivers, whether this apparatus materially impedes the shipping, to which it is answered that much will depend upon the breadth of the river, and the number and dimensions of the ships frequenting it; but upon the whole, this apparatus does not present a greater obstacle to shipping than the common occurrence of one vessel under sail meeting with another in *beating to windward*, or lying at anchor, circumstances which are unavoidable upon navigable rivers. With regard to the expense of such an apparatus, calculating in a general way, for a river or ferry of 500 yards in breadth, where the depth of water does not exceed four or five fathoms, the expense may be as follows:

Four mooring anchors and two pieces of chain cable for attaching the decked boats to them	£	s.
	400	0
A floating pier on both sides of the river.	450	0
Eight decked boats attached to the chain, cables, and mooring hawsers.	800	0
The price of 1200 fathoms of mooring hawser, eight inches in circumference, for swinging the Fly Bridge both with flood and ebb tide.	500	0
The price of a <i>Dalswinton Boat</i> with a platform or deck containing an area of 30 square yards, with her appurtenances.	1000	0
Contingencies on £ taken at 15 per cent.	472	10

3,622 10

The above estimate amounting to the sum of 3,622*l.* 10*s.* is of course to be considered as a very general statement, for every estimate of this kind must vary according to the position of things. This, however, appears but small, compared with what would be necessary for the erection of a bridge either of iron or stone of 500 yards in length; while such a mode of crossing a large river is not only expeditious, but is safe and commodious. Horses and carriages can also be conveyed by this means without unyoking, and passengers may either remain upon deck, or retire into cabins below. For cattle ferries this would be an improvement of much consequence, as almost any number of cattle may be carried across at one operation.

But establishments of this kind may be required upon a still larger scale than is necessary for crossing the Scheldt, the Rhine, and the Po; and as it is believed that such an idea has not been put in practice in this country, it will be an easy matter to make the trial on any river, or ferry, at a small expense, it being only necessary to apply the ordinary tackle and materials of shipping for making the experiment.

I am, dear Sir, yours truly,

ROBERT STEVENSON.

ARTICLE IV.

*Experiments on the Effect of a Current of Electricity on the Magnetic Needle.** By John Christian Oersted, Knight of the Order of Danneborg, Professor of Natural Philosophy, and Secretary to the Royal Society of Copenhagen.

THE first experiments respecting the subject which I mean at present to explain, were made by me last winter, while lecturing on electricity, galvanism, and magnetism, in the University. It seemed demonstrated by these experiments that the magnetic needle was moved from its position by the galvanic apparatus, but that the galvanic circle must be complete, and not open, which last method was tried in vain some years ago by very celebrated philosophers. But as these experiments were made with a feeble apparatus, and were not, therefore, sufficiently conclusive, considering the importance of the subject, I associated myself with my friend Esmarck to repeat and extend them by means of a very powerful galvanic battery, provided by us in common. Mr. Wleugel, a Knight of the Order of Danneborg, and at the head of the Pilots, was present at, and assisted in, the experiments. There were present likewise Mr. Hauch, a man very well skilled in the Natural Sciences, Mr. Reinhardt,

* Translated from a printed account drawn up in Latin by the author, and transmitted by him to the Editor of the *Annals of Philosophy*.

Professor of Natural History, Mr. Jacobsen, Professor of Medicine, and that very skilful chemist, Mr. Zeise, Doctor of Philosophy. I had often made experiments by myself; but every fact which I had observed was repeated in the presence of these gentlemen.

The galvanic apparatus which we employed consists of 20 copper troughs, the length and height of each of which was 12 inches; but the breadth scarcely exceeded $2\frac{1}{2}$ inches. Every trough is supplied with two plates of copper, so bent that they could carry a copper rod, which supports the zinc plate in the water of the next trough. The water of the troughs contained $\frac{1}{10}$ th of its weight of sulphuric acid, and an equal quantity of nitric acid. The portion of each zinc plate sunk in the water is a square whose side is about 10 inches in length. A smaller apparatus will answer provided it be strong enough to heat a metallic wire red hot.

The opposite ends of the galvanic battery were joined by a metallic wire, which, for shortness sake, we shall call the *uniting conductor*, or the *uniting wire*. To the effect which takes place in this conductor and in the surrounding space, we shall give the name of the *conflict of electricity*.

Let the straight part of this wire be placed horizontally above the magnetic needle, properly suspended, and parallel to it. If necessary, the uniting wire is bent so as to assume a proper position for the experiment. Things being in this state, the needle will be moved, and the end of it next the negative side of the battery will go westward.

If the distance of the uniting wire does not exceed three-quarters of an inch from the needle, the declination of the needle makes an angle of about 45° . If the distance is increased, the angle diminishes proportionally. The declination likewise varies with the power of the battery.

The uniting wire may change its place, either towards the east or west, provided it continue parallel to the needle, without any other change of the effect than in respect to its quantity. Hence the effect cannot be ascribed to attraction; for the same pole of the magnetic needle, which approaches the uniting wire, while placed on its east side, ought to recede from it when on the west side, if these declinations depended on attractions and repulsions. The uniting conductor may consist of several wires, or metallic ribbons, connected together. The nature of the metal does not alter the effect, but merely the quantity. Wires of platinum, gold, silver, brass, iron, ribbons of lead and tin, a mass of mercury, were employed with equal success. The conductor does not lose its effect, though interrupted by water, unless the interruption amounts to several inches in length.

The effect of the uniting wire passes to the needle through glass, metals, wood, water, resin, stoneware, stones; for it is not taken away by interposing plates of glass, metal or wood.

Even glass, metal, and wood, interposed at once, do not destroy, and indeed scarcely diminish the effect. The disc of the electrophorus, plates of porphyry, a stone-ware vessel, even filled with water, were interposed with the same result. We found the effects unchanged when the needle was included in a brass box filled with water. It is needless to observe that the transmission of effects through all these matters has never before been observed in electricity and galvanism. The effects, therefore, which take place in the conflict of electricity are very different from the effects of either of the electricities.

If the uniting wire be placed in a horizontal plane under the magnetic needle, all the effects are the same as when it is above the needle, only they are in an opposite direction; for the pole of the magnetic needle next the negative end of the battery declines to the east.

That these facts may be the more easily retained, we may use this formula—the pole *above* which the *negative* electricity enters is turned to the *west*; *under* which, to the *east*.

If the uniting wire is so turned in a horizontal plane as to form a gradually increasing angle with the magnetic meridian, the declination of the needle *increases*, if the motion of the wire is towards the place of the disturbed needle; but it *diminishes* if the wire moves further from that place.

When the uniting wire is situated in the same horizontal plane in which the needle moves by means of the counterpoise, and parallel to it, no declination is produced either to the east or west; but an *inclination* takes place, so that the pole, next which the negative electricity enters the wire, is *depressed* when the wire is situated on the *west* side, and *elevated* when situated on the *east* side.

If the uniting wire be placed perpendicularly to the plane of the magnetic meridian, whether above or below it, the needle remains at rest, unless it be very near the pole; in that case the pole is *elevated* when the entrance is from the *west* side of the wire, and *depressed*, when from the *east* side.

When the uniting wire is placed perpendicularly opposite to the pole of the magnetic needle, and the upper extremity of the wire receives the negative electricity, the pole is moved towards the east; but when the wire is opposite to a point between the pole and the middle of the needle, the pole is most towards the west. When the upper end of the wire receives positive electricity, the phenomena are reversed.

If the uniting wire is bent so as to form two legs parallel to each other, it repels or attracts the magnetic poles according to the different conditions of the case. Suppose the wire placed opposite to either pole of the needle, so that the plane of the parallel legs is perpendicular to the magnetic meridian, and let the eastern leg be united with the negative end, the western leg with the positive end of the battery: in that case the nearest

pole will be repelled either to the east or west, according to the position of the plane of the legs. The eastmost leg being united with the positive, and the westmost with the negative side of the battery, the nearest pole will be attracted. When the plane of the legs is placed perpendicular to the place between the pole and the middle of the needle, the same effects recur, but reversed.

A brass needle, suspended like a magnetic needle, is not moved by the effect of the uniting wire. Likewise needles of glass and of gum lac remain unacted on.

We may now make a few observations towards explaining these phenomena.

The electric conflict acts only on the magnetic particles of matter. All non-magnetic bodies appear penetrable by the electric conflict, while magnetic bodies, or rather their magnetic particles, resist the passage of this conflict. Hence they can be moved by the impetus of the contending powers.

It is sufficiently evident from the preceding facts that the electric conflict is not confined to the conductor, but dispersed pretty widely in the circumjacent space.

From the preceding facts we may likewise collect that this conflict performs circles; for without this condition, it seems impossible that the one part of the uniting wire, when placed below the magnetic pole, should drive it towards the east, and when placed above it towards the west; for it is the nature of a circle that the motions in opposite parts should have an opposite direction. Besides, a motion in circles, joined with a progressive motion, according to the length of the conductor, ought to form a conchoidal or spiral line; but this, unless I am mistaken, contributes nothing to explain the phenomena hitherto observed.

All the effects on the north pole above-mentioned are easily understood by supposing that negative electricity moves in a spiral line bent towards the right, and propels the north pole, but does not act on the south pole. The effects on the south pole are explained in a similar manner, if we ascribe to positive electricity a contrary motion and power of acting on the south pole, but not upon the north. The agreement of this law with nature will be better seen by a repetition of the experiments than by a long explanation. The mode of judging of the experiments will be much facilitated if the course of the electricities in the uniting wire be pointed out by marks or figures.

I shall merely add to the above that I have demonstrated in a book published five years ago that heat and light consist of the conflict of the electricities. From the observations now stated, we may conclude that a circular motion likewise occurs in these effects. This I think will contribute very much to illustrate the phenomena to which the appellation of polarization of light has been given.

ARTICLE V.

Observations on the late Solar Eclipse. By Col. Beaufoy, F.R.S.

(To Dr. Thomson.)

MY DEAR SIR,

Bushey Heath, Stanmore, Sept. 8, 1820.

I HAVE the pleasure to send you my observations on the late solar eclipse; and as they were made under very favourable circumstances, the beginning and end may be relied on within a second of time. The period of the greatest obscuration cannot pretend to the same accuracy. It was ascertained by measuring the distance of the lunar horns with an eight inch Hadley's sextant made by Stancliff, divided to 15 seconds, and mounted on a stand. The magnifying power of the sextant's telescope was 12.

I remain, my dear Sir, yours very sincerely,

MARK BEAUFOY.

Latitude $51^{\circ} 37' 44.3''$ N.; Long. in time, West, $1^{\circ} 20.93''$
Sept. 7, 1820.

Solar Eclipse	{	Beginning.	$0^h 22' 57''$	} Apparent time at Bushey.
		Greatest obscuration.	$1 \ 52 \ 45$	
		End.	$3 \ 14 \ 57$	

Meteorological Table.

Hour.	Barom.	Therm.	Hygrom.	Ther. in sun.	Wind.
$0^h \ 10$	29.655	63	52	79	S by E
2 00	29.659	61	55	64	SSW
3 20	29.655	64	53	73	SSW

Remarks.

The limbs of the sun and moon were extremely well defined from the commencement to the end; the horns of the moon were sharp, and terminated in a point. No occurrence took place that denoted the appulse of the moon's limb towards the sun's disc. At the beginning, the velocity of the wind was about ten miles per hour; it died away at the greatest obscuration, and afterwards freshened. The moon's edge was serrated, with one protuberance larger than the rest, on her apparent upper limb. No spots were visible on the sun. The magnetic needle was not affected by any tremulous or libratory motion, but the morning and noon variation were less than during the preceding days of the month. A sensation of cold was experienced at the middle of the eclipse. A convex lens, four inches in diameter, and 11 inches focal length, was fixed at one extremity of a frame that had both a horizontal and vertical motion, and consequently

readily turned towards the sun. At the distance of the focal length was placed a vertical circle of pasteboard moving on an axis, and divided into white, grey, and black compartments. Soon after the commencement of the eclipse, the lens was exposed to the sun's rays, and from a mean of several trials, the white part of the pasteboard was burned through in 11 seconds; at the time of the greatest obscuration, the sun's rays were so much weakened, that no effect was produced on either the white, grey, or black paper. On the termination of the eclipse, the white was perforated in 24 seconds; the difference between the first and third experiments is partly to be attributed to the decrease of the sun's altitude, and partly to the interposition of a thin cloud. Clouds prevented Venus being seen at this place. No dew was perceptible during the eclipse; and the diminution of light was not so great as expected, it far exceeding the brightest moonlight.

The telescope used was a five feet acromatic made by Dolland, and the magnifying power 80.

ARTICLE VI.

On the Climate of New South Wales. By Mr. Trimmer.

(To Dr. Thomson.)

DEAR SIR,

Brentford, Aug. 12, 1820.

THE New South Wales Almanack for 1820 having lately come into my hands, I have extracted from it an account of the usual state of the weather at Sydney, thinking that, perhaps, you might deem it sufficiently interesting to insert it in the *Annals of Philosophy*. I am, dear Sir, your obliged humble servant,

JAMES TRIMMER.

Jan.

1. Frequent vivid lightning.
4. Dry squalls, creating clouds of dust.
10. Heat frequently intense, and but for nocturnal dews the fields and gardens would be literally scorched up.
17. The harvesting is mostly over.

Thermometer exceeds 62° at sunrise, and at noon up to 80° , 90° , and sometimes 100° , and upwards.

Feb.

1. Mornings and evenings very agreeable.
6. Light showers to be expected.
10. Days continue warm, and sometimes sultry; lightning not unfrequent.

Feb.

15. Weather changeable from much heat to sensibly cool.

Thermometer the first few days 75° , and even 80° , at noon ; but falls towards the end of the month.

March.

1. Rains frequent and heavy.

5. Mornings and evenings very chilly ; nights sensibly cold.

11. The days, when fair, are pleasant.

15. Rains continue.

17. Beware of floods.

Thermometer changes much in the same day, being sometimes at 62° , and not unfrequently up to 80° of heat.

April.

1. The weather begins to be more settled, but now and then showery.

8. Days get warmer, and nights less chilly ; as the month advances, the heat of the day abates, and a pleasant season ensues.

Thermometer varies throughout the month ; the first part very warm, and at the end of the month pleasant : first part up to 74° , and above, and afterwards declines to between 66° and 63° .

May.

1. Mostly clear, and days pleasant ; nights become rather chilly, and mornings and evenings very agreeable.

11. In general little, if any, rain ; though in 1813 was an exception, as it rained hard all the month.

Thermometer at the beginning of the month 54° , and above, at sunrise ; but declines several degrees towards the end.

June.

1. About as cold as the middle of November in and about London, but clear.

7. Thin ice in the morning in exposed situations ; and fogs in wet and marshy places, rising from the river Hawkesbury, like intense columns of smoke.

15. Days not unpleasantly cold, but nights severe ; often cloudy, but little rain.

Thermometer at sunrise down to 46° and 50° ; at noon 54° to 60° ; at four afternoon, 60° to 66° ; at four morning, 44° to 48° .

July.

1. Mornings and evenings very chilly ; nights severely cold.

6. Little rain ; intense fogs in damp places ; hoar frosts bespangle the verdure of the field as the sun advances in his early course.

Thermometer, 45° at daylight, and 67° at and after noon.

August.

1. Heavy fogs in all damp places. Hawkesbury involved in fogs till nearly mid-day.
10. Droughts mostly prevalent.
Days agreeable, nights still cold.
The fields afford scarcely enough to keep the flocks and herds alive.
Slight showers very acceptable.
Thermometer at noon sometimes down to 54° , and sometimes up to 63° and 64° .

Sept.

1. Mostly fair.
6. Droughts decline, and showers renovate the country.
10. Days pleasantly warm, with now and then a cold wind, accompanied with refreshing showers; nights not unpleasantly cool.
20. A mild and agreeable season.
Thermometer sometimes at 50° in the morning, and 66° to 70° at noon.

Oct.

- Pleasant the whole of the month; the middle of the day sometimes inclining to heat.
- The morning and evening air very salubrious; but towards the end of the month, the orchard liable to suffer from hot, blighting winds; if droughty, this season the grasses languish, but occasional showers are pleasing and profitable.
- Thermometer at daylight 56° to 60° ; at noon 61° to 71° ; four afternoon, 60° to 70° .

Nov.

- Hot throughout the month; frequent storms with an unclouded atmosphere and vivid lightning.
12. Thunder-storms, with short, heavy showers.
 15. Heat becomes inclement throughout the day; and in evening hot winds.
Thermometer at sunrise 51° to 64° ; noon 66° to 80° ; at four in afternoon 67° to 80° , and upwards.

Dec.

1. Heat increases, but blighting winds less prevalent.
6. Fruits protected from the N.W. will thrive best.
This and January are the hottest months; but thanks to the Great Protecting Power, no distempers attend the inclemency of the weather.
Thermometer at sunrise 64° to 70° ; noon 72° to 90° ; some times to 100° .

Spring.—September, October, and November.

Summer.—December, January, and February.

Autumn.—March, April, and May.

Winter.—June, July, and August.

ARTICLE VII.

On the Direct Method of Finite Differences.

By Mr. James Adams.

(To Dr. Thomson.)

SIR,

Stonehouse, near Plymouth, June 30, 1820.

IF, in your opinion, the following problems and examples relative to the *direct method of finite differences*, are likely to be of service to the young analyst, your inserting them in the *Annals of Philosophy* will much oblige,

Your obedient servant,

JAMES ADAMS.

Problem 1.—To find the n th order of the function ϕ .

$$\begin{aligned} \phi_n &= \phi + n \Delta \phi + \frac{n(n-1)}{2} \Delta^2 \phi + \frac{n(n-1)(n-2)}{2 \cdot 3} \Delta^3 \phi + \&c. \\ &= (1 + \Delta)^n \phi. \end{aligned}$$

—(See *Annals of Philosophy* for Feb. 1820.)

Corollary 1.—By omitting the terms affected with $\Delta^2 \phi$, $\Delta^3 \phi$, &c. or supposing $\Delta \phi$ constant, we have $\phi_n = \phi + n \Delta \phi$.

Corollary 2.—Since $\phi_1 = (1 + \Delta) \phi$, $\phi_2 = (1 + \Delta)^2 \phi$, $\phi_n = (1 + \Delta)^n \phi$. It plainly appears that the result in the problem is obtained by operating on the *symbol only*.

Problem 2.—To find the n th increment of the function ϕ .

$$\begin{aligned} \Delta^n \phi &= \phi_n - n \phi_{n-1} + \frac{n(n-1)}{2} \phi_{n-2} - \frac{n(n-1)(n-2)}{2 \cdot 3} \phi_{n-3} \dots \\ &\pm \phi = (\phi_1 - 1)^n, \text{ writing } \phi \text{ for unity in the expansion of} \\ &(\phi_1 - 1)^n. \end{aligned}$$

—(Ibid.)

Corollary.—By Dr. Taylor's theorem, and detaching the symbol of operation, we have $\phi_n = \phi e^{n^d}$, $\phi_{n-1} = \phi e^{(n-1)^d}$, $\phi_{n-2} = \phi e^{(n-2)^d}$, &c. then by substitution we get $\Delta^n \phi = \phi$

$$\left(e^{n^d} - n e^{(n-1)^d} + \frac{n(n-1)}{2} e^{(n-2)^d} - \&c. \right) = \phi \left(e^d - 1 \right)^n.$$

$$\text{Or, } \Delta^n \phi = e^{n^d} \phi - n e^{(n-1)^d} \phi + \frac{n(n-1)}{2} e^{(n-2)^d} \phi - \&c. e =$$

2.718, &c. and d denoting differential.

Example 1.—To find the n th increment of x^n .

Put $\phi = x^n$ and $\Delta x = w$ a constant quantity; then will

$$\phi_1 = (x + w)^m = \left(1 + \frac{w}{x}\right)^m \cdot x^m; \phi^2 = (x + 2w)^m = \left(1 + \frac{2w}{x}\right)^m \cdot x^m; \phi_n = (x + nw)^m = \left(1 + \frac{nw}{x}\right)^m \cdot x^m, \&c.$$

Then by substituting in problem 2, we have

$$\Delta^n (x^m) = \left(1 + \frac{nw}{x}\right)^m \cdot x^m - n \left(1 + \frac{(n-1)w}{x}\right)^m \cdot x^m + \frac{n(n-1)}{2} \left(1 + \frac{(n-2)w}{x}\right)^m \cdot x^m \cdot \&c. \pm x^m.$$

$$\text{Or, } \Delta^n (x^m) = x^m \left\{ \left(1 + \frac{nw}{x}\right)^m - n \left(1 + \frac{(n-1)w}{x}\right)^m + \frac{n(n-1)}{2} \left(1 + \frac{(n-2)w}{x}\right)^m - \&c. \dots \pm 1 \right\}$$

Example 2.—To find the n th increment of $\frac{1}{x^m}$.

Put $\phi = \frac{1}{x^m}$ and $\Delta x = w$, a constant quantity, then will $\phi_1 = \frac{1}{(x+w)^m} = \frac{1}{\left(1 + \frac{w}{x}\right)^m \cdot x^m}$; $\phi_2 = \frac{1}{(x+2w)^m} = \frac{1}{\left(1 + \frac{2w}{x}\right)^m \cdot x^m}$,

$\phi_n = \frac{1}{(x+nw)^m} = \frac{1}{\left(1 + \frac{nw}{x}\right)^m \cdot x^m}$, &c. By substituting in problem 2, we have

$$\Delta^n \left(\frac{1}{x^m}\right) = \frac{1}{x^m} \left\{ \frac{1}{\left(1 + \frac{nw}{x}\right)^m} - \frac{n}{\left(1 + \frac{(n-1)w}{x}\right)^m} + \frac{n(n-1)}{2 \left(1 + \frac{(n-2)w}{x}\right)^m} - \&c. \dots \pm 1 \right\}$$

Example 3.—To find the n th increment of a^x .

Put $\phi = a^x$ and $\Delta x = w$, a constant quantity, then will $\phi_1 = a^{(x+w)}$, $\phi_2 = a^{(x+2w)}$, $\phi_n = a^{(x+nw)}$, &c. By substituting in problem 2, $\Delta^n (a^x) = a^x \left\{ a^{nw} - n a^{(n-1)w} + \frac{n(n-1)}{2} a^{(n-2)w} - \&c. \dots \pm 1 \right\}$

Example 4.—To find the n th increment of $\frac{1}{a^x}$.

Put $\phi = \frac{1}{a^x}$ and $\Delta x = w$, a constant quantity, then will $\phi_1 = \frac{1}{a^{(x+w)}}$, $\phi_2 = \frac{1}{a^{(x+2w)}}$, $\phi_n = \frac{1}{a^{(x+nw)}}$, &c. By substitution in problem 2, $\Delta^n \left(\frac{1}{a^x}\right) = \frac{1}{a^x} \left\{ \frac{1}{a^{nw}} - \frac{n}{a^{(n-1)w}} + \frac{n(n-1)}{2 \cdot a^{(n-2)w}} - \&c. \dots \pm 1 \right\}$

Example 5.—To find the n th increment of the log. x .

Put $\phi = lx$ and $\Delta x = w$, a constant quantity, then will

$\phi_1 = l(x + w) = l\left(1 + \frac{w}{x}\right)x$, $\phi_2 = l(x + 2w) = l\left(1 + \frac{2w}{x}\right)x$, $\phi_n = l(x + nw) = l\left(1 + \frac{nw}{x}\right)x$, &c. By substituting in problem 2, $\Delta^n(lx) = l(x + nw) - wl(x + (n-1)w) + \frac{n(n-1)}{2} \cdot l(x + (n-2)w) - \dots \pm lx$.

Or, $\Delta^n(lx) = l\left(1 + \frac{nw}{x}\right) - n \cdot l\left(1 + \frac{(n-1)w}{x}\right) + \frac{n(n-1)}{2} \cdot l\left(1 + \frac{(n-2)w}{x}\right) - \dots$

The latter expression may appear plainer by considering that $l\left(1 + \frac{nw}{x}\right)x = l\left(1 + \frac{nw}{x}\right) + lx$, and that in the expansion of $(x - y)^m$, the sum of the affirmative is equal to the sum of the negative coefficients.

In like manner we find $\Delta^n\left(\frac{1}{lx}\right) = \frac{1}{l\left(1 + \frac{nw}{x}\right)} - \frac{n}{l\left(1 + \frac{(n-1)w}{x}\right)} + \frac{n(n-1)}{2 \cdot l\left(1 + \frac{(n-2)w}{x}\right)} - \dots$

Example 6.—To find the n th increment of $x \log. x$.

Put $\phi = x \log. x$ and $\Delta x = w$, a constant quantity, then will $\phi_n = (x + nw) \log(x + nw)$; $\phi_{n-1} = \{x + (n-1)w\} \log\{x + (n-1)w\}$ &c.

By substituting in problem 2,
 $\Delta^n(x \log. x) = (x + nw) \log(x + nw) - n \{x + (n-1)w\} \log\{x + (n-1)w\} + \frac{n(n-1)}{2} \{x + (n-2)w\} \log\{x + (n-2)w\} \dots \pm x \log. x$.

By multiplying and dividing by $x \log. x$, and putting $1 + \frac{nw}{x} = v$, $1 + \frac{(n-1)w}{x} = v'$, $1 + \frac{(n-2)w}{x} = v''$ &c. we shall have

$\Delta^n(x \log. x) = x \log. x \left(v \log v - n v' \log v' + \frac{n(n-1)}{2} \cdot v'' \log v'' \dots \pm 1\right)$

Example 7.—To find the n th increment of $(\log. x)^m$.

Put $\phi = (\log. x)^m$ and $\Delta x = w$, a constant quantity, then will $\phi_n = \{l(x + nw)\}^m = \left\{l\left(1 + \frac{nw}{x}\right)x\right\}^m$
 $\phi_{n-1} = \{l(x + (n-1)w)\}^m = \left\{l\left(1 + \frac{(n-1)w}{x}\right)x\right\}^m$
 &c.....

By writing $v, v', v'',$ &c. for quantities similar to those in Example 6, and substituting in Problem 2, we have

$$\Delta^n (l x)^m = (l \cdot v x)^m - n (l \cdot v' x)^m + \frac{n(n-1)}{2} (l \cdot v'' x)^m \dots \pm (l x)^m$$

By multiplying and dividing by $(l x)^m$

$$\Delta^n (l x)^m = (l x)^m \left\{ (l \cdot v)^m - n (l \cdot v')^m + \frac{n(n-1)}{2} (l \cdot v'')^m \dots \pm 1 \right\}$$

Example 8.—To find the n th increment of $\sin. x$ (radius unity).

Put $\phi = \sin. x$ and $\Delta x = w$, a constant quantity, then will

$$\frac{\phi_n}{\sin. x} = \frac{\sin. (x + n w)}{\sin. x} = \frac{\sin. x \cdot \cos. n w + \cos. x \sin. n w}{\sin. x} = \cos. n w + \sin. n w \cot. x$$

$$\frac{\phi_{n-1}}{\sin. x} = \cos. (n - 1) w + \sin. (n - 1) w \cot. x$$

$$\frac{\phi_{n-2}}{\sin. x} = \cos. (n - 2) w + \sin. (n - 2) w \cot. x$$

&c.....

Then by substituting in Problem 2,

$$\Delta^n (\sin. x) = \sin. x \left\{ \cos. n w + \sin. n w \cot. x - n \left(\cos. (n - 1) w + \sin. (n - 1) w \cot. x \right) + \frac{n(n-1)}{2} \left(\cos. (n - 2) w + \sin. (n - 2) w \cot. x \right) \dots \pm 1 \right\}$$

The arc Δx or w being considered as incomparably small, the $\cos. n w, \cos. (n - 1) w, \cos. (n - 2) w,$ &c. will be very near unity, and the $\sin. n w, \sin. (n - 1) w, \sin. (n - 2) w,$ &c. will be nearly equal to $n w, (n - 1) w, (n - 2) w,$ &c. on these suppositions, the above equation would become

$$\Delta^n (\sin. x) = \sin. x \left\{ 1 + n w \cot. x - n \left(1 + (n - 1) w \cot. x \right) + \frac{n(n-1)}{2} \left(1 + (n - 2) w \cot. x \right) \dots \pm 1 \right\}$$

Example 9.—To find the n th increment of $\cos. x$ (radius unity).

Put $\phi = \cos. x$, and $\Delta x = w$, a constant quantity, then will

$$\frac{\phi_n}{\cos. x} = \frac{\cos. (x + n w)}{\cos. x} = \cos. n w - \sin. n w \tan. x$$

$$\frac{\phi_{n-1}}{\cos. x} = \cos. (n - 1) w - \sin. (n - 1) w \tan. x$$

$$\frac{\phi_{n-2}}{\cos. x} = \cos. (n - 2) w - \sin. (n - 2) w \tan. x$$

&c.....

By substituting in Problem 2,

$$\begin{aligned} \Delta^n (\cos. x) = \cos. x \{ & \cos. n w - \sin. n w \tan. x \\ & - n (\cos. (n-1) w - \sin. (n-1) w \tan. x) \\ & + \frac{n(n-1)}{2} (\cos. (n-2) w - \sin. (n-2) w \tan. x) \\ & \dots \pm 1 \} \end{aligned}$$

The same remark relative to the smallness of the arc w , may be applied to this example as was in the last, and give

$$\Delta^n (\cos. x) = \cos. x \{ 1 - n w \tan. x - n (1 - (n-1) w \tan. x) + \frac{n(n-1)}{2} (1 - (n-2) w \tan. x) \dots \pm 1 \}$$

Example 10.—To find the n th increment of $\tan. x$ (radius unity).

Put $\phi = \tan. x$ and $\Delta x = w$, a constant quantity, then

$$\begin{aligned} \frac{\phi_n}{\tan. x} &= \frac{\tan. x + \tan. n w}{\tan. x (1 - \tan. n w \tan. x)} = \frac{1 + \tan. n w \cot. x}{1 - \tan. n w \tan. x} \\ \frac{\phi_{n-1}}{\tan. x} &= \frac{1 + \tan. (n-1) w \cot. x}{1 - \tan. (n-1) w \tan. x}; \quad \frac{\phi_{n-2}}{\tan. x} = \frac{1 + \tan. (n-2) w \cot. x}{1 - \tan. (n-2) w \tan. x}, \text{ \&c.} \end{aligned}$$

By substituting in Problem 2,

$$\begin{aligned} \Delta^n (\tan. x) = \tan. x \{ & \frac{1 + \tan. n w \cot. x}{1 - \tan. n w \tan. x} - n \left(\frac{1 + \tan. (n-1) w \cot. x}{1 - \tan. (n-1) w \tan. x} \right) \\ & + \frac{n(n-1)}{2} \left(\frac{1 + \tan. (n-2) w \cot. x}{1 - \tan. (n-2) w \tan. x} \right) \dots \pm 1 \} \end{aligned}$$

By considering the arc w incomparably small, the following equation will be nearly true, viz.

$$\begin{aligned} \Delta^n (\tan. x) = \tan. x \{ & \frac{1 + n w \cot. x}{1 - n w \tan. x} - n \left(\frac{1 + (n-1) w \cot. x}{1 - (n-1) w \tan. x} \right) \\ & + \frac{n(n-1)}{2} \left(\frac{1 + (n-2) w \cot. x}{1 - (n-2) w \tan. x} \right) \dots \pm 1 \} \end{aligned}$$

Example 11.—To find the n th increment of $\cot. x$ (radius unity).

Put $\phi = \cot. x = \frac{1}{\tan. x}$ and $\Delta x = w$, a constant quantity, then

$$\begin{aligned} \phi_n \div \frac{1}{\cot. x} &= \frac{1 - \tan. n w \tan. x}{1 + \tan. n w \cot. x}; \quad \phi_{n-1} \div \frac{1}{\cot. x} = \\ & \frac{1 - \tan. (n-1) w \tan. x}{1 + \tan. (n-1) w \cot. x}, \text{ \&c.} \end{aligned}$$

By substituting in Problem 2,

$$\begin{aligned} \Delta^n (\cot. x) = \cot. x \{ & \frac{1 - \tan. n w \tan. x}{1 + \tan. n w \cot. x} - n \left(\frac{1 - \tan. (n-1) w \tan. x}{1 + \tan. (n-1) w \cot. x} \right) \\ & + \frac{n(n-1)}{2} \left(\frac{1 - \tan. (n-2) w \tan. x}{1 + \tan. (n-2) w \cot. x} \right) \dots \pm 1 \} \end{aligned}$$

By considering the arc w inconsiderably small, the following equation is nearly true, viz.

$$\Delta^n (\cot. x) = \cot. x \left\{ \frac{1 - n w \tan. x}{1 + n w \cot. x} - n \left(\frac{1 - (n-1) w \tan. x}{1 + (n-1) w \cot. x} \right) \right. \\ \left. + \frac{n(n-1)}{2} \left(\frac{1 - (n-2) w \tan. x}{1 + (n-2) w \cot. x} \right) \dots \pm 1 \right\}$$

Example 12.—To find the n th increment of sec. x (radius unity).

Put $\phi = \sec. x$ and $\Delta x = w$, a constant quantity, then

$$\frac{\phi_n}{\sec. x} = \frac{\sec. x \sec. n w}{\sec. x (1 - \tan. x \tan. n w)} = \frac{1}{\cos. n w - \sin. n w \tan. x}; \\ \frac{\phi_{n-1}}{\sec. x} = \frac{1}{\cos. (n-1) w - \sin. (n-1) w \tan. x}; \frac{\phi_{n-2}}{\sec. x} = \\ \frac{1}{\cos. (n-2) w - \sin. (n-2) w \tan. x}, \&c.$$

By substituting in Problem 2,

$$\Delta^n (\sec. x) = \sec. x \left\{ \frac{1}{\cos. n w - \sin. n w \tan. x} \right. \\ \left. - \frac{n}{\cos. (n-1) w - \sin. (n-1) w \tan. x} \right. \\ \left. + \frac{n(n-1)}{2 (\cos. (n-2) w - \sin. (n-2) w \tan. x)} \dots \pm 1 \right\}$$

By considering the arc w incomparably small, the following equation will be nearly true.

$$\Delta^n (\sec. x) = \sec. x \left\{ \frac{1}{1 - n w \tan. x} - \frac{n}{1 - (n-1) w \tan. x} \right. \\ \left. + \frac{n(n-1)}{2 (1 - (n-2) w \tan. x)} \dots \pm 1 \right\}$$

Example 13.—To find the n th increment of cosec. x (radius unity).

Put $\phi = \text{cosec. } x$ and $\Delta x = w$, a constant quantity, then

$$\frac{\phi_n}{\text{cosec. } x} = \frac{\text{cosec. } x \text{ cosec. } n w}{\text{cosec. } x (\cot. x + \cot. n x)} = \frac{\text{cosec. } n w}{\cot. x + \cot. n w} = \\ \frac{1}{\cos. n w + \sin. n w \cot. x} \\ \frac{\phi_{n-1}}{\text{cosec. } x} = \frac{1}{\cos. (n-1) w + \sin. (n-1) w \cot. x}; \frac{\phi_{n-2}}{\text{cosec. } x} = \\ \frac{1}{\cos. (n-2) w + \sin. (n-2) w \cot. x}, \&c.$$

By substituting in Problem 2,

$$\Delta^n (\text{cosec. } x) = \text{cosec. } x \left\{ \frac{1}{\cos. n w + \sin. n w \cot. x} \right. \\ \left. - \frac{n}{\cos. (n-1) w + \sin. (n-1) w \cot. x} \right. \\ \left. + \frac{n(n-1)}{2 (\cos. (n-2) w + \sin. (n-2) w \cot. x)} \dots \pm 1 \right\}$$

By considering the arc w incomparably small, the following equation will be nearly true.

$$\Delta^n (\operatorname{cosec}. x) = \operatorname{cosec}. x \left\{ \frac{1}{1 + n w \cot. x} - \frac{n}{1 + (n-1) w \cot. x} + \frac{n(n-1)}{2(1 + (n-2) w \cot. x)} \dots \pm 1 \right\}$$

Example 14.—To find the n th increment of x_y .

Put $\phi = x_y$, then will

$$\phi_n (x_n)_y = (x + n \Delta x)_{y+n \Delta y} = (x + n \Delta x) + (y + n \Delta y)$$

$$\Delta x = x_n + y_n \times \Delta x,$$

$$\phi_{n-1} = (x_{n-1})_{y_{n-1}} = x_{n-1} + y_{n-1} \times \Delta (x_{n-1}) = x_{n-1} + y_{n-1} \times \Delta x, \&c.$$

Δx and Δy supposed constant.

By properly arranging the terms,

$$\Delta^n (x_y) = x_n - n x_{n-1} + \frac{n(n-1)}{2} x_{n-2} - \&c.$$

$$+ \left(y_n - n y_{n-1} + \frac{n(n-1)}{2} y_{n-2} - \&c. \right) \Delta x$$

Therefore by Problem 2, $\Delta^n (x_y) = \Delta^n x + \Delta^n y \cdot \Delta x$.

Scholium.—By corollary to Prob. 2, we have

$$\Delta^n \phi = \phi (e^d - 1)^n = e^{n d \phi} - n e^{(n-1) d \phi} + \frac{n(n-1)}{2} e^{(n-2) d \phi} -$$

$\&c.$ (I.)

Therefore any of the preceding examples may be solved by finding the *differential* of the function ϕ , and writing its value in the equation marked (I.): take Example 1, where

$$d \phi = d (x^m) = m x^{m-1} d x = m x^{m-1} \Delta x = m x^{m-1} w,$$

Then by substituting in equation (I.) we have

$$\Delta^n (x^m) = e^{m w n x^{m-1}} - n e^{m w (n-1) x^{m-1}} + \frac{n(n-1)}{2} e^{m w (n-2) x^{m-1}}$$

$- \&c.$

By taking Example 3, we have

$$d \phi = d (a^x) = d x \cdot a^x \log. a = \Delta x a^x \cdot l a = w a^x l a,$$

Then by substituting in equation (I.) we have

$$\Delta^n (a^x) = e^{w n a^x l a} - n e^{w (n-1) a^x l a} + \frac{n(n-1)}{2} e^{w (n-2) a^x l a} - \&c.$$

and so on.

Example 15.—To find the n th increment of $x y$.

By separating the symbols of operation from their quantities, and denoting the symbol of x by Δ , and that of y by Δ' , we have

$$\begin{aligned} \Delta (x y) &= (x + \Delta x) (y + \Delta' y) - x y \\ &= (1 + \Delta) (1 + \Delta') x y - x y \\ &= x y \{ (1 + \Delta) (1 + \Delta') - 1 \} \\ &= x y (\Delta + \Delta' + \Delta \Delta') \\ &= x y \{ \Delta + \Delta' (1 + \Delta) \} \end{aligned}$$

Then by operating on the *symbols only*, we have

$$\begin{aligned} \Delta (x y) &= x y \{ \Delta + \Delta' (1 + \Delta) \} \\ \Delta^2 (x y) &= x y \{ \Delta + \Delta' (1 + \Delta) \}^2 \\ \Delta^3 (x y) &= x y \{ \Delta + \Delta' (1 + \Delta) \}^3 \\ &\dots\dots\dots \\ \Delta^n (x y) &= x y \{ \Delta + \Delta' (1 + \Delta) \}^n. \end{aligned}$$

Therefore,

$$\begin{aligned} \Delta^n (x y) &= x y \left\{ \Delta^n + n \Delta^{n-1} \Delta' (1 + \Delta) + \frac{n(n-1)}{2} \Delta^{n-2} \Delta'^2 \right. \\ &\quad \left. (1 + \Delta)^2 + \&c. \right\} \\ &= y \Delta^n x + n \Delta y \Delta^{n-1} (1 + \Delta) x + \frac{n(n-1)}{2} \\ &\quad \Delta^2 y \Delta^{n-2} (1 + \Delta)^2 + \&c. \end{aligned}$$

Or by substituting for $(1 + \Delta) x$, $(1 + \Delta)^2 x$, &c. as in Prob. 1,

$$\Delta^n (x y) = y \Delta^n x + n \Delta y \Delta^{n-1} x + \frac{n(n-1)}{2} \Delta^2 y \Delta^{n-2} x + \&c.$$

Example 16.—To find the n th increment of $x y z$.

By separating the symbols from their corresponding quantities, as in the last example, and denoting the symbol of z by Δ'' , we have

$$\begin{aligned} \Delta (x y z) &= (x + \Delta x) (y + \Delta' y) (z + \Delta'' z) - x y z \\ &= (1 + \Delta) (1 + \Delta') (1 + \Delta'') x y z - x y z \\ &= \{ (1 + \Delta) (1 + \Delta') (1 + \Delta'') - 1 \} x y z \\ &= (\Delta + \Delta' + \Delta'' + \Delta \Delta' + \Delta \Delta'' + \Delta' \Delta'' + \Delta \Delta' \Delta'') x y z \end{aligned}$$

Then by operating on the *symbols only*, we have

$$\Delta^n (x y z) = x y z (\Delta + \Delta' + \Delta'' + \Delta \Delta' + \Delta \Delta'' + \Delta' \Delta'' + \Delta \Delta' \Delta'')^n,$$

which being expanded by the multinomial theorem, and the symbols placed before their corresponding quantities, the increment required will be shown.

Example 17.—To find the n th increment of $x y z$, &c.

$$\begin{aligned} \Delta (x y z, \&c.) &= (x + \Delta x) (y + \Delta' y) (z + \Delta'' z) \&c. - x y z, \&c. \\ &= \{ (1 + \Delta) (1 + \Delta') (1 + \Delta'') \&c. \} x y z \&c. - x y z, \&c. \\ &= \{ (1 + \Delta) (1 + \Delta') (1 + \Delta'') \&c. - 1 \} x y z, \&c. \end{aligned}$$

Then by operating on the *symbols only*, we have

$$\Delta^n (x y z, \&c.) = \{ (1 + \Delta) (1 + \Delta') (1 + \Delta'') \&c. - 1 \}^n x y z, \&c.$$

which being expanded and so arranged that each symbol be placed before its respective quantity, the required increment will be exhibited.

See Appendix to the Translation of Lacroix's Differential and Integral Calculus, by Messrs. Babbage, Peacock, and Herschel, for the three last examples.

I am not aware that the foregoing examples have before been applied to the two general problems herein given. Since the publication of the *Annals of Philosophy* for May, 1820, I have seen demonstrations of the series therein printed at p. 69, vol. iii. Dr. Hutton's Course. After I had finished my solutions, and before I sent them to be printed, I saw demonstrations of the same series in Cagnoli's Trigonometry: the three solutions are very different from each other.

ARTICLE VIII.

Exposition of the Atomic Theory of Chemistry; and the Doctrine of Definite Proportions. By William James Macneven, M.D. Professor of Chemistry and Materia Medica in the College of Physicians and Surgeons of the University of the State of New-York.

(Continued from p. 214.)

45. *Potassium*.—It follows from a comparison of the experiments of Davy, Gay-Lussac, Thenard, and Berzelius, that pure potash is a binary compound of 100 potassium and 20 oxygen by weight; and as in this compound, being the lowest, one atom of potassium combines with one atom of oxygen, these respective atoms will be in the same proportion, i. e.

Ox. Potas. At. ox.

20 : 100 :: 1 : 4 weight of the atom potassium.

Again, Gay-Lussac and Thenard have shown that the peroxide of potassium is composed of 100 potassium and 60 oxygen; or three times the quantity in the protoxide; therefore oxygen combines with potassium in the proportion of 1 to 3, showing the peroxide as a compound of 1 potassium + 3 oxygen.

46. *Barium*.—The base of baryta may be determined from the combination of baryta with sulphuric and carbonic acids.

Sulphate of baryta is composed of acid 100 + 194 base, and the carbonate of acid 100 + 354.54 base. Now, according to a canon of Berzelius deduced by him from numberless experiments, whenever a base and an acid combine, the oxygen in the acid bears a fixed relation to the oxygen in the base. One hundred parts by weight of sulphuric acid will saturate a quantity of base, containing 20 parts oxygen; 100 parts carbonic acid will saturate a quantity of base containing 36.26 parts oxygen. Hence 194 parts baryta, which combine with 100 sulphuric acid, contain 20 oxygen; and 354.54 baryta, which combine with 100 carbonic acid, contain 36.267 parts oxygen. To obtain the proportion of oxygen in 100 of baryta from these data we say, as 194 baryta : 20 oxygen :: 100 baryta : 10.309 oxygen. And as 354.54 baryta : 36.267 oxygen :: 100 baryta : 10.2 oxygen. The mean of these (10.269) is the oxygen in 100 of baryta; and if the oxygen be 10.269, the barium is 89.731. Reducing these numbers to their lowest terms, if the atom of oxygen be 1, the proportional weight of the atom of barium will be 8.73.

47. *Calcium*, as far as yet known, combines with only one proportion of oxygen, and forms the important oxide of lime. It is from the sulphate and carbonate that we can best arrive at the composition of lime, and this method admits of considerable accuracy.

Sulphate of lime has been analyzed with great care, and is a compound of

Sulphuric acid.	100.00	5.0
Lime	72.41	3.620

That is, 100 sulphuric acid are to 72.41 lime as 5, the integrant particle of sulphuric acid, is to 3.620, the integrant particle of lime.

Carbonate of lime has been likewise analyzed with great care, and is composed of

Carbonic acid	43.2	2.75
Lime	56.8	3.61

Thus, the equivalent number for lime, by both experiments, is the same; and as calcium combines with only one proportion of oxygen, we must suppose lime a compound of 1 atom calcium + 1 atom oxygen.

Calcium.	2.62	100.00
Oxygen.	1.00	28.57

48. *Strontium*.—The proportional weight of an atom of strontium may be obtained after the same manner as that of barium. Sulphate of strontium is composed, according to the experiments of Stromeyer, of

Strontian (viz. oxide of strontium) ..	132.55
Sulphuric acid.	100.00

And carbonate of strontian, by the experiments of Dr. Thomson, of

Strontian	234.44
Carbonic acid	100.00

Now, since sulphate and carbonate of strontian are composed of integrant particles of oxide of strontium and sulphuric acid in binary combination; we may find the weight of an integrant particle of strontian by knowing that of either of the acids. The integrant particle of sulphuric acid is represented by 5; namely, 1 atom sulphur = 2, and 3 atoms oxygen = 3; and the integrant particle of carbonic acid by 2.76 viz. .76 carbon and 2 oxygen. To deduce the weight from the sulphate, we use the proportion as 100 sulph. acid : 132.55 strontian :: 5 weight of an integrant particle of sulphuric acid :: 6.62 weight of an integrant particle of strontian, 100 : 132 :: 5 : 6.62.

To deduce it from the carbonate, as 100 carb. acid : 234.44 strontian :: 2.76 : 6.47. The mean of these two 6.62 and 6.47 gives 6.5 without any sensible error for the weight of an integrant particle of strontian.

It is known that 100 parts of sulphuric acid will combine with a base, having 20 parts oxygen; hence, 132.55 parts of stron-

tian have 20 parts of oxygen, or 100 have 15.08. It is likewise known that 100 parts carbonic acid require a quantity of base having 36.267 oxygen; hence, 234.44 parts strontian have 36.267 oxygen, or 100 have 15.46, nearly the same as that from the sulphate. The mean of both results is 15.27, the parts of oxygen in 100 of strontian, which therefore consists in 100 parts of

Oxygen	15.27.
Strontium	84.73
	100.00

Now one integrant particle of strontian = 6.5 divided in the proportion of 100 parts will give the weight of an atom of strontium, which, as far as we can view it, must consist of one atom of strontium + 1 atom of oxygen. Therefore, 100 strontian : 15.27 oxygen :: 6.5 strontian : 0.99275 oxygen in one integrant part of strontian. A result so near 1, the weight of an atom of oxygen, that it may be taken as such. Then 6.5 - 1 = 5.5 weight of an atom of strontium.

49. *Magnesium*.—Magnesium may be determined like barium and strontium. The sulphate of magnesia, according to Berzelius, is composed of

Sulphuric acid.	100.00
Magnesia.	50.06

and as there is but one combination of sulphuric acid and magnesia known, we must consider it a binary, formed by the union of 1 integrant particle of sulphuric acid, joined to 1 integrant particle of magnesia. The integrant particle of sulphuric acid is 5; therefore, as 100 : 50.06 :: 5 : 2.51, the integrant particle of magnesia.

Further, 100 parts of sulphuric acid require a base containing 20 parts oxygen; hence, 50.06 parts of magnesia contain 20 oxygen, or 100 contain 40. Now, to find the proportional part of oxygen in 2.51, integrant particle of magnesia, the same as in 100, there is the proportion 100 : 40 :: 2.51 : 1.

Magnesium.	= 1.5 atom of magnesium
Oxygen	1.0 atom of oxygen.

50. *Yttrium*.—According to the analysis of Berzelius, the sulphate of yttria is composed of equal weights of acid and base; hence, the weight of an integrant particle of yttria is the same as that of sulphuric acid = 5.

Since we know but one combination of yttrium and oxygen, and that oxygen is 1, yttrium must be 4.

Moreover, since 100 parts sulphuric acid require as much base as contains 20 parts oxygen, finding the oxygen of one integrant particle in the same proportion as that of 50, we arrive at the

above result, as well by this useful canon of Berzelius, as by his analysis, since $50 : 10 :: 5 : 1$. The integrant particle of yttria is 5, and deducting 1 oxygen, the atom yttrium = 4.

51. *Glucinum*.—The sulphate of glucina, by the experiments of Berzelius, is composed of

Sulphuric acid	100·0
Glucina	64·1

Considering the sulphate as formed by the union of particle to particle, the weight of an integrant particle of glucina is determined by that of sulphuric acid as $100 : 64·1 :: 5$ weight of the integrant particle of sulphuric acid : $3·205$, integrant particle of glucina. And as 64·1 parts glucina saturate 100 parts sulphuric acid, they contain 20 parts oxygen, and we obtain the oxygen in an integrant particle of glucina by the proportion as $64·1 : 20 :: 3·205 : 1$; consequently $3·205 - 1 = 2·205$ weight of an atom of glucinum.

52. *Aluminum*.—The weight of the atom of aluminum is also determined from the experiments of Berzelius by means of the sulphate. It consists of

Acid	100·0
Alumina	42·722

Hence as $100 : 42·722 :: 5 : 2·115$; and the oxygen in 2·115 is in the same proportion as in 42·722, that is as $42·722 : 20 :: 2·115 : 1$, and $2·115 - 1$ oxygen = 1·115 weight of an atom of aluminum.

53. *Zirconium*.—As no very accurate analyses of the union of acids and zirconia have been made, the number for an atom of zirconium, deduced from the analyses of Klaproth and Vauquelin, can only be considered as an approximation. From the analyses of those chemists, we are led to consider the weight of a particle of zirconia as represented by 5·625, and supposing it the lowest union of oxygen and zirconium, for we know no lower; the weight of the atom of zirconium is found = 4·620 by deducting 1, the weight of oxygen.

54. *Iron*.—The weight of an atom of iron may be obtained from the salts which it forms, by applying to them the canon of Berzelius, that an acid will combine only so far with a base as its oxygen bears a proportion to the oxygen of the acid. In all combinations of sulphuric acid and a base, no matter in what quantity we take one, the oxygen of the acid must be to that of the base as 60 : 20. Different acids have different proportions with regard to their oxygen, and that of the base with which they unite. As, for instance, the oxygen in carbonic acid is always to that of the base with which it unites as 72·73 nearly is to 36·267.

This is a most important law, and merits constant attention, from the great variety of cases to which it is applicable. Pro-

ceeding by this method, if we take the combination of iron with sulphuric acid, it is composed, according to Berzelius, of

Sulphuric acid	100
Protoxide of iron	88

and since composed of particle to particle, by knowing the weight of the integrant particle of sulphuric acid, we can obtain that of iron; for as $100 : 88 :: 5 : 4.4$, weight of an integrant particle of protoxide of iron; and subtracting 1, the atom of oxygen, 3.4 will represent the atom of iron.

Or by the canon of Berzelius, as 88 protoxide of iron is to 20 oxygen, so is 4.4 , integrant particle of protoxide of iron, to 1; but 1 is the weight of an atom of oxygen, and as the protoxide is composed of an atom of oxygen + an atom of iron, $4.4 - 1 = 3.4$, as above.

It may be obtained from other acids with which iron combines by a similar process.

55. *Tin*.—There are two well defined oxides of tin; the first, composed of 100 tin + 13.6 oxygen; the second of 100 metal + 27.2 oxygen; from which it appears that the oxygen of the first is one-half that of the second, or as $1 : 2$. Hence 100 tin + 13.6 is a protoxide composed of atom to atom. We have, therefore, this proportion as $13.6 : 100 :: 1 : 7.352$ weight of an atom of tin.

56. *Copper*.—There are two oxides of copper determined by very exact experiments, the first composed of 100 metal + 12.5 oxygen; the second, of 100 metal + 25 oxygen; now $12.5 : 25 :: 1 : 2$. Hence the first is a protoxide of one atom metal + one atom oxygen, which gives the weight of the atom of copper thus, as $12.5 : 100 :: 1 : 8$ weight of the atom of copper.

57. *Bismuth*.—There is but one oxide of bismuth. It is formed by the union of 100 metal with 11.267 oxygen. We must deem it a protoxide, because we do not know any lower; and to determine the weight of the atom of bismuth, we say as $11.267 : 100 :: 1 \text{ atom ox.} : 8.785 \text{ atom of bismuth.}$

58. *Mercury*.—We may in most cases of the metallic oxides determine which is the protoxide by applying the canon of Berzelius, "that the oxygen of the protoxide is one-half the sulphur in the lowest sulphuret of the same quantity of metal." Now the lowest sulphuret of mercury contains, according to Guibault, 8.2 sulphur + 100 metal. Hence the protoxide of mercury must contain $8.2 \div 2 = 4.1$ oxygen + 100 metal.

Söfstrom, as quoted by Berzelius, found the protoxide composed of 100 metal + 3.99 oxygen, and the peroxide of 100 metal + 7.99 oxygen. Hence we may take 4 for the oxygen in the protoxide, and 8 for that in the peroxide; and the atom of mercury will be represented by 25, as $4 : 100 :: 1 : 25$.

(To be continued.)

ARTICLE IX.

ANALYSES OF BOOKS.

An Essay on Magnetic Attractions, particularly as respects the Deviation of the Compass on Ship-Board, occasioned by the local Influence of the Guns, &c. With an easy practical Method of observing the Same in all Parts of the World. By Peter Barlow, of the Royal Military Academy. 8vo. London. Taylor. 1820.

THE deviation of the compass on ship-board from the iron in the vessel, and the great errors into which navigators are liable to fall, in consequence of this deviation, first attracted the attention of Captain Flinders, while employed in surveying the coasts of New Holland. After a great deal of investigation, he hit upon a method of correcting these errors in his ship. This method was explained in a paper which Captain Flinders transmitted to the Royal Society, and which was printed in the Transactions of that learned body. After Capt. Flinders's return to this country he was employed by the Admiralty to make a set of observations on board different vessels in the Channel to ascertain whether his method of correcting the deviation was generally accurate. It is well known that the result of the investigation was unfavourable. His rule, though it seems to have held good, or very nearly so, in his own vessel, was found not to apply with equal facility to other vessels and other circumstances. During the late voyages of discovery to the north pole, the attention of the public was again drawn to this subject, and many curious observations were published, by Captain Sabine, and others; but no attempt was made to discover a new method of correcting the deviation.

It seems to have been this voyage which led Mr. Barlow to investigate the subject; and he was led by a set of experiments, which he relates in the little volume now under review, or the result of these experiments, to a practical method of correcting the deviation, or, which is the same thing, of determining its amount. This method he details at sufficient length to make it intelligible to practical men.

But though the correction of this deviation was the principal object of Mr. Barlow's labours, he did not neglect to consider the phenomena of magnetism under a scientific point of view; and he has made a discovery, which, if it prove correct, must be admitted to be of first-rate importance, and will tend more to bring magnetism into the state of an accurate science than any fact respecting it yet brought into view. He has found that the force of magnetism, like that of electricity, depends not upon

the solid contents, but upon the surface of the magnetic body ; so that a solid magnet and a hollow magnet, supposing them to have the same surface, may possess the same attractive and repulsive forces. But it will be worth while to take a more particular view of the principal points discussed in this little volume.

The first experiments of the author were made by putting a compass in the centre of a circle, and placing iron balls of various dimensions on the circumference of the circle, making them move gradually round the circumference, and observing the effect produced upon the needle in every position. The result of these experiments was the discovery by the author, that in every ball of iron there are two planes, in which, if a needle be placed, its position will not be in the least affected by the ball. The first of these planes is in the magnetic meridian ; the second, which Mr. Barlow calls the plane of *no attraction*, is perpendicular to the direction of the dipping needle. After having ascertained this very important fundamental law, Mr. Barlow next ascertained the deviation produced upon the needle in all the different degrees of latitude and longitude of an iron ball. The plane of no attraction was considered as the equator, and the circle perpendicular to the magnetic meridian, or passing through the poles of the sphere, and the points of the equator, at the greatest distance east and west from the magnetic meridian, was considered as the first meridian. His observations led him to the two following conclusions :

1. The tangents of the deviations are proportional to the rectangle of the sine and cosine of the latitude, or to the sine of the double latitude, which is the same thing.

2. Other things being the same, the deviation is proportional to the cosine of the longitude.

After proceeding thus far, our author drew up an account of his experiments, and sent them to the Royal Society. The committee of the Society, whose province it is to determine what papers are to find a place in their Transactions, did not consider this paper as fit for publication ; and, as is the constant custom of the Royal Society, they refused to return the paper to the author, or to allow him a copy, unless he paid for the transcription of it.

Not having had an opportunity of hearing Mr. Barlow's paper read, and only being acquainted with the contents of it from the account which he has given of it in the first section of the present work, and not being fully aware of the reasons which led the committee not to publish it in the Transactions of the Royal Society, it would be improper in me to hazard any remarks upon the subject. From some allusions in Mr. Barlow's preface, I am led to conclude that the reason assigned by the committee was, that Mr. Barlow's discovery of the plane of no attraction, though new to him, had, in reality, been long known to those

who had devoted themselves to the study of magnetism. Now I am disposed, in some measure, to concede this. It is distinctly stated by Gilbert, and indeed by all writers on magnetism since his time, that if we draw a needle along a magnet from one pole to the other, there is a particular point between the two poles in which the needle is not affected by the magnet at all. But though this well-known fact ought to have led to the conclusion of the existence of a plane of no attraction, it must be admitted that little or no attention had been paid to it, and that the laws of the deviations of the needle, according to its position with respect to this plane, had never been established. Indeed, unless my memory deceives me, the late Prof. Robison, of Edinburgh, had made an imperfect set of experiments on the subject; but without coming to any very definite conclusions. I own, therefore, that the mere want of novelty does not appear to me a very legitimate reason for refusing publicity to our author's experiments. It would have been a more generous proceeding to have published them with all their imperfections on their head, especially as the Royal Society, as a body, do not consider themselves as responsible for the statements contained in the papers published by them; but hold the author of every paper responsible for its contents. The art of experimenting requires an education, as well as every other art, before a person can become an adept in it. The committee of the Royal Society ought to bear in mind that the harsh rejection of the lucubrations of a young experimenter has a tendency to damp his ardour in the cause of science, and may possibly even drive him into idleness. It is this haughtiness on the part of those who have set themselves up as judges of philosophical merit, which has diminished to so great a degree the number of experimenters in this country. Whether our reviews and our Royal Societies have not of late years been more injurious than favourable to the interests of science is with me no longer a question. When I compare M. Deluc's paper On the Electric Column, Mr. Donovan's paper On the Oxides of Mercury, and Mr. Barlow's paper On Magnetism, all of which have been rejected by the Royal Society within these few years, with many papers published by that learned body, I cannot avoid feeling a good deal of surprise mixed with regret. The committee of the Royal Society ought to be impartial. But when we find such curious facts as are contained in the three papers above-mentioned not sufficient to compensate for the imperfections which they may have displayed, while all the papers written by another favoured individual, however numerous, however expensive, however trifling, or however absurd, are sure to find a place in the Transactions of that learned body, we may give them credit for many valuable qualities, but certainly not for impartiality. Our author may satisfy himself however, from the rejection of Deluc's and Donovan's papers, that no hostility against mere mathematicians

exists in the breasts of the committee; for neither electricity nor chemistry have met with a better fate than his own.

In the present state of society, every discoverer of new and important facts has so many methods of making them known to the public, and the nation contains so many reading, and so many enlightened men, that merit is certain of acquiring celebrity in spite of all the obstacles which those, who think themselves already seated upon the summit, are disposed to throw in its way. A man of science, therefore, need be under no manner of uneasiness, though his discoveries are refused a place in the Transactions of the Royal Society. Deluc's Electric Column is just as well known to men of science, and the discoverer of it possesses just as much credit, as if his paper had been inserted in the Transactions. This will be the case also with Mr. Barlow's magnetical discoveries, provided they possess real merit, and contribute to the real improvement of this very important but imperfect science.—But let us return to the results of Mr. Barlow's experiments.

He was enabled by the kindness of the late General Mudge to construct a much more perfect apparatus in the Model Room of the Royal Military Academy, Woolwich. By means of this, he ascertained that the inclination of the plane of no attraction was $19^{\circ} 24'$, while the dip at Woolwich on July 13, 1819, was $70^{\circ} 30.45'$. It is evident from this that the inclination of the angle of no attraction is the compliment of the angle of the dip. Hence it follows that the plane of no attraction is perpendicular to the position of the dipping needle.

In the sixth section, our author examines the effect on the deviation produced by varying the distance of the needle. He deduces, by a very ingenious train of reasoning, that the tangents of the angles of deviation are reciprocally proportional to the cubes of the distances. If we suppose, according to the experiments of Coulomb, that the force of magnetic attraction varies inversely as the square of the distance, then the above law will become *the tangents of the deviation are directly as the force, and reciprocally as the distance.*

The next object of investigation was the law of attraction as regards the mass. This led to the discovery of what I consider by far the most important new fact contained in the present work. He found that the power of attraction resided wholly on the surface, and was independent of the mass. The tangents of the deviation are proportional to the cubes of the diameters, or as the $\frac{3}{2}$ power of the surface, whatever may be the weight and thickness. Our author, however, found afterwards that this law requires some modification; for the magnetic fluid was found to require a certain thickness of metal, exceeding $\frac{1}{50}$ th of an inch, in order effectually to develop itself, and to act with its maximum effect.

The next object of investigation was to ascertain whether

these laws hold in irregular masses of iron. For this purpose our author applied to Sir William Congreve for permission to pursue his inquiries in the Royal Military Repository at Woolwich; which was immediately granted. He selected for his purpose an iron 24-pounder, mounted on a platform, which admitted of its being traversed through an entire circumference; the trucks at the bottom running over a circle ten feet six inches in diameter. With this gun he made a set of experiments, which agreed with the preceding ones in every essential particular, and showed that the same laws hold in the case of irregular masses of iron, as were ascertained to exist in iron balls.

In the 12th section, Mr. Barlow explains the method which he conceives calculated to enable us to determine the deviation occasioned by the iron on ship-board in all situations of the vessel, and in every part of the world. The method is simply this: Procure an iron plate of such dimensions that when placed within a certain distance of the compass, it will just double the angle of deviation. Ascertain this position, and mark it; then set the plate of iron aside. When at any time it is wished to ascertain the angle of deviation, we have only to observe the position of the compass, and then place the iron plate in the place marked for it. The angle of deviation will now be doubled. Hence the deviation produced by the plate will mark the deviation of the compass from the true point. In reality indeed it is only the tangents of these two angles that are equal; but as in small angles the tangents are very nearly proportional to the angles, we may assume the angles as equal, without any material error. Mr. Barlow, however, has given a formula for calculating the true angle of deviation from the tangents.

The 13th section is taken up with a set of directions for applying this corrective apparatus to the binnacle of a vessel. These directions must be of great importance to practical seamen, though they would not be sufficiently interesting to the general reader to make it requisite for us to give them a place here. We understand that Mr. Barlow is at present employed verifying this method, by actually trying the experiment on board several men of war. We trust that the method will be found satisfactory in practice. It will furnish a most valuable addition to the improvements in navigation which have been made during the course of the last century; and ought greatly to facilitate the discovery of the law by which the declination of the compass is regulated. At least it will enable navigators to determine the true declination with much greater facility than formerly.

The 14th section is occupied with a hypothetical explanation of the daily variation of the compass. It is now well known that the declination of the needle not only varies from year to year, but even, during every day, a small variation may be perceived by means of a delicate instrument. Let us suppose the needle at sunrise to have a particular position; as the sun advances

towards the west, the declination of the needle becomes more westerly, and it reaches its maximum about half-past one, p.m. It then begins to move in a retrograde direction, and at sunset reaches nearly the same point that it pointed to at sunrise. The most complete sets of observations on this subject are those of Col. Beaufoy, which have been continued for these three years, and have been regularly published in the *Annals of Philosophy*. Mr. Barlow's hypothesis to account for this daily variation is, that the sun possesses a certain quantity of magnetic influence, derived from ferruginous particles in its composition. He shows, by a satisfactory induction, that all the phenomena of the daily variation are consistent with this hypothesis, and completely explained by it, provided we admit that the magnetic energy is not propagated instantaneously, but moves at the rate of about 20,000,000 of miles per hour.

The 15th section of Mr. Barlow's work deserves, I think, the peculiar attention of my scientific readers. I shall, therefore, terminate this imperfect analysis of a highly interesting work by inserting the whole of this concluding section. I do it the more readily, because several of the notions which the author seems to entertain coincide very closely with some opinions respecting magnetism which I have entertained for some time, and which indeed struck me on perusing the very passages in Biot's elaborate work to which our author alludes.

General Remarks relative to the Nature of Magnetic Action, consequent Corrections, &c.

129. At present I have hinted at no hypothesis explanatory of the law of action which may be conceived to have place between the iron and compass. We know that, agreeably to the theory, first, I believe, advanced by Gilbert, but since adopted and extended by Coulomb, Biot, and others, the ball of iron which I have employed in my experiments being placed in the neighbourhood of the great terrestrial magnet, has itself acquired a certain portion of magnetic influence; its upper part possessing the boreal, and the lower, the austral quality. Consequently, according to this supposition, the end of the needle, which possesses the same, or the opposite magnetism, to that pole of the ball which is exposed to its action, or which has a predominance of action, will be repelled or attracted.

I have great apprehension in calling in question a theory sanctioned by the approbation of so many eminent writers who have investigated this subject since the time of its first promulgation by our ingenious countryman; and shall not, at least in the present instance, attempt to do so: I shall only observe respecting it, that whether it be correct or not, it furnishes no clue to any *practical* mode of computing the angles of deviation, as we have found them to obtain in the experiments reported in the preceding pages of this work. I do not assert that it is actually inconsistent with the observed deductions, but merely, that it leads to such a complicated analysis as to render it wholly useless as a practical theory; and, if I were to add that I have some doubts of the accuracy of the deductions on which it is founded, I should be supported by the opinion of some other writers on the same subject. Dr. Young, for example, speaking of this hypothesis, or rather perhaps of others necessary to support it, observes: "This is obviously improbable, but still the hypotheses are of great utility in assisting us to generalize and to retain in memory a number of particular facts, which would otherwise be insulated.*" This is all that I wish to have conceded to me in what I am about to advance; it is, in fact, all that I intend; namely, to have something on which the mind may be fixed, in order to see, *à priori*, the probable consequence of this or that particular or relative position of the ball and compass.

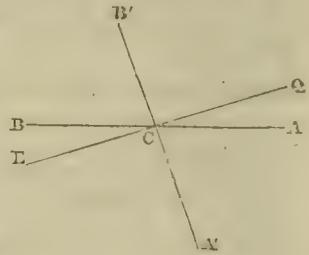
* Dr. Young's Nat. Phil. vol. i. p. 685.

130. According to the hypothesis above adverted to, it is almost impossible to see beforehand whether any given situation of the ball will produce an easterly or westerly deviation, and more particularly to form any idea of the quantity, as to greater or less. In all the early part of my experiments, I had this difficulty to encounter; and it was only after some reflection that I was enabled to arrange the results in a manner which presented a certain degree of order: this order was rendered, however, tolerably perspicuous after I had determined the existence of the plane of no attraction.

131. I had already arrived at this conclusion when I mentioned the results of my experiments to my colleague, S. H. Christie, Esq. who became much interested in the inquiry, and afterwards spent with me some time in the pursuit; and to him I owe the following simple idea of the mode of action, which, whether it be actually correct or not, will serve (as Dr. Young has remarked, with reference to the theory of Coulomb), as a convenient vehicle of illustration, and for uniting under one general head a number of facts which would be otherwise detached and insulated.

On examining the nature of the several results obtained in my first series of experiments, adverted to in Sect. I. the above-named gentleman observed, that both the quality and quantity of the several deviations were (as nearly as could be then ascertained) such as would have had place, supposing the needle to have been inclined in its natural direction, and then referred to the horizontal plane: in fact, that whatever might be the position of the ball, the result still seemed to have a certain relation with the quality and quantity of the deviation which would take place on a line passing through the centre of the needle, and having an inclination corresponding to that of the dip. This line, for the purpose of fixing the ideas, we may imagine to be a fluid passing in this direction, and that the deviation is caused by the action of the iron upon either of its branches to which the iron is most exposed. Let us examine how far, by admitting this view of the subject, we may be able to account for the several results obtained in the foregoing experiments.

132. Conceive A B to represent a magnetized needle, directed freely on the plane of the magnetic meridian: the natural direction which this needle would assume, were it not for the counterbalance applied to the arm B C, would be B' A'; and to some agent acting in the latter direction, according to our supposition, it owes its directive quality; which is only changed from its inclined to its horizontal position by the counterweight. This agent still maintaining its inclined direction, it is obvious, that if by the approach of a ball, or mass of iron, it is deflected out of its natural course, the needle itself will experience a similar change; consequently, whatever may be the position of the needle, the effect produced by the approach of a mass of iron will depend entirely upon its situation with reference to the line B' A'.*



If the centre of attraction of the mass be placed any where in the line E Q, or in the plane of that line (which is supposed perpendicular to B' A'), its action will be equal upon each branch B' C, and C A', and consequently no motion ought to ensue; and hence we see the origin and cause of the plane of no attraction. If, on the other hand, the ball be elevated above the plane E Q, it will act strongest upon, and attract the southern branch B' C, and consequently the arm B C of the needle will likewise approach the iron. Again, when the ball is below the plane E Q, then the attraction being strongest upon the branch of the fluid C A', it will be deflected towards the ball, and the arm C A of the needle will accordingly be attracted in the same direction; or the other arm B C will appear to be repelled in the contrary one. All the phenomena, therefore, attending our leading experiments, are consistent with this view of the subject. The apparent anomaly attending the action of the great and small ball (art. 3) is also immediately accounted for on the same principle; for, in the one case, the centre of the ball is above, and in the other below the plane of no attraction, and ought, therefore, to produce opposite effects. Let us now see how far this idea will assist us in correcting some of the anomalies observed in the preceding sections.

* It is to be understood that we here suppose the iron to be so far distant from the needle as to be out of that particular sphere of influence, of either extremity, adverted to in (art. 70).

Whence $\tan. A$ in oblique angled spherical triangles, having the side a very small, is to $\tan. A'$ in right angled spherical triangles under the like conditions, very nearly as $\cos. l$ to radius.

135. That the side a or $S S''$ in our case is very small, may be shown immediately, as follows :

We have seen that when $l = 0$, that is in the case of a right angled triangle, we have

$$\tan. H V = \tan. S S' \cdot \operatorname{cosec}. S Z$$

$$\text{Whence } \tan. S S' = \frac{\tan. H V}{\operatorname{cosec}. S Z} = \frac{\tan. H V}{3.02056}.$$

Now, in no case that I have reported in Section V. does the arc $H V$ exceed $0\frac{1}{2}^{\circ}$; and, consequently, in no one of those cases does $S S''$ exceed $2^{\circ} 10'$, and is, therefore, small in comparison with the constant arc $S Z = 19\frac{1}{2}^{\circ}$.

136. In order to see the greatest actual error which can arise by the approximate formula alluded to, we have only to introduce into our corrected formula, viz.

$$\tan. A = \frac{\tan. a \cos. l}{\sin. b - \frac{\cos. b \cos. C}{\cot. a}}$$

The value of $\cot. a$, taking the arc at its maximum 2° , the cotangent of which is 28.6362; this will give us

$$\tan. A = \frac{\tan. a \cos. l}{\sin. b - \frac{\cos. b \cos. C}{28.63, \&c.}}$$

In which expression it is obvious, since $\cos. b$ and $\cos. C$ are each less than unity, that the fraction

$$\frac{\cos. b \cos. C}{28.63, \&c.}$$

can never be greater than 1-28th; and consequently, the value of $\tan. A$ can never be increased beyond its correct value more than 1-27th of the same, nor diminished by more than 1-29th.

The slight aberrations, therefore, noticed in Section V. which had somewhat perplexed me before I fell upon the preceding explanation, may now be considered as a confirmation of the accuracy of my observations, and of that particular view of the subject which has enabled me to account for such minute discrepancies.

137. *Observations relative to the General Formula.*—

$$\tan. \Delta = \frac{D^3}{\Lambda d^3} (\sin. 2 \lambda \cos. l) \frac{\cos. m \delta}{\cos. m \delta'}$$

In Art. 66, where I have introduced this formula, I have spoken very cautiously of what might be the probable value of m , that would render it general for all latitudes; and although I afterwards ventured to estimate it at $\frac{3}{2}$, I did it with considerable hesitation, and have, I trust, sufficiently apprised the reader that he ought to consider it entirely as a matter of doubt, which I did not even consider removed by the approximation it was afterwards found to give towards the observations of Captain Sabine, in Baffin's Bay. (Art. 88.)

All that I have stated on this head in the articles referred to was written previous to my having fallen upon the explanation of the aberrations in the law of the longitudes contained in the preceding articles, and will, I trust, convey no unfavourable impression upon the mind of the reader, as to the caution with which I have made my several deductions. I have now to show, that notwithstanding the plausible approximation which this formula was found to give in Art. 88, the value of m is probably erroneously determined, at least if we admit the explanation given in Art. 133, et seq.

According to the ideas there advanced, it seems to follow, that the deviation of the inclined needle, or line of fluid, is wholly independent of the directive power of the horizontal needle; and, consequently, that the deviation shown by the latter, when acted upon by the same mass of iron, similarly posited as to longitude and latitude, will be directly as the cosecant of the arc $S Z$, or as the secant of the dip; or, which is still the same, inversely as the cosine of the dip. Instead, therefore, of $m = \frac{3}{2}$, we ought, according to this doctrine, to have $m = 1$. It is, how-

ever, I conceive, impossible to determine this question satisfactorily, independent of observations in different latitudes. Those mentioned by Captain Sabine are unfortunately not sufficient for this purpose, as either value of m gives us an error so nearly the same in quantity, being in one case as much in excess as it is in defect in the other, that we cannot decide between them: moreover, the way in which these deviations are reported, is so equivocal, that they can hardly be considered as forming a sufficient test of the accuracy of any general law. It is, therefore, to future observations in different latitudes that we must look for the decision of the above question; and these may probably show that, independent of the correct determination of the value of m , it may be found necessary to introduce another indeterminate quantity, to represent the law of *intensity* of magnetic action in different latitudes.

138. I have already stated (Art. 98) that the Admiralty had complied with my request, viz. to be allowed to affix one of the apparatus described in Section XIII. on board some one of his Majesty's ships, and the *Owen Glendower* had been mentioned for the purpose; but for certain reasons it was thought best to defer the experiment for a more convenient opportunity.

The Lords of the Admiralty have since submitted my proposition to the consideration of the Board of Longitude, who have been pleased to sanction it with their approbation; and I am, therefore, in hopes that some new order will ere long be issued for the experiment being put fully in practice, at least as respects those points which, in the present state of the question, cannot but be considered as doubtful.

139. Before I finally dismiss this subject, I beg once more to observe, that I wish not the views which have been advanced in this Section to be considered in the light of an hypothesis, but merely as a convenient mode of illustration. It is not impossible but that the hypothesis of Coulomb may lead (by a reduction of the several forces) to a single resultant, whose direction, and the amount of the deviations produced by it, may correspond, or be analogous to those which we have found to obtain in the experiments reported in the preceding part of this work; and should this be the case, it would be highly satisfactory, both by confirming the accuracy of my practical observations, and the theoretical deductions of the author referred to above. I may further observe, that it would be sufficient that this agreement be found to have place when the mass of iron is great, and the distance considerable; this being the case in my experiments. For having had a particular object in view, namely, to find some law or method of correcting the deviation of the needle on shipboard, I carefully avoided embarrassing myself with any inquiries which did not bear immediately upon that question; I employed, therefore, only needles of the usual length, and endeavoured to work at as great a distance from them as was consistent with the nature of my investigation: I am, therefore, by no means disposed to advance, that with small pieces of iron, and with needles of all lengths, the same laws will still obtain: indeed I have already pointed out (Art. 70) that within certain limits or distances they must necessarily fail.

If, therefore, when the mass of iron is great, and the distance at which it acts be considerable, the laws which I have developed should be found to be the necessary consequence of the hypothesis to which we have alluded, the agreement will furnish one of the best proofs that has yet been given of the accuracy of the deductions upon which that hypothesis is founded, and I should hope without detracting in any manner from the value of the experimental results detailed in the foregoing pages of this work.

140. The reader, however, will perceive that while I admit the possibility of the hypothesis of Coulomb being consistent with my experimental deductions, I have by no means expressed myself very positive that such will be the case: indeed, I have hinted, in more than one place, that I have some doubts with regard to the truth of that theory, and on this point it may not be amiss to offer a few remarks.

The leading features of the hypothesis in question are, that the earth is actually, or at least operates as, a powerful magnet; that it has two principal poles, the one in a high north latitude, and the other in south latitude; and that, abstracting from certain inequalities, it gives a directive equality to magnetized bars, which accordingly arrange themselves in any plane passing through those poles.

Moreover, the earth being, according to this supposition, a magnet, it is imagined to have, like all other magnets, the power of imparting its own qualities to every mass of iron in its vicinity; and consequently, if a bar or ball of iron be suspended near the earth, the lower half of it will possess a south pole, and the upper half a

north pole; and hence is explained why on approaching the lower end of an iron bar to the north end of a needle, it will repel it; while, if we approach the upper end of the bar to the same end of the needle, it will attract it.

The following experiment, given by M. Biot, in his "Précis Elementaire de Physique," tom. ii. p. 7, will at once illustrate what is above stated.

"Suspend, a magnetized needle at its centre, by an assemblage of untwisted silk, and equilibrate it by a counterweight at the south end, so that it may maintain itself in the horizontal plane, as well as in that of the magnetic meridian. Now take a bar of soft iron, A B, about a metre and a half in length, and two centimetres square: then, inclining this bar very nearly in the plane of the magnetic dip, bring its lower end, A, near the north end of the needle, and the latter will be repelled: approach, on the contrary, the upper end, B, (as in the bottom figure) by gradually lowering the bar parallel to itself, and the same end will be attracted. We see then that in this state of inclination the bar is found suddenly animated with the magnetic influence of the terrestrial globe, as it would have been presented to any other magnet; its inferior or lower half possessing a magnetism contrary to that which predominates in our hemisphere, viz. the austral magnetism; and the upper half requiring the opposite species, or the boreal.

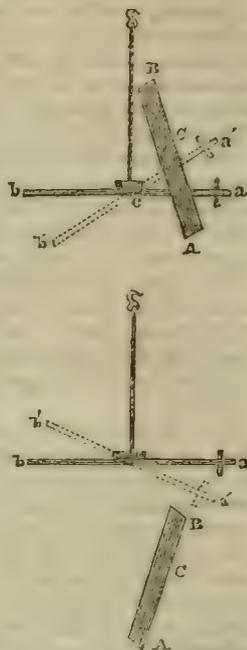
"The two ends of the bar are, therefore, in the same state as the needle, a b, itself, and this is the reason that on bringing the end A of the bar to the extremity a of the needle, we observe repulsion, while on approaching the other end of the bar to the same end a of the needle, we find attraction.

"In order to show that these phenomena depend on a sudden animation impressed upon the bar in virtue of its position, we have only to invert it end for end, its inclination remaining the same, when each of its extremities will produce again the same phenomena as we have described above; and consequently, these phenomena will be directly the reverse of those which the same ends produced in the first instance. The magnetic poles of the bar have been therefore suddenly changed by this inversion; but in order that this effect may be instantaneous, it is requisite to employ a bar of soft iron, and not one of hard iron or steel."

141. I have made several experiments, with a view of either confirming or confuting this doctrine, and have met with some results which I find very difficult to reconcile with it: I shall, however, only state the following: As we are particularly requested in the preceding directions to employ a bar of soft iron, on account of the facilities with which it changes its poles, I was naturally curious to ascertain what would be the result if we made use of a bar of hard steel; as this is known to receive its magnetic properties very slowly, and to retain it obstinately when once acquired. I, therefore, procured a steel bar of the above description three feet long, an inch broad, and half an inch thick, and had it rendered, according to the expression of the workmen, "as hard as fire and water could make it;" and I must say that I was not at all surprised to find that it produced precisely the same effects as the softest iron, changing its poles with its position (to adopt the language of our author) with equal facility.

Now this being undeniably the case, why is the reader requested to use soft iron, "and not hard iron or steel," unless it be to hide a blot in the theory, which I cannot see any very easy way to remove.

142. That this is an important objection to the hypothesis in question is obvious from the following remarks of Dr. Robison. (Supplement to Encyclopædia Britannica. Art. Magnetism, No. 46.) "It is of peculiar importance to remember, that the acquisition of magnetism is gradual, and that the gradation is the more perceptible in proportion as the steel is of a harder temper. When a magnet is brought to one end of a bar of common iron, its remote extremity, unless it be exceedingly long, acquires its utmost magnetism immediately. But when the north end of a magnet is applied to one end of a bar of hard steel, the part in contact immediately becomes a south pole, and the far end is not yet affected. We observe a north



pole formed at some distance from the contact, and beyond this a faint south pole. These gradually advance along the bar. The remote extremity becomes first a south pole, and it is not till after a very long while (if ever) that it becomes a simple vigorous north pole: more commonly it remains a diffused and feeble north pole: nay, if the bar be very long, it often happens that we have a succession of north and south poles, which never make their way to the far end of the bar."

Now let me ask again, if there be (and no one who has made experiments will doubt the fact) the difficulty above stated, in transmitting the magnetic quality through a bar of hard steel, even when in contact with a powerful magnet, how are we to account for that sudden change which (according to the explanation given of the former experiment) takes place merely from a change of position? At all events I think it must be admitted that the direction given for using soft iron is unnecessary, and worse than superfluous, because it supposes a distinction which does not exist.

143. I have also some doubt whether the computation given by M. Biot relative to the position of the poles of the terrestrial magnet does not go a considerable way towards proving a defect in the theory in question.

If the earth be a magnet, and has, like other magnets, two strongly attracting poles, we ought to find a very considerable increase of magnetic intensity, as we approach towards those parts of the globe where they are situated: whereas, as far as voyages have hitherto been made, the change in this respect is far from being inconsiderable.

This circumstance alone would naturally lead us to infer, that they must be situated at an immense depth below the surface; but we have better authority with reference to this point. M. Biot, after assuming their existence, left the distance between them indeterminate; and then, by comparing his general expressions with the results of observations in different latitudes, endeavoured to determine their distance and position.

Making this comparison he found that the nearer these poles were supposed to approach towards each other, the nearer his numbers approximated to those deduced from observation; and, finally, by assuming the poles *infinitely near to each other* in the centre of the earth, he obtained something like coincidence between his two series of results.

Now I cannot help observing, that it appears to me if this computation prove any thing, it is, that the hypothesis of the earth, containing within itself two magnetic poles, is altogether erroneous; for what idea can we have of an infinitely small magnet, having its *north* and *south poles* coincident in the centre of the earth, giving directions to bodies at the distance of 4000 miles?

Whether there are any circumstances connected with the experiment reported in Art. 141, or in the nature of the deduction referred to above, that have escaped my observation, and that will reconcile these apparent anomalies with the theory in question, I am unable to say; but till these circumstances are pointed out and rendered obvious, I cannot but entertain some doubt with respect to the accuracy of the principles upon which that theory is founded.

144. I have said that M. Biot, by assuming the two poles of the terrestrial magnet to be indefinitely near to each other in the centre of the earth, had found "something like coincidence between his computed and observed result." This was meant principally in reference to the law of the dip, which, according to the corrected, or rather simplified formula of that author, is

$$\tan. \delta = 2 \tan. \lambda,$$

where δ is the dip and λ the magnetic latitude.

145. In order to enable the reader to judge of the approximation which this law affords towards the angle of inclination, as determined by the best observations, I have made the computations indicated in the following table:

Terrestrial latitude.	Terrestrial longitude.	Magnet. latit.	Observers.	Observed dip.	Computed dip by Biot's formula.	Errors.
68° 22' N	53° 30' W	78° 3'	Sabine.	83° 08'	83° 57'	+0° 49'
60 9	1 12	70 15	Ditto.	74 21	79 50	+5 29
51 31	0 0 fr.	61 44	Do. London.	70 34	74 57	+4 23
	London					
48 50	0 0 fr.	59 20	Biot.	70 0	73 29	+3 29
	Paris					
38 52	16 22	50 8	Humboldt.	68 11	67 20	-0 51
24 53	20 58	36 56	Ditto.	60 50	56 22	-4 28
21 29	25 42	33 34	Ditto.	58 11	52 0	-6 11
19 54	28 45	31 58	Ditto.	58 58	51 18	-7 40
14 15	48 3	25 21	Ditto.	50 40	43 27	-7 13

146. By examining the errors in the last column of this table, and comparing them with the amount of the observed inclinations, I cannot but think that it will be immediately admitted that the former bear too great a proportion to the latter to allow us to consider the formula by which the computations were effected, as exhibiting the true law of magnetic action applicable to this case.

I should hope that my intentions in pointing out these apparent anomalies in the theory we have been examining will not be mistaken; I have no motive for so doing but to give those who are desirous an opportunity of explaining them, if they admit of explanation, and not with the desire of invidiously calling in question the accuracy of the views of a philosopher of such acknowledged talents and perseverance as M. Coulomb.

147. It has been observed that, besides the experiment mentioned in Art. 141, I had made several others in connexion with the same inquiry; which, however, for certain reasons, I shall not publish in the present instance, but shall content myself with proposing, by way of conclusion, the following queries:

The direction of the needle being supposed due to the action of an infinitely small but powerful terrestrial magnet, how are we to account for the following facts, viz.

1. The disturbance or oscillation of the compass needle during lightning, the Auroræ Boreales, and other meteoric phenomena.

2. The fixed polarity given to certain bars of soft iron in exposed situations. Or we may put the question thus:—If certain iron bars become permanently magnetic by their contiguity with the terrestrial magnet,* why is not this the case with all bars of the same metal?

3. How can we explain the curious circumstance, recently discovered by Morichini, relative to the magnetism of the violet ray?

4. How, again, are we to reconcile with the hypothesis of Coulomb, the remarkable fact, lately published by Colonel Gibbs, respecting the magnetic iron ore of Succasunny, viz. "That the ore in the upper part of the bed is magnetic, but that raised from the bottom has no magnetism at first, but acquires it after it has been some time exposed to the influence of the atmosphere?"—(See Thomson's *Annals of Philosophy*, No. 81.

* To prove that common iron may remain very long in *immediate* contact with the most powerful magnets, without becoming itself magnetic, I may state the following striking fact:—

Early in the course of my experiments I had been favoured with the loan of a strong horse shoe magnet, which would support 84lbs. the lifter of which had been in contact with it, in all probability, for 45 years, and yet had not acquired the least sensible degree of magnetism. This instrument was made for Mr. Adams, the celebrated teacher of, and author on, navigation, about the year 1774; and at his death passed into the hands of Sir James Lake; it was afterwards purchased by the Rev. Mr. Warren, of Edmonton, and by him presented to Mr. Adams, of the same place, the son of its first possessor, to whom I was indebted for the loan of it, as well as for the above particulars relative to its date, &c.

5. Mr. Dalton, in his ingenious "Meteorological Observations and Essays," has proved the actual existence of beams of magnetic matter in the atmosphere, having directions corresponding to those of the dipping needle, which that author supposes to be guided by the earth's magnetism. Instead of this, may we not rather consider them as constituting the great directive agent, and that they give rise to those magnetic phenomena hitherto attributed to the earth's magnetism, instead of being guided by it? This view of the subject would be very consistent with the ideas of Mr. Christie, as given in the commencement of this section.

ARTICLE X.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Yeast as a Manure.*

A very curious experiment was made in the early part of this summer by my friend Mr. Philip Taylor, of Bromley, the effects of which I had an opportunity of witnessing in the month of June. It deserves, I think, to be generally known, as various cases may occur in which it may be employed with advantage. He put upon a part of a grass field which surrounds his house at Bromley a quantity of common porter yeast, in order to see what effect it would have as a manure. In the month of June, when I saw the field, the effect of the yeast was very remarkable. That portion of the field which had been manured with it was of a much darker colour, and the grass on it was much longer than on the remaining portion of the field. Thus there cannot be a doubt that porter yeast, and probably every kind of yeast, may be employed as a very efficacious manure. It is easy to imagine various cases in which such yeast might be applied as a manure with considerable advantage.

II. *Yeast.*

Chemists have not yet been able to satisfy themselves of the chemical nature of yeast; nor have they come to a definite conclusion with respect to the part of the very complex body commonly called *yeast*, which really acts as a ferment. Indeed the experiments of Kirchoff seem to lead to the conclusion that yeast owes its efficacy to gluten and starch acting upon each other, and that both of these bodies must be present in yeast (probably in some peculiar state) before it be capable of exciting fermentation. That pure gluten alone does not answer as a ferment has been long known; yet any person may easily satisfy himself that gluten constitutes a very considerable portion of common porter yeast. It was customary some years ago to reduce porter yeast to dryness, and in that state it was carried to the West Indies, and probably to other places, where it was brought by means of water to its original state, or as near it as possible, and then employed as a ferment. Now when yeast is dried, it acquires

the smell, and something of the taste, of cheese; but being changed into cheese by the progress of fermentation is the characteristic property of gluten. Hence it is obvious that yeast, since it undergoes that change in a degree sufficiently sensible to the most careless observer, must contain a quantity of gluten.

III. Colouring Matter of the Cancer *Astacus*.

It is well known that when these animals are boiled, they assume a red colour. M. Lassaigne has made some experiments to ascertain the cause of this development of *red*. He found that crabs' heads separated with care from the fleshy part, when plunged into absolute alcohol of the temperature of 60°, speedily acquire a scarlet colour, which they gradually communicate to the alcohol. He steeped them in repeated doses of alcohol till they ceased to communicate any colour to this liquid. The crabs' heads thus treated were deprived of the property of becoming red when boiled in water. The different alcoholic solutions had a fine red colour. They were put into a capsule, and allowed to evaporate spontaneously. There remained a red matter which had a greasy appearance.

This matter is destitute of taste and smell. It is insoluble in water, whether hot or cold, but it dissolves in sulphuric ether, and in strong alcohol, without the assistance of heat. The solution has a scarlet colour, and is not rendered muddy by the addition of water. This shows that it is not of a fatty nature. Neither potash, soda, nor ammonia, alter its colour. Even the mineral acids, when diluted with water, have no action on it; but when concentrated, they destroy it by changing it into a dirty yellow. Neither salts of lead, tin, iron, or copper, precipitate this colouring matter from its alcoholic solution diluted with water. M. Lassaigne informs us that this colouring matter is lodged in a membrane, which adheres strongly to the head in young crabs, but which may be easily separated in those which have attained a considerable size. This membrane is very fine, and when seen by reflected light has a violet colour; but is purple by transmitted light.—(Jour. de Pharmacie, vi. 174.)

IV. Crystallization of Balsam of *Copaiva*.

M. Pelletier, having broken a bottle of balsam of *copaiva* which had been in his house for 30 years, found at the bottom of the vessel a transparent plate of resin, from which proceeded hexagonal plates, some of which exhibited distinctly the form of six-sided prisms terminated by a face perpendicular to the axis of the prism. These crystals possessed the property of polarising light.—(Ibid. p. 315.)

V. Meteoric Stone.

On Oct. 13, 1820, a meteoric stone fell near Köstritz, in Rus-

sia. This stone has been lately analyzed by Stromeyer, and found to be composed as follows :

Silica	38·0574
Magnesia	29·9306
Alumina	3·4688
Protoxide of iron	4·8959
Oxide of manganese	1·1467
Oxide of chromium	0·1298
	<hr/>
	77·6292
Iron	17·4896
Nickel	1·3617
Sulphur	2·6957
	<hr/>
	98·7762

(Gilbert's Annalen, lxiii. 451.)

VI. Calomel.

Corrosive sublimate was known to the Arabians, and is distinctly enough described by Avicenna, and, according to Bergman, by Abubeker-al-Rhasi, who preceded Avicenna by at least a century ; but we have no evidence that calomel, or *mercurius dulcis*, as it was formerly called, was known to the alchemists. Crollius, in his *Basilica Chymica*, published in 1608, hints at it as a most valuable preparation of mercury, but without giving the least information about the method of preparing it. The same year Joannes Beguin published a book in Paris, entitled "*Tyrocinium Chemicum*," in which he described the whole process of making calomel, and he distinguished the preparation by the name of *Draco mitigatus*. This mercurial preparation very speedily acquired great celebrity, and came to be distinguished by the name of *panchymagogus quercitanus*, in honour of Joseph Du Chesne Domini de la Violette, a very famous French chemist of those days, who published in 1615 a work, entitled "*Pharmacopœa Dogmaticorum Restituta*." During the 18th century, the term *mercurius dulcis* came into general use to denote this preparation of mercury, or sometimes it was called *mercurius sublimatus dulcis*; but this mode of naming was blamed by Neuman, because it was apt to occasion corrosive sublimate and *mercurius dulcis* to be confounded together. The term *calomelas* (which signifies *beautiful black*) was introduced by the French chemists, and applied to *mercurius dulcis*, when several times sublimed, which, in their opinion, added greatly to its virtue. I do not know who the person was who first applied this term ; but I think it must have been between the years 1730 and 1766. I cannot find the term in any French book older than 1730, and Spielmann, whose *Institutiones Chimiæ* were published in the year 1766, gives us the term *calomelas* as new, and as not generally received by chemists. Bergman, whose paper On the

Combinations of Muriatic Acid and Mercury, was first given to the world in 1769, speaks of the term calomel precisely in the same manner as Spielmann. What was the reason of applying so odd a name as *calomelas* to this preparation, and how came the term to acquire such universal currency in Great Britain? I should suppose that it would be easy for some of the editors of the *Journal de Pharmacie* to discover the history of this term, which originated in France. If they can throw any light on the subject, I should take it as a favour if they would publish the results of their inquiries; for I myself have not been so fortunate, during the course of my reading, as to meet with any satisfactory account of this word.

VII. *Karpholite*. By Mr. G. B. Sowerby.

(To Dr. Thomson.)

SIR,

Lisle-street, Leicester-square, Aug. 14, 1820.

A substance named *Karpholite*, by Werner, on account of the general resemblance it bears in colour to straw, is noticed at p. 72, in No. XCI. of your *Annals*. Its colour is there said to be "intense straw yellow, sometimes, though seldom, wax yellow;" to these two colours might be added "perfectly white." It is there said to be "always amorphous," and the "fracture" is said to be "fibrous;" this, with respect to mineralogical description, appears to me to be quite a new language. If a substance be always *amorphous*, I know not how it can have a fibrous fracture. It is true, a substance may be said to be amorphous if, occurring in masses, those masses cannot be said to have any definite external form; but I am not aware that this meaning has ever been given to the term amorphous; and if it were so, it is very incorrectly applied in the present instance; for the *karpholite* generally occurs in very distinctly diverging fibrous concretions, the fibres being of a very crystalline structure, and somewhat transparent; the substance seems to possess a high degree of lustre, and to be very brittle; so that at every minute cross fracture the light is reflected, and the strongly glimmering character is produced; the longitudinal arrangement of the fibres also produces the satiny lustre. It occurs also in an amorphous state in which its fracture is finely earthy, and the substance is white. This last variety appears to result from the *decomposition* of the mineral. I cannot state with certainty what the nature of the rock in which it occurs is, but I think it is granite, though if it be, the feldspar is with difficulty distinguishable: it very nearly resembles the stone in which the crystallized mica of Zinnwald and Schlackenroad occurs, and the *karpholite* is sometimes found filling vesicles in a dark-green irregularly crystallized mica. I am, Sir, your very obedient servant,

G. B. SOWERBY.

* * * The description of the *karpholite* inserted in No. XCI. of

the *Annals of Philosophy* was translated from a paper by Prof. Steinmann, which was referred to in Schweigger's Journal, vol. xxv. p. 413, where it was published. The German word which I translated amorphous is *derb*. This word is usually translated *massive*, but our English word *massive* conveys I apprehend only one half of the meaning of the German word, which indicates the want of a distinct form, or crystalline shape. I conceive the word *amorphous* to be equivalent to *destitute of a regular shape*. A mineral may be very well fibrous, and destitute of a regular shape, at the same time. Thus no mineral can be more completely fibrous than amianthus, yet it never occurs in crystals. When I inserted the notice in question, I had never seen a specimen of *karpholite*. By the kindness of Mr. Heuland I have now a specimen of that rare mineral in my possession. It consists of fibres diverging from a centre. Hence they are probably of a crystalline nature. It would not, therefore, be surprising if it should occur in crystals; so that it is not unlikely that Prof. Steinmann's description will require to be modified hereafter.—T.

VIII. On the Bed of the German Ocean.

At a meeting of the Wernerian Society, Mr. Stevenson, engineer, concluded his second paper on the bed of the German Ocean, or North Sea, in which many striking proofs are again brought forward descriptive of the waste of the *land* upon all parts of the shores, and of the silting or filling up of the bottom of the sea. In illustration of this, after a careful examination and comparison of the cubical contents of the great sand banks situated in the central parts of the German Ocean, it is estimated that the average depth of this sea does not exceed 31 fathoms, although the range of *soundings* extends from 120 to 18 fathoms. Supposing, therefore, that these vast accumulations are of the same uniform mass throughout, they would amount to a quantity of solid matter equal to no less than 28 feet of the perpendicular height or depth of the firm land of Great Britain taken at the level of the sea, and supposing the whole to be a level plain.

It is accordingly assumed that although these calculations are necessarily of a very general nature, yet that they at least tend to show that a great body of water must be displaced in consequence of these extensive sand banks occupying so considerable a portion of the bed of the North Sea, and that the unavoidable effect of which must give a direct tendency to the waters of the German Ocean to overflow their margin in the same manner (if the comparison may be made) as if stones, or other matter, were thrown into a vessel already nearly brimful of water.

In discussing this subject, the German Ocean, or North Sea, is described as a great basin having two inlets, or apertures, the one towards the north, measuring about 280 miles in breadth, which is bounded by the Orkney Islands on the west, and the

Norwegian coast on the east; while the other passage is at the Straits of Dover, measuring only 21 miles across. Now as this aggregate *waterway* is understood to remain nearly a constant quantity, when a storm agitates the Atlantic Ocean, it is inferred that the surplus and tidal waters of this great sea entering the German Ocean, which is found to be so much encumbered with debris, the effect of this operation must be to produce the destructive appearances which we every where find to arise from the encroachments of the sea.

From the numerous sources of waste adduced in these papers as observable both at the margin of the ocean, and in the interior of the land, we are not surprised to find such immense collections of solid matter accumulated in the form of sand-banks, to which not only the sea itself along the shores, but every rill and river, is constantly bringing fresh matter to the ocean, that great storehouse for these exuvia, which, in their turn, must necessarily displace a corresponding bulk of water.

It, therefore, becomes an interesting branch of this subject to inquire how the surplus waters are to be disposed of, especially if we extend this reciprocating principle of the deterioration of the land, and the elevation of the waters of the ocean, to all parts of the globe.

In accounting for this, it is observed in the first place that water, being the great *pabulum of nature*, is universally employed in the organisation and nourishment of all bodies, and that the quantity of water thus entering, either simply, or chemically, into the constitution of the whole animate and inanimate creation, an immense quantity of water is supplied by the process of evaporation, which must thus be permanently held in solution. On the other hand, another view is given upon the supposition of its being contended that the processes of evaporation, condensation, decomposition, and regeneration, so completely compensate and counterbalance each other, that the quantity of water must ever remain the same. In this case, it is suggested that the surplus waters arising from the silting up of the bottom of the ocean may be taken off by the natural tendency of the fluid to find its level at the oblate figure of the Poles, these points being comparatively nearer to the centre of the earth than the equatorial regions where the centrifugal force acting more strongly must there prevent the accumulation of these waters.

It is further presumed that such an accumulation of water in the great polar basins may at some former period in the history of the globe have deranged these imaginary points which observation has shown not to be altogether stationary; and to this cause we may perhaps trace many of those appearances which are so puzzling to geologists, and afford undoubted proofs of a greater elevation of the waters of the ocean than we now find them to possess.

This paper concludes by stating the evidences of the waste of the land on all the shores of Great Britain from the northmost island of Shetland to the southmost island of Scilly, and also on some parts of the coasts of France and Holland. Mr. Stevenson has personally made the observations. But he has not confined his inquiries to these shores only; for by the kind assistance of some of his nautical friends, he has been enabled to extend his observations on this subject to all parts of the world, affording, upon the whole, the ultimate result that the land is in a state of waste and decay, while the bottom of the ocean is silting or filling up.

With regard, however, to the distribution of the surplus waters, Mr. Stevenson observes, that he should be happy to see the subject treated by some one better qualified than himself. He at the same time announces another paper upon the bed of the English Channel, which we shall be most happy to see. In the present state of this important inquiry, we can only confess our obligations to the author of this paper for the theory he has given, though we are not prepared to follow him in all his views, but we trust that so interesting a subject will meet with that attention which it deserves, and that it will ultimately come to be more fully discussed.

IX. *Account of the late Solar Eclipse.* By L. Howard, Esq.

At Stoke Newington, during the eclipse, I made the following observations. At noon,

Barometer	30.05 in.
Therm. (a very delicate one of quicksilver)	..	66°
At 0h. 30 min. the eclipse begun some min.		58.5
40	69.5
42	temp. lowered by a great sheet of <i>Cirrocumulus</i> .	66.0
0 56	sun shining out.	68.5
1 0	much <i>Cirrocumulus</i>	68.5
0 10	cloud more dense	66.0
0 15	the strong S E breeze fallen: still some <i>Cirrocumulus</i> .	65.0
0 25	barom. now 30.045 in.	64.5
0 30—45	64.0
0 50	sun shining out	63.5
0 55	the greatest obscuration past	63.0
2 0	in full sun 63°, in shade.	62.5
0 15	in shade	63.0
0 20	sun shining out	64.0
0 40	65.0
0 50	66.5
3 0	67.0
0 20	eclipse over.	65.0

In the night after, the temperature fell at Tottenham to 48·5 with dew. The lowest temperature was observed about seven minutes *after* the greatest obscuration; and by the rate at which it should have advanced, instead of falling, from one to two, p. m. I think we may safely add 3° to the 7° which were observed at Newington, and estimate the total abatement of heat on this occasion at full 10° of Fahr.

At the time of the greatest obscuration, the thermometer being brought from under the tree, and exposed to the sun's rays, the quicksilver rose only half a degree. Yet the little *crescent* formed by the rays collected in a lens of 2½ inches focus, was still capable of firing tinder, and burning out the black spots in blotting paper. My son observed, that the spots of light falling through the shade of trees on the ground, instead of being globular, as usual, were *crescents*.

X. Surrey Institution.

The following arrangements have been made for lectures at this Institution during the ensuing season :

1. On Metallurgy and Mineralogical Chemistry, by Frederick Accum, Esq. M.R.I.A. To commence on Tuesday, Oct. 31, at seven o'clock in the evening precisely, and to be continued on each succeeding Tuesday.

2. On Electricity, by Charles Woodward, Esq. To commence on Friday, Nov. 3, and to be continued on each succeeding Friday at the same hour.

3. On Music, by W. Crotch, Mus. Doc. Professor of Music in the University of Oxford, early in 1821.

ARTICLE XI.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

Mr. Swainson is about to publish the first number of his Zoological Illustrations.

Dr. Sir A. B. Faulkner has in the press a Treatise on the Plague, with Observations on its Prevention, Character, and Treatment.

Mr. E. Morgan, surgeon, will soon publish, Practical Observations on the Nature and Cure of Dropsies and Diseases of the Skin, illustrated by several cases.

Sir R. K. Porter has in the press, Travels in Georgia, Persia, Armenia, &c. in 1817, 1818, 1819, 1820, with engravings of antiquities.

Mr. Edward Grainger, jun. is preparing a Concise System of Anatomy, for the Use of Medical Students.

Outlines of Midwifery: developing its Principles and Practice, with illustrative lithographic engravings, in 1 vol. 12mo. principally designed for Students; by J. T. Conquest, M.D. F.L.S. will appear early in the present month.

Mr. Godwin will shortly publish a new work under the title—"Of Population; an Inquiry concerning the Power of Increase in the Numbers of Mankind: being an Answer to Mr. Malthus's Essay on that Subject."

Dr. Rigby, of Norwich, has in the press, *Franningham, and its Agriculture*; a Treatise designed to show the Utility of applying the New System of Agriculture to small Farms.

Mr. W. G. Rogers will publish, early in October, an Engraving of the Warwick Vase, in the lithographic manner.

JUST PUBLISHED.

Le Dentiste de la Jeunesse, or the Way to have sound and beautiful Teeth. By J. R. Duval. 8vo. 7s.

A Toxicological Chart, in which are exhibited, at one view, the Symptoms, Treatment, and Modes of detecting the various Poisons, Mineral, Vegetable, and Animal, according to the latest Experiments and Observations. By a Member of the Royal College of Surgeons. 2s. 6d.

A Sketch of the History and Cure of Febrile Diseases, more particularly as they appear in the West Indies among the Soldiers of the British Army. By Robert Jackson, M.D. 2 vols. 8vo. the Second Edition, with many Additions.

The Hunterian Oration delivered before the Royal College of Surgeons, London, Feb. 21, 1820. By Anthony Carlisle, F.R.S. F.L.S. 4to. 4s.

Elements of the Theory and Practice of Physic. Designed for the Use of Students. By George Gregory, M.D. 8vo. 10s. 6d.

Lectures on the Structure and Physiology of the Parts composing the Skeleton, and on the Diseases of the Bones and Joints of the Human Body, &c. By James Wilson, F.R.S. 8vo. 12s.

An Historic Sketch of the Causes, Progress, Extent, and Mortality of the Contagious Fever epidemic in Ireland during the Years 1817, 1818, and 1819. By William Harty, M.B. 8vo. 16s.

An Introduction to the Knowledge of Fungusses, intended to bring into Notice this much-neglected Tribe of Vegetables. With Plates. 12mo. 5s.

ARTICLE XII.

NEW PATENTS.

Job Rider, of Belfast Foundry, Ireland, for certain improvements which produce a concentric and revolving eccentric motion, applicable to steam-engines, water-pumps, mills, and other machinery. July 20, 1820.

Henry Botfield Thomason, of Birmingham, for improvements in the making and manufacturing of cutlery, viz. table-knives, desert-knives, fruit-knives, pocket-knives, scissars, razors, and surgical instruments. July 20.

James Harvie, Glasgow, late of Berbice, for improvements in the construction of machines, commonly called ginning machines, and which are employed in separating cotton wool from the seeds. Communicated to him by certain persons residing abroad. Aug. 18.

George Millichap, of Worcester, for an improvement on axletrees and boxes. Aug. 18.

ARTICLE XIII.

Astronomical, Magnetical, and Meteorological Observations.
By Col. Beaufoy, F.R.S.

Bushey Heath, near Stanmore.

Latitude $51^{\circ} 37' 44.27''$ North. Longitude West in time $1^{\circ} 20.93''$.

Astronomical Observation.

Aug. 10.	Immersion of Jupiter's second satellite	{ 11 ^h 13' 30"	Mean Time at Bushey.
		{ 11 14 51	Mean Time at Greenwich.
13.	Immersion of Jupiter's first satellite	{ 13 52 27	Mean Time at Bushey.
		{ 13 53 48	Mean Time at Greenwich.
17.	Immersion of Jupiter's second satellite	{ 13 50 02	Mean Time at Bushey.
		{ 13 51 23	Mean Time at Greenwich.
22.	Immersion of Jupiter's first satellite	{ 10 15 22	Mean Time at Bushey.
		{ 10 16 43	Mean Time at Greenwich.

Magnetical Observations, 1820. — Variation West.

Month.	Morning Observ.		Noon Observ.			Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.	
Aug. 1	8 ^h 40'	24° 26' 41"	1 ^h 25'	24° 41' 03"	— —	—° —' —"	
2	8 35	24 29 53	1 25	24 41 03	— —	— — —	
3	8 40	24 32 51	1 05	24 35 57	— —	— — —	
4	8 40	24 30 38	1 15	24 39 31	— —	— — —	
5	8 35	24 31 26	1 20	24 40 45	7 30	24 35 03	
6	8 35	24 31 06	1 35	24 40 15	7 30	24 33 34	
7	8 45	24 29 26	1 20	24 37 25	7 30	24 33 04	
8	8 05	24 28 38	— —	— — —	7 30	24 33 04	
9	8 50	24 31 01	1 25	24 39 11	7 30	24 34 42	
10	8 40	24 31 02	1 20	24 40 10	7 25	24 34 34	
11	8 35	24 30 04	1 25	24 39 59	— —	— — —	
12	8 35	24 30 51	1 20	24 40 48	— —	— — —	
13	8 35	24 29 58	1 35	24 39 57	— —	— — —	
14	8 40	24 31 56	1 15	24 40 20	7 15	24 33 33	
15	8 35	24 29 07	1 10	24 38 49	7 20	24 33 49	
16	8 35	24 29 26	1 15	24 38 54	— —	— — —	
17	8 45	24 30 41	1 25	24 38 32	7 15	24 31 26	
18	8 35	24 30 18	1 20	24 40 59	7 15	24 31 53	
19	8 35	24 28 15	1 05	24 39 29	7 15	24 31 45	
20	8 35	24 30 59	1 15	24 41 06	7 15	24 32 33	
21	8 55	24 30 40	1 30	24 40 57	— —	— — —	
22	8 40	24 31 37	1 25	24 39 57	— —	— — —	
23	8 35	24 31 05	1 25	24 41 39	7 10	24 32 20	
29	8 35	24 31 25	1 15	24 40 33	7 10	24 33 32	
30	8 35	24 31 20	1 45	24 42 02	— —	— — —	
31	8 40	24 31 05	1 15	24 40 42	7 60	24 33 38	
Mean for Month.	{ 8 37	24 30 25	1 21	24 40 00	7 19	24 33 14	

Meteorological Observations.

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.	
Aug.		Inches.				Feet.			
	1	Morn....	29.333	64°	67°	SW		Fine	61°
		Noon....	29.375	71	57	SW		Fine	71
		Even....	—	—	—	—		Fine	—
	2	Morn....	29.620	61	63	W by S		Fine	} 54
		Noon....	29.643	70	52	W by S		Fine	
		Even....	—	—	—	—		—	—
	3	Morn....	29.533	66	72	SW		Cloudy	} 58 $\frac{2}{3}$
		Noon....	29.500	68	70	SSW		Showery	
		Even....	—	—	—	—		—	70 $\frac{1}{2}$
	4	Morn....	29.286	64	90	SW by S		Showery	} 62
		Noon....	29.389	70	65	W by S		Fine	
		Even....	—	—	—	—		Thun. rn.	72 $\frac{1}{2}$
	5	Morn....	29.368	60	68	W by S		Fine	} 53 $\frac{1}{2}$
		Noon....	29.380	64	59	W by S		Showery	
		Even....	29.382	60	64	SW		Cloudy	67
	6	Morn....	29.252	59	91	SW by S		Rain	} 56 $\frac{1}{2}$
		Noon....	29.150	62	88	SSW		Rain	
Even....		29.147	60	73	SSW		Cloudy	65 $\frac{1}{2}$	
7	Morn....	29.277	61	71	W by N		Cloudy	} 55 $\frac{3}{4}$	
	Noon....	29.373	63	63	WNW		Showery		
	Even....	29.486	60	64	NW by W		Fine	66	
8	Morn....	29.528	57	77	SSW		Fine	} 50 $\frac{3}{4}$	
	Noon....	—	—	—	—		—		
	Even....	29.371	61	62	SSW		Fine	67	
9	Morn....	29.500	61	65	WSW		Fine	} 55 $\frac{1}{2}$	
	Noon....	29.568	69	50	W		Fine		
	Even....	29.680	61	55	W		Fine	69 $\frac{1}{2}$	
10	Morn....	29.813	58	76	W		Fine	} 52	
	Noon....	29.830	71	54	WNW		Fine		
	Even....	29.820	65	63	SSW		Fine	72 $\frac{1}{2}$	
11	Morn....	29.838	62	73	N		Very fine	} 58	
	Noon....	29.820	71	58	W by S		Fine		
	Even....	—	—	—	—		—	73 $\frac{1}{4}$	
12	Morn....	29.740	62	63	WNW		Clear	} 55	
	Noon....	29.736	68	54	Var.		Fine		
	Even....	—	—	—	—		—	71	
13	Morn....	29.664	60	63	NNE		Fine	} 54	
	Noon....	29.623	69	52	Var.		Very fine		
	Even....	—	—	—	—		—	71	
14	Morn....	29.540	62	66	WSW		Very fine	} 55 $\frac{1}{3}$	
	Noon....	29.508	72	47	SSW		Very fine		
	Even....	29.478	64	52	SSW		Fine	75	
15	Morn....	29.430	60	73	SW by S		Cloudy	} 56	
	Noon....	29.418	71	59	SW by W		Cloudy		
	Even....	29.362	65	63	SW by W		Cloudy	76	
16	Morn....	29.340	65	75	SW		Rain	} 61	
	Noon....	29.347	71	61	W by S		Cloudy		
	Even....	—	—	—	—		—	72 $\frac{1}{2}$	
17	Morn....	29.346	66	74	SW		Showery	} 63	
	Noon....	29.332	67	70	WSW		Cloudy		
	Even....	29.332	65	65	NNW		Cloudy	71 $\frac{1}{2}$	
18	Morn....	29.400	60	65	NNE		Cloudy	} 55 $\frac{1}{2}$	
	Noon....	29.393	67	53	SW		Fine		
	Even....	29.340	62	58	WNW		Fine	69	

Month.	Time.	Barom.	Ther.	Hyg.	Wind.	Velocity.	Weather.	Six's.
Aug.		Inches.				Feet.		
19	Morn....	29·268	58°	62°	NE		Fine	51°
	Noon....	29·264	60	62	W		Rain, th.	63 $\frac{2}{3}$
	Even....	29·278	55	73	NW by W		Very fine	47 $\frac{1}{2}$
20	Morn....	29·400	55	71	NW		Very fine	47 $\frac{1}{2}$
	Noon....	29·409	60	56	Var.		Fine	64 $\frac{1}{3}$
	Even....	29·410	57	63	SSE		Fine	51 $\frac{1}{2}$
21	Morn....	29·375	54	51	E by S		Rain	51 $\frac{1}{2}$
	Noon....	29·344	58	70	E		Showery	58 $\frac{1}{2}$
	Even....	29·325	—	85	E by N		Rain	50
22	Morn....	29·329	55	64	NE		Cloudy	50
	Noon....	29·357	60	55	NE		Cloudy	60 $\frac{1}{2}$
	Even....	—	—	—	—		—	46 $\frac{1}{2}$
23	Morn....	29·604	53	66	N by E		Cloudy	46 $\frac{1}{2}$
	Noon....	29·653	59	58	NNE		Cloudy	60
	Even....	29·689	58	63	NE		Cloudy	50
24	Morn....	29·700	—	62	SW		Fine	50
	Noon....	—	—	—	—		—	—
	Even....	—	—	—	—		—	—
29	Morn....	29·179	54	74	WSW		Fine	—
	Noon....	29·228	61	55	W by N		Fine	64
	Even....	29·260	56	61	WNW		Fine	47 $\frac{1}{3}$
30	Morn....	29·428	54	67	WNW		Clear	47 $\frac{1}{3}$
	Noon....	29·454	62	52	Var.		Cloudy	64
	Even....	29·500	—	77	N		Rain	47 $\frac{1}{2}$
31	Morn....	29·600	54	74	NE by E		Fine	47 $\frac{1}{2}$
	Noon....	29·604	55	84	NE		Hail, th.	59 $\frac{3}{4}$
	Even....	29·610	52	72	E by N		Very fine	59 $\frac{3}{4}$

Rain, by the pluviometer, between noon the 1st of Aug. and noon the 1st of Sept. 1·992 inch. Evaporation, during the same period, 4·820 inches.

ARTICLE XIV.

METEOROLOGICAL TABLE.

1820.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.	Hygr. at 9 a. m.
		Max.	Min.	Max.	Min.			
8th Mo.								
Aug. 1	S W	30·11	29·88	77	53	—		61
2	W	30·11	30·01	77	58	—		58
3	S W	30·01	29·80	75	63	45	20	64
4	S W	29·85	29·80	76	54	—	08	88
5	W	29·86	29·76	75	57	—	13	62
6	S W	29·76	29·75	69	57	35	16	74
7	W	29·96	29·76	69	58	—	18	64
8	S W	29·96	29·92	71	56	34		61
9	W	30·30	29·96	74	48	—		61
10	N W	30·29	30·26	78	50	—		65
11	N W	30·29	30·21	76	50	36		64
12	N	30·21	30·14	75	49	—		60
13	N	30·14	30·03	74	49	—		57
14	S W	30·03	29·93	79	51	36		59
15	S W	29·93	29·84	81	61	—		60
16	S W	29·84	29·84	78	62	—		67
17	S W	29·87	29·84	75	51	37		66
18	N W	29·87	29·78	73	50	—		58
19	N E	29·87	29·78	69	41	—	—	57
20	N W	29·87	29·85	69	43	—	14	56
21	N E	29·85	29·85	61	51	32	52	71
22	N E	30·08	29·85	69	47	—	—	64
23	N E	30·19	30·08	64	46	—		60
24	S W	30·19	29·96	70	48	—		54
25	S W	29·96	29·75	68	55	40	19	68
26	S W	29·76	29·72	70	47	—	08	64
27	N W	29·76	29·56	68	52	—	14	59
28	S W	29·68	29·55	67	46	39		62
29	S W	29·89	29·68	71	38	—		62
30	N	30·08	29·89	70	42	20		60
31	N E	30·15	30·08	72	41	08		62
		30·30	29·55	81	38	3·62	1·82	88—54

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Eighth Month.—1, 2. Fine. 3. Cloudy. 4. *Cirrus: Cirrocumulus:* showery morning. 5. Fine. 6, 7. Showery. 8—14. Fine, with the usual superior modifications of cloud. 15. Thunder clouds. 16—18. Cloudy. 19. A thunder storm at two, p.m. 21. Showery morning: wet day. 22—26. Cloudy. 27. *Cirrus, Cirrocumulus:* windy: rain by night. 28, 29. Fine. 30. Some lightning in the evening. 31. *Cirrocumulus.*

RESULTS.

Winds: N, 3; NE, 5; E, 0; SE, 0; S, 0; SW, 14; W, 4; NW, 5.

Barometer: Mean height

For the month.	29.932 inches.
For the lunar period, ending the 29th	29.924
For 14 days, ending the 11th (moon north)	29.978
For 14 days, ending the 25th (moon south)	29.943

Thermometer: Mean height

For the month.	61.515°
For the lunar period, ending the 29th	61.879
For 31 days, the sun in Leo.	63.741

Evaporation. 3.62 incl.

Rain 1.82

Mean of hygrometer 62°

On the night preceding the *Solar Eclipse* on the 7th of the ninth month, the minimum temperature at Tottenham was 41°, and during the eclipse, the register thermometer there made a retrograde movement from 71° to 63°.

The morning was very misty; temp. at 30 min. after five, 42°; barometer, 30.02 inches; vane at N E, calm air: at 20 min. after seven, temperature 50°; barometer, 30.05 inches: at 30 min. after nine, temperature 65°; barometer, 30.05 inches. Wind, now S E, with *Cirri* above, and *Cumuli* appearing beneath them.

ANNALS
OF
PHILOSOPHY.

NOVEMBER, 1820.

ARTICLE I.

Description of Chromate and Bichromate of Potash.
By Thomas Thomson; M.D. F.R.S.

SINCE the original discovery of chromic acid by Vauquelin scarcely any addition has been made to our knowledge of the salts which it forms, except a few very short notices respecting them by John, and the analysis of a single chromate by Berzelius. Indeed the reserve with which they have been mentioned by systematic writers will not surprise practical chemists, who are aware of the great difficulty of preparing the chromates in a state of purity, in consequence of the disposition which they have to enter into triple compounds. I have repeatedly turned my attention to these salts, and have been at considerable pains to obtain them pure, and to determine their properties and their composition. I propose at present to confine myself to an account of the salts which chromic acid forms with potash; because these salts constituted the materials out of which all the other chromates were formed.

To obtain chromate of potash, I employed the method recommended by Vauquelin many years ago. Chromate of iron was reduced to powder, and heated for several hours in a crucible mixed with half its weight of saltpetre. The mass was then digested in water, and the same process was repeated with the undissolved residuum. The yellow-coloured liquids thus obtained were slightly alkaline. They were saturated with nitric acid, and concentrated, till no more crystals of saltpetre could be obtained from them. The yellow liquid was now set aside for

some weeks in a dry place. A copious crop of yellow-coloured crystals was obtained in small needles. These were separated, redissolved in water, and crystallized again. By this means crystals of chromate of potash were procured, which, if not absolutely pure, were very nearly so. Dissolve any quantity of these crystals in water, add to the solution such a quantity of sulphuric acid that the liquid will have a pretty sour taste. Set the liquid aside for 24 hours, crystals of bichromate of potash will be deposited in small regular needles. I shall now describe the properties of these two salts.

1. *Chromate of Potash.*—The crystals are usually four-sided slender prisms terminated by dihedral summits; but by slow crystallization they may be obtained in large oblique four-sided prisms terminated at both extremities by four-sided pyramids. The faces of the prism make with each other angles of 110° and 70° . The inclination of a face of the terminal pyramid to the corresponding face of the prism is about 130° . These measurements are to be considered only as approximations; for the faces of all the crystals which I examined were too rough to admit of the application of the reflecting goniometer.

The crystals are not in the least altered by exposure to the open air. Their colour is an intense lemon-yellow with a slight shade of orange. The colouring power of this salt is very great. One grain of it dissolved in 40,000 grs. of water forms a solution which has a very perceptible yellow colour. If we dissolve 1 gr. of chromate of potash and 20 grs. of saltpetre in water, and evaporate the solution, we obtain crystals of saltpetre almost as deep-coloured as chromate of potash itself.

Its taste is cooling, and bitter, and very disagreeable. It continues very long in the mouth. I put into my mouth a small crystal to taste it. Two days elapsed before my mouth lost the disagreeable impression. Indeed for more than 24 hours, every article taken as food appeared to have the very taste of the salt itself.

It is exceedingly soluble in water. Boiling water indeed seems capable of dissolving almost any quantity of it: 100 grs. of water saturated with it at the temperature of 60° leave, when evaporated to dryness, 32.6 grs. of dry chromate; so that 100 grs. of water at the temperature of 60° dissolve 48.368 grs. of this salt. The consequence of this great solubility is a considerable difficulty in obtaining the chromate of potash in regular crystals.

It is insoluble in alcohol. Its specific gravity is 2.6115. It undergoes no change whatever, though kept ever so long, in the temperature of 400° . When exposed to a red heat, it acquires a beautiful crimson colour; but on cooling becomes of a very intense lemon-yellow. It cannot be fused by the greatest heat of a common fire. The fusion may be accomplished by means of a wind furnace; but in that case, the salt acquires a green

colour, and the acid appears to be deprived of a portion of its oxygen. One hundred grains of chromate of potash when kept for half an hour in a red heat lose 3·2 grs. of their weight. This loss is probably owing to water, which does not seem chemically combined, but only mechanically mixed with the layers of which the crystals are composed.

The following table exhibits the effects produced by a pretty strong solution of chromate of potash in water when mixed with solutions of different salts.

1. *Sulphate of Copper*.—A reddish-brown precipitate.
2. *Sulphate of Zinc*.—Light-yellowish flocks appear at first redissolved by agitation; but on adding more chromate of potash, they become permanent.
3. *Muriate of Manganese*.—Colour becomes darker and redder; but no immediate precipitate falls. After an interval of 24 hours, a black sediment makes its appearance.
4. *Nitrate of Mercury*.—A fine red precipitate. Scarlet, when dry.
5. *Pernitrate of Iron*.—Becomes nearly the colour of muriate of platinum; but no precipitate appears after two days.
6. *Protosulphate of Iron*.—A copious brown flocky precipitate like that from sulphate of copper.
7. *Nitromuriate of Gold*.—No change after standing two days.
8. *Nitrate of Silver*.—A copious dark-red precipitate. Brown, when dry.
9. *Muriate of Platinum*.—A copious dark-red precipitate, similar to that from nitrate of silver. Brick-red, when dry.
10. *Soda-muriate of Rhodium*.—0.
11. *Sulphate of Nickel*.—0.
12. *Sulphate of Cobalt*.—0.
13. *Muriate of Antimony, with Excess of Acid*.—A dark-green precipitate, redissolved on agitation. The liquid assumes a fine green colour.
14. *Hydrosulphuret of Soda-and-Antimony*.—A precipitate in yellow flocks.
15. *Tartar Emetic*.—No immediate change; but a slight precipitate in 24 hours. The liquid assumes a dirty-greenish yellow colour.
16. *Nitrate of Bismuth*.—A white precipitate redissolved by agitation. A copious yellow precipitate on the addition of water.
17. *Muriate of Tin*.—A very copious flocky precipitate of a yellowish-brown colour. Black, when dry.
18. *Nitrate of Barytes*.—A copious yellow precipitate.
19. *Muriate of Strontian*.—Ditto, rather darker coloured.
20. *Nitrate of Lime*.—A slight yellow precipitate, which gradually increases, and adheres to the sides and bottom of the vessel.
21. *Lime Water*.—0

22. *Sulphate of Magnesia*.—0.
 23. *Aluræ*.—A copious white precipitate.
 24. *Muriate of Alumina*.—A copious gelatinous precipitate, which speedily diminishes in bulk, and becomes crystalline.
 25. *Prussiate of Potash*.—0.
 26. *Tincture of Nutgalls*.—A very copious chocolate-brown precipitate. Becomes dirty-orange, when dried.
 27. *Hydriodate of Zinc*.—A copious yellow precipitate.
 28. *Alcohol*.—The chromate of potash is immediately precipitated in small crystals.

Such are the properties of this salt as far as I have determined them. To analyze it, I find the best reagent is nitrate of lead, which it is well known throws down the chromic acid united to the oxide of lead in the form of an intensely orange powder. Nitrate of mercury indeed is fully as sensible. I find that when 1 gr. of chromate of potash is dissolved in 40,000 grs. of water, a very considerable precipitate is obtained when we drop into the solution either nitrate of mercury, or nitrate of lead. When the solution of the chromate of potash is so much diluted, the precipitate by nitrate of mercury is reddish-yellow, and that by nitrate of lead greenish-yellow. Nitrate of silver is a less sensible reagent for discovering chromic acid; for it occasions no precipitate in a solution of 1 gr. of chromate of potash in 40,000 grs. of water; but 1 gr. of the chromate dissolved in 20,000 grs. of water may be detected by this reagent. Nitrate of barytes is about as sensible as nitrate of silver, producing like it a precipitate in a solution of 1 gr. of chromate of potash in 20,000 grs. of water. While this solution is neither acted upon by nitrate of lime, nor muriate of strontian. I have tried to analyze chromate of potash both by means of nitrate of lead, nitrate of mercury, and nitrate of barytes; but the results obtained by means of nitrate of lead were on the whole the most satisfactory. The method which I found to answer best was the following:

Expose the chromate of potash to be subjected to analysis to a red heat. Then weigh out 20 grs. of it, and dissolve them in distilled water. Drop into this solution a solution of crystallized nitrate of lead in water as long as any precipitate continues to fall. Separate this precipitate, wash it well, dry it, and then expose it for an hour to a heat of 400° . Then weigh it; $\frac{1}{4}\frac{2}{7}$, or 0.317, of this precipitate is chromic acid. Therefore multiply the weight of the precipitate by 0.317, the product indicates the weight of chromic acid in 20 grs. of chromate of potash. If the experiment be rightly performed, the chromate of lead from 20 grs. of chromate of potash will weigh 32.8 gr. Now $\frac{1}{4}\frac{2}{7}$ of 32.8 is 10.4. Thus the chromic acid in 20 grs. of chromate of potash is 10.4 gr.; therefore 100 grs. of the salt must contain $10.4 \times 5 = 52$ grs. The only other constituent being potash, it is obvious that chromate of potash is composed of

Chromic acid	52	or 6·5
Potash	48	6·0
	100	

If any of my readers wish to repeat this analysis, it will be necessary for him to make sure of the purity of his chromate of potash, which is by no means easy. I repeated the analysis four times with crystals of chromate of potash which I had prepared on purpose; but the results never agreed exactly with each other. Nor was I able to obtain constant results till I formed the chromate of potash from the bichromate, by dissolving that salt in water, and adding with infinite care exactly the quantity of potash necessary to neutralize the chromic acid. How I ascertained that the exact weight of an atom of chromic acid is 6·5, and the exact composition of chromate of potash, I shall explain in a paper which will be found immediately after the present. The introduction of the necessary details would lead us too far at present from the subject in hand.

The only other person who has made experiments, as far as I know, to determine the weight of an atom of chromic acid is Prof. Berzelius. According to him, an atom of chromium weighs 7·0364, and an atom of chromic acid 39·1092. These numbers differ enormously from mine; but the composition of chromate of potash resulting from his method does not differ much from the analysis which I have just given. According to Berzelius, it is composed of

Chromic acid.	52·49
Potash	47·51
	100·00

I shall demonstrate in the subsequent article, I trust, to the satisfaction of chemists, that the numbers which I have given are the true ones. I think it not unlikely that Berzelius failed in consequence of the presence of a small quantity of saltpetre remaining mixed or united to his chromate of potash. This at least was the case with the chromate of potash which I attempted to analyze when I began my experiments on this salt.

2. *Bichromate of Potash.*—This is perhaps the most beautiful salt which chemists have hitherto discovered. It has an intense orange-red colour, and crystallizes sometimes in large rectangular or square tables, with bevelled edges, the inclination of the bevelled faces to the corresponding face of the prism about 120°, sometimes in flat rectangular four-sided prisms, terminated in some cases by a face perpendicular to the axis of the prism, in some cases by a four-sided pyramid, and in others by a dihedral summit. I have obtained the tables nearly an inch in length,

but the prisms were not large enough to allow the measurement of the angles of the terminal faces. The crystals are not altered by exposure to the air.

Its taste is cooling, bitter, and metallic, and it leaves a hot impression in the mouth, which, however, does not last nearly so long as the taste of the chromate of potash.

It is much less soluble in water than the chromate. Fifty-five grains of water, saturated with it at the temperature of 63° , left, when evaporated to dryness, 5.2 grs. of bichromate. We see from this that at the temperature of 63° , 100 grs. of water are capable of dissolving 10.44 grs. of this salt. Chromate of potash is more than four times as soluble. Thus the bichromate of potash agrees with bitartrate and binoxalate of potash in being much less soluble than the corresponding salt composed of an atom of acid and an atom of potash.

The aqueous solution of bichromate of potash has an intense orange colour, similar to chromate of lead. It reddens vegetable blue colours.

It is insoluble in alcohol. Its specific gravity is 1.9801.

When 100 grs. of the dry crystals were kept for an hour at the temperature of 400° , they sustained a loss of weight amounting to six grains. When the salt thus dried was exposed to a red heat, it melted into a transparent red liquid, which, on cooling, concreted into a semitransparent red salt, without undergoing any additional loss of weight.

The action of this salt upon the different reagents is almost the same as that of the chromate, making allowance for its inferior solubility in water. It is, therefore, unnecessary to enter into particulars.

I have given this salt the name of bichromate, because I have ascertained by experiment that it is a compound of two atoms of chromic acid with one atom of potash, while the chromate is a compound of one atom acid + one atom potash.

Take 19 grs. of bichromate previously exposed to a red heat, dissolve them in water, and precipitate by nitrate of lead. The chromate of lead obtained, after being washed, dried, and exposed to a heat of 400° , will be found to weigh 41 grs. Now $\frac{4}{4}$ of 41 is obviously 13. It follows from this, that 19 grs. of bichromate of potash contain 13 grs. of chromic acid. The residue is potash. Hence the salt is composed of

Chromic acid.	13	or	68.421
Potash	6		31.579
	19		100.000

If the reader will look back to the table exhibiting the composition of chromate of potash, he will see that it is a compound of 6.5 acid + 6 potash; while the bichromate is a compound of

13 acid + 6 potash. Thus the weight of potash in each being the same, the weight of acid in the bichromate is precisely double that in the chromate. The analysis of the bichromate is much easier than that of the chromate, because it is easy to procure the salt pure.

ARTICLE II.

Experiments to determine the true Weight of the Atoms of Barytes, Potash, Soda, Lead, Sulphuric Acid, Nitric Acid, Muriatic Acid, and Chromic Acid. By Thomas Thomson, M.D. F.R.S.

IN my paper on the Specific Gravity of the Gases, inserted in the last number of the *Annals of Philosophy*, p. 241, I stated that it was impossible for Prof. Berzelius ever to deduce the true composition of sulphuric acid from his analysis of the sulphates, because the numbers which he assigns to the constituents of sulphate of barytes, by means of which all his analyses are calculated, are not quite accurate. I am aware that in consequence of the high character which Berzelius bears for precision, and which he in fact so richly deserves, such an allegation on my part will not, and indeed ought not, to be admitted unless its truth be demonstrated by experiments of the most decisive nature. I consider it as necessary, therefore, to lay before my readers the reasons upon which my opinions are founded with as little delay as possible; and I have no doubt that after I have stated them, and after the experiments have been repeated and confirmed by others, as they undoubtedly will be, every practical chemist, who turns his attention to the subject, and even Berzelius himself, will admit that the numbers, or at least the ratios, which I shall assign to the weights of the atoms of the substances enumerated at the head of this essay are mathematically accurate, or at least that the error, if any exist, is wholly unappreciable.

That chemists are still far from being agreed about the atomic weights of these important bodies must be well known to every person at all interested in the subject. Determinations of them have been published by Dalton, Wollaston, Berzelius, and myself, and perhaps also by others whose results I do not at present recollect. The following little table exhibits the weights of the respective atoms according to these various chemists. To enable the reader to compare them together, I have reduced them to what they would be on the supposition that the weight of an atom of oxygen is unity; because this is the mode which I myself am accustomed to employ.

	Dalton.	Wollaston.	Berzelius.	Thomson.
Barytes	9·7128	9·7	19·1386	9·75
Potash.	6·000	5·91	11·7983	6·0
Soda.	4·000	3·91	7·8184	4·0
Protoxide of lead.	14·571?	14·00	27·8900	14·0
Sulphuric acid. . .	4·8571	5·00	5·0116	5·0
Nitric acid	2·7128	6·754	6·7726	6·75
Muriatic acid. . .	3·1285	4·542	3·4265	4·625
Chromic acid. . .	—	—	13·0364	6·5

Berzelius's numbers appear to differ more from those of Dr. Wollaston and my own than is really the case; because both Dr. Wollaston and myself have expressed the atomic ratios in the lowest terms or nearly so; while Berzelius, from certain notions which he entertains respecting the number of atoms of oxygen which unite with the respective bases, has very frequently raised his ratios to higher numbers than would be otherwise requisite. It is not yet quite time to enter into a discussion respecting Berzelius's opinions on this subject, though the period is fast approaching when such a discussion would be attended with advantage. Mean while I may just observe that all which we know, or ever can know, respecting the weights of the atoms of bodies, is the ratio which they bear to each other. Now every person in the least conversant with numbers must see that the lower the terms in which these ratios are expressed so much the more advantageous are they both for the memory and for comparing them with each other. On this account, I think, it would have been better if both Wollaston and Berzelius had represented the weight of an atom of oxygen by unity, as I have done, than by 10, or 100, the numbers which they have respectively employed. I find at least that I can remember the small numbers which I have affixed to the atoms of bodies more easily than those given either by Dr. Wollaston, or by Professor Berzelius.

When the numbers of Berzelius are brought to their lowest terms, they are as follows:

Barytes	9·5693
Potash.	5·89915
Soda.	3·9092
Protoxide of lead.	13·9450
Sulphuric acid.	5·0116
Nitric acid.	6·7726
Muriatic acid.	3·4265
Chromic acid.	6·5182

Now these numbers, though not one of them be absolutely the same as mine, do not deviate very far from them. The atomic

weights which I have given as my own are those which will be found in the fifth edition of my System of Chemistry, where I have stated the reasons which induced me to pitch upon them. The reader will perceive that they are all multiples of 0.125, the weight of an atom of hydrogen. Dr. Prout was the first person who pointed out the important fact that the weights of the atoms of all bodies are multiples of the weight of an atom of hydrogen.* The greater number of the weights which he assigned in his important paper are the same, or nearly the same, with those which I have given as my own. This coincidence was the more gratifying to me, because I had determined upon the numbers above assigned before I was aware of his conclusions. But the general law, that the *weights of the atoms of all bodies are multiples of the atom of hydrogen*, belongs exclusively to Dr. Prout. I consider it as the third great step in our investigation of the atomic theory, and I have not a doubt that it will hereafter prove most important and most prolific. I adopted it in the fifth edition of my System of Chemistry, and was enabled by my knowledge of it to come much nearer the true numbers than I otherwise could have done.

The greater number of chemical writers in this country have adopted the atomic weights assigned by Dr. Wollaston. This was the natural consequence of the introduction of his scale of chemical equivalents, which must be found so useful in every laboratory. The weights assigned in that scale are certainly in general very near the truth; but as very few of them are absolutely correct, they who employ that scale will fall into small errors, of no consequence in many cases, but sufficiently great to be very perceptible, when experiments of the very delicate nature that distinguish those which constitute the subject of the present paper are to be tried.

Let us now compare the atomic weights as assigned by Dalton, Wollaston, and Berzelius, with mine. I reckon the weight of an atom of barytes 9.75. Mr. Dalton's weight, 9.7128, comes nearest this number; but it is less. Dr. Wollaston's, 9.7, is still less; while that of Berzelius, 9.5693, is the least of all. My number is a multiple of the atom of hydrogen, for $0.125 \times 78 = 9.75$; but neither the number of Dalton, Wollaston, nor Berzelius, is a multiple of 0.125. My number is easily recalled to memory; but it would not be an easy task to remember such complicated numbers as that of Berzelius.

My number for sulphuric acid agrees exactly with that of Dr. Wollaston, both being 5. That of Berzelius is only a very little greater; namely, 5.0116; while that of Dalton is a very little less, being 4.8571. My number is a multiple of 0.125; for $0.125 \times 40 = 5$; while neither the number of Berzelius nor of

* *Annals of Philosophy*, vol. vi. p. 321.

Dalton is a multiple of 0.125, and both are too complex numbers to be easily recalled to memory.

My number for the atom of potash is 6. Mr. Dalton's number exactly agrees with mine; while both Wollaston's and Berzelius's are a very little less, and very nearly the same with each other: 6 is a multiple of 0.125, for $0.125 \times 48 = 6$; but neither the numbers of Wollaston nor of Berzelius are multiples of 0.125. Berzelius's number is too long to be easily remembered.

My number for the atom of muriatic acid is 4.625; for I consider it to be demonstrated that it is composed of 1 atom of chlorine = 4.5 + 1 atom of hydrogen = 0.125, making together 4.625. The reasons for adopting these weights have been given in a preceding essay on the specific gravity of the gases, and they are such that I conceive no competent judge can entertain any doubts respecting their accuracy. It would be needless to compare the numbers of Dalton or Berzelius with mine. They are derived from quite different principles, very complicated, and not multiples of 0.125. Dr. Wollaston's number is only a little less than mine; but neither is it a multiple of 0.125. My number is obviously a multiple, for $0.125 \times 37 = 4.625$.

I propose now to demonstrate by an unexceptionable experiment, which any person may repeat with the greatest ease, that my numbers represent exactly the ratios of the weights of the atoms of these four bodies.

1 atom sulphuric acid	= 5
1 atom potash.	= 6
<hr style="width: 10%; margin-left: auto; margin-right: 0;"/>	
1 atom of sulphate of potash.	= 11
1 atom barytes.	= 9.75
1 atom muriatic acid	= 4.625
<hr style="width: 10%; margin-left: auto; margin-right: 0;"/>	
1 atom muriate of barytes	= 14.375

When crystallized muriate of barytes is exposed to a red heat in a platinum crucible, it loses all its water of crystallization, while at the same time the hydrogen of the muriatic acid unites with the oxygen of the barytes, and flies off in the state of water. The consequence is, that the muriate of barytes is changed into chloride of barium. Now barytes is composed of

Barium	8.75
Oxygen.	1.00
<hr style="width: 10%; margin-left: auto; margin-right: 0;"/>	
9.75	

And muriatic acid of

Chlorine.	4.5
Hydrogen.	0.125
<hr style="width: 10%; margin-left: auto; margin-right: 0;"/>	
4.625	

An atom of chloride of barium, therefore, weighs 13·25; and 13·25 of chloride of barium are always equivalent to 14·375 of muriate of barytes;* because, whenever the chloride of barium is dissolved in water, it is converted into muriate of barytes. These things being premised, I proceed to the experiment.

Take any quantity of pure crystals of sulphate of potash and of muriate of barytes, expose them in two separate platinum crucibles to a red heat, which must be continued till both salts are completely deprived of all their water. Then dissolve 11 grs. of sulphate of potash in distilled water. Dissolve likewise 13·25 grs. of the chloride of barium in another separate portion of distilled water. Now mix the two liquids together, and agitate them well. Immediately the sulphuric acid unites with the barytes, and the sulphate of barytes gradually precipitates to the bottom. When the liquid has become transparent, draw it off with a sucker, and divide it into two portions. Into one of the portions drop a little nitrate of barytes, and into the other a little sulphate of soda. No precipitate will appear in either portion of the liquid, if the experiment has been rightly performed. I say, therefore, that the liquid contains no traces either of sulphuric acid, or of barytes. Consequently the sulphuric acid in 11 grs. of sulphate of potash is just capable of neutralizing the quantity of barytes in 13·25 grs. of chloride of barium, or 14·375 grs. of muriate of barytes. Whoever repeats this experiment must be sure that the salts which he employs are perfectly pure and perfectly dry. The respective weights of each must be very exactly those indicated above, and special care must be taken that not a drop of either solution is lost, and that the two liquids are intimately mixed. I say then that I am entitled from this experiment to conclude, that the weights of the atoms of potash,

* Those chemists who adopt the opinions of Berzelius and the late Dr. Murray, respecting muriatic acid, may state the numbers in the text in the following manner:

The weight of an atom of muriatic acid gas is 4·625. It is composed of an atom of muriatic acid and an atom of water.

Weight of an atom of muriatic acid	3·5
water	1·125
	4·625
Atom of muriatic acid gas	= 4·625

When muriatic acid gas unites to barytes, it loses its water. Hence muriate of barytes is a compound of

Muriatic acid	3·50
Barytes	9·75
	13·25
Atom of muriate of barytes	= 13·25

The only difference between this statement and that in the text is in the opinion entertained respecting the atom of water which escapes. Berzelius considers it as previously united to the muriatic acid, while those chemists that consider chlorine as a simple substance believe that the water is derived from the mutual decomposition of the muriatic acid and the barytes. The weights in both cases are the same, nothing differs but the names.

barytes, sulphuric acid, and muriatic acid, are exactly those which I have assigned.

If the practical chemist wishes now to ascertain whether the numbers assigned by Berzelius for the weight of the atoms of these bodies be correct, he may make the experiment as follows:

1 atom of sulphuric acid.	=	5.0116
1 atom potash.	=	5.89915

Atom of sulphate of potash.	=	10.91075
1 atom muriatic acid.	=	3.4265
1 atom barytes.	=	9.5693

Atom of muriate of barytes.	=	12.9958

Let him, therefore, dissolve in distilled water 10.91075 grs. of dry sulphate of potash, and 12.9958 grs. of chloride of barium. If he examine the liquid after the sulphate of barytes has subsided, he will find that it still contains sulphuric acid; for it will be precipitated by nitrate of barytes. I say then that the weights assigned by Berzelius are inaccurate. It is obvious that the atom of sulphuric acid is either too high, or that of barytes too low. By comparing his numbers with mine, the reader will see that the weight of the sulphuric acid is a little too high, and that of the barytes a little too low.

It is obvious from the preceding experiment that the true composition of sulphate of barytes is

Sulphuric acid.	5.00	or	33.8984
Barytes.	9.75		66.1016
	-----		-----
	14.75		100.0000

And this is the constitution of the salt which must hereafter be employed to determine the proportion of sulphuric acid in any sulphate.

The atomic weights of Dalton and Wollaston may be tried precisely in the same way. It will be found that the liquid left after both will be precipitated, indicating clearly that their numbers are not quite accurate; but the precipitate which appears when we take Dr. Wollaston's weights is much smaller than with either those of Berzelius or Dalton, indicating that his numbers are nearest the truth of the three.

It is obvious from the preceding experiment that the weight of an atom of potash is exactly 6. In the fifth edition of my System of Chemistry, I showed that the mean of the different experiments to determine the composition of potash comes exceedingly near this number—so near indeed that nobody could hesitate to adopt it as the true number. I have now demonstrated that it is absolutely true; for the error which can

escape the test of the experiment which I have now stated must be very small indeed.

I shall now show by another similar experiment that the weight of an atom of soda is precisely 4.

1 atom of sulphuric acid	= 5
1 atom soda	= 4
	—
1 atom anhydrous sulphate of soda. =	9
1 atom chloride of barium.	= 13·25

Take pure crystals of sulphate of soda, dry them in a platinum crucible on the sand bath, then expose the dry residue to a red heat for half an hour. Dissolve in distilled water 9 grs. of this dry salt. Dissolve in another portion of distilled water 13·25 grs. of dry chloride of barium. Mix the two solutions well together. When the sulphate of barytes has fallen to the bottom draw off the clear liquid by means of a sucker, and examine it, for barytes and sulphuric acid, in the manner stated above. No traces of either substance can be found in it. It is clear, therefore, that 9 grs. of anhydrous sulphate of soda contain exactly as much sulphuric acid as 11 grs. of sulphate of potash. Both contain precisely 5 grs. of true sulphuric acid; consequently if an atom of potash weigh 6, an atom of soda must weigh 4. It will be seen by consulting the fifth edition of my *System of Chemistry*, that the number 4 is derived very nearly from the different experiments undertaken to determine the composition of soda.

If we apply this experimental method to the examination of the atomic weights given by Dalton, Wollaston, and Berzelius, we shall find that none of them are exact.

I have not been able to succeed in these experiments when I used crystals of sulphate of soda. From the analysis of these crystals by Berzelius, it follows that they are composed of 1 atom sulphuric acid, 1 atom soda, and 10 atoms water. Now

1 atom sulphuric acid.	= 5·00
1 atom soda.	= 4·00
10 atoms water.	= 11·25
	—
	20·25

So that 20 $\frac{1}{4}$ grs. of crystals of sulphate of soda ought to be exactly equivalent to 9 grs. of anhydrous sulphate. But when we mix together a solution of 20·25 grs. of crystals of sulphate of soda and 13·25 grs. of chloride of barium, and examine the clear liquid after the separation of the sulphate of barytes, we always find that it contains an excess of barytes. It is obvious from this that the crystals of sulphate of soda contain more than 10 atoms of water. I would have investigated the real quantity had I not been stopped by a circumstance which it does not

seem easy to obviate. I found the quantity of water in these crystals to vary somewhat in different specimens. Besides the water which enters as a chemical constituent into the salt, there seems to be some water lodged mechanically between the plates of the crystal, and this water I suppose may vary in quantity. But the best formed, and firmest, and most solid crystals of sulphate of soda that I have been able to procure contain more than 10 atoms of water united to an atom of the anhydrous salt.

Dr. Wollaston's number for the weight of an atom of protoxide of lead is the same as mine, while the weight assigned by Dalton is a little too high, and that of Berzelius a little too low. My number is a multiple of 0.125; for $0.125 \times 112 = 14$. This is not the case either with the numbers of Dalton, or of Berzelius, and Berzelius's number is so large and complicated that it cannot easily be remembered.

Dr. Wollaston's number for the atom of nitric acid differs very little from mine. The number of Berzelius is a little too high. As usual, it is complicated, difficult to remember, and not a multiple of 0.125. As to Mr. Dalton's number, it deviates so very far from the truth that it is not worth while to compare it with that of the other chemists.*

I shall now show by a simple and decisive experiment that my numbers both for nitric acid, and for protoxide of lead, are correct.

Nitrate of lead crystallizes in octahedrons, and when properly prepared, it contains no water of crystallization. It is composed of an atom of nitric acid united to an atom of protoxide of lead. Now

1 atom nitric acid	= 6.75
1 atom protoxide of lead	= 14.00
	—
1 atom nitrate of lead	= 20.75

We have seen already that sulphate of potash is composed of

1 atom sulphuric acid	= 5
1 atom potash	= 6
	—
	11

Dissolve in distilled water 20.75 grs. of pure and dry nitrate of lead, dissolve in another portion 11 grs. of perfectly dry sulphate of potash. Mix the two solutions together, and agitate them well.

Sulphate of lead speedily precipitates. After the liquid has become quite clear draw it off by a sucker, and divide it into

* It is proper to observe, however, that Mr. Dalton conceives the nitrates to be compounds of two atoms of nitric acid with one atom of base. In reality, therefore, we ought to double the number which he assigns for the weight of an atom of nitric acid. This would make it 5.4256, a number which is still a great deal too low, though much nearer the truth than the number in the table.

two portions. Into one portion drop a little nitrate of lead; into the other a little sulphate of soda. The liquid will not be affected by either of these reagents. Hence it is obvious that it contains no sulphuric acid, and no oxide of lead. Consequently the two salts when mixed in the proportions above indicated exactly decompose each other. We see that sulphate of lead is composed of

Sulphuric acid.....	5
Protoxide of lead.....	14
	19

And nitrate of potash of

Nitric acid.....	6.75
Potash.....	6.00
	12.75

If we take the proportions indicated by Berzelius, the liquid will contain an excess of sulphuric acid. Dr. Wollaston's proportions will leave an excess of oxide of lead. It is obvious then that the weight of an atom of lead is 13, and the weight of an atom of protoxide of lead 14. Consequently all the analyses which Berzelius has made by means of salts of lead are somewhat inaccurate. The degree of error may be easily estimated from what has been said above.

The only remaining atomic weight in the little table given near the beginning of this essay which requires to be verified is that of chromic acid. In a preceding paper I have described the methods which I employed to prepare chromate and bichromate of potash; but I have reserved for this place a detail of the experiments by means of which the true weight of an atom of chromic acid and the true composition of the chromates was ascertained. The only person who has attempted before me to determine the weight of this acid is Prof. Berzelius; his number when reduced by dividing by 2 is 6.5182, which is only a very little greater than 6.5, the number which I pitched upon in consequence of the near approach of Berzelius's number to that quantity. 6.5182 is not only a number difficult to remember, but it is not a multiple of 0.125; whereas 6.5 is obviously a multiple; for $0.125 \times 52 = 6.5$.

Bichromate of potash is a compound of two atoms chromic acid and one atom potash.

2 atoms chromic acid.....	= 13
1 atom potash.....	= 6
	19

Chromate of lead is a compound of

1 atom chromic acid.	=	6.5
1 atom oxide of lead.	=	14.0
		<hr/>
1 atom chromate of lead	=	20.5

While nitrate of lead is composed of

1 atom nitric acid.	=	6.75
1 atom oxide of lead.	=	14.00
		<hr/>
1 atom nitrate of lead	=	20.75

It is obvious from this that in order to decompose completely one atom of bichromate of potash, we must employ two atoms of nitrate of lead. Dissolve, therefore, in distilled water 19 grs. of bichromate of potash which has been dried in a red heat. In another portion of distilled water dissolve 41.5 grs. of dry nitrate of lead. Mix the two solutions intimately together, and allow the chromate of lead to precipitate; then draw off the clear liquid with a sucker. It is perfectly colourless, and is neither precipitated by sulphate of soda, nor by nitrate of lead. Hence it is obvious that it contains neither chromic acid, nor oxide of lead. Consequently the chromic acid in 19 grs. of dry bichromate of potash is just capable of saturating the whole oxide of lead in 41.5 grs. of dry nitrate of lead. If the chromate of lead formed be dried in a temperature of about 400°, and weighed, its weight will be found to amount to

2 atoms chromic acid.	=	13	grs.
2 atoms of oxide of lead	=	28	
		<hr/>	
		41	

We see from this that chromate of lead is a compound of

1 atom chromic acid	=	6.5	or	31.7074
1 atom oxide of lead	=	14.0		68.2926
		<hr/>		<hr/>
		20.5		100.0000

And the weight of an atom of chromic acid is exactly 6.5

If the experiment be made by mixing the weights of nitrate of lead and bichromate of potash indicated by Berzelius's numbers for the weights of the respective atoms; namely, 41.4352 grs. of nitrate of lead and 18.93555 grs. of bichlorate of potash, we shall find that the liquid, after the whole of the chromate of lead has precipitated, still retains lead in solution—a plain proof that the atomic weights of Berzelius are not correct.

If we take 12.5 grs. of pure dry chromate of potash, and mix them with 20.75 grs. of dry and pure nitrate of lead, the result will be precisely the same. After the precipitation of the chromate of lead, the liquid will be colourless, and will contain no

traces either of chromic acid, or of oxide of lead. It deserves the attention of the practical chemist that I repeated this experiment a number of times before I could succeed. The liquid after the precipitation of the chromate of lead always contained a minute but variable quantity of oxide of lead. I discovered at last, though not without a good deal of trouble, that my chromate of potash was not absolutely free from nitrate of potash. Before I was perfectly successful in the experiment, I found it necessary to prepare pure chromate of potash by saturating a solution of bichromate of potash with potash, and then crystallizing the salt. With perfectly pure chromate of potash thus prepared, the experiment may be repeated without difficulty. It was in consequence of this very difficult separation of all the saltpetre from chromate of potash that I have preferred using the bichromate instead of the chromate. It is much easier to procure the former in a state of purity than the latter.

Unless I am mistaken in my conclusions altogether, I have demonstrated in this essay the true atomic weights of eight very important substances, and I have shown that neither the atomic weights of these bodies given by Dalton, Wollaston, nor Berzelius, are absolutely correct, or capable of standing the test which Richter long ago applied to the analyses of Kirwan, Wenzel, and Bergman. My numbers, on the contrary, stand that severe test perfectly well, and consequently are accurate.

I shall conclude this essay by laying these weights again before my readers, and with requesting them to endeavour to remember the respective numbers; because they indicate the weights of the different acids and bases which just saturate each other, and will be found of infinite value to the practical chemist.

	Weight of atom.
Barytes.	9.75
Potash.	6.00
Soda.	4.00
Protoxide of lead.	14.00
Sulphuric acid.	5.00
Nitric acid.	6.75
Muriatic acid.	4.625
Chromic acid.	6.5

Henceforth these numbers may be used with perfect confidence by practical chemists.

ARTICLE III.

Exposition of the Atomic Theory of Chemistry; and the Doctrine of Definite Proportions. By William James Macneven, M.D. Professor of Chemistry and Materia Medica in the College of Physicians and Surgeons of the University of the State of New-York.

(Concluded from p. 293.)

59. *Silver.*—According to Vauquelin, a protosulphuret of silver is composed of 100 metal + 14.59 sulphur. Hence the protoxide of silver must contain 7.29 oxygen nearly + 100 silver.

There is but one oxide of silver known, composed, according to Thomson, of 7.291 oxygen + 100 metal. Hence, by the canon of Berzelius, this must be the protoxide. Therefore, as 7.29 : 100 :: 1 (atom ox.) : 13.71 weight of an atom of silver.

60. *Gold.*—Berzelius obtained two oxides of gold, the second containing three times the oxygen contained in the first.

First oxide	100 gold +	4.005 ox.
The peroxide	100	+ 11.982

Hence as the difference is 8 in this progression, oxygen cannot combine with gold according to any other term above unit than the first, which is, therefore, a binary, composed of one atom of gold + one atom oxygen. We have, therefore, this proportion to determine the weight of an atom of gold as 4.005 : 100 :: 1 : 24.96.

The peroxide is a quaternary, as it consists of 1 atom gold + 3 atoms oxygen.

61. *Platinum.*—From the experiments of Mr. Cooper, it follows that 100 parts of platinum combine with 4.423 oxygen; hence the atom of platinum must weigh 22.625.

62. *Palladium.*—Berzelius determined the only known oxide of palladium as composed of 100 palladium + 14.209 oxygen. If this be a protoxide, the atom of palladium will be 7.03, for 14.209 : 100 :: 1 : 7.03.

But if we suppose the sulphuret of palladium, as found by Vauquelin, to consist of 100 metal + 24 sulphur, and to be a protosulphuret, then the atom of palladium would be represented by 8.338 for $24 \div 2 : 100 :: 1 : 8.333$.

63. *Rhodium.*—There are three oxides of rhodium, composed, as determined by different analyses, of first, 100 metal + 6.71 oxygen; the second, 100 metal + 13.42 oxygen; the third, 100 metal + 20.13 oxygen. Now 6.71, 13.42, and 20.13, are to each other as 1, 2, 3, and consequently the first is a protoxide, and 6.71 : 100 :: 1 : 14.9, or nearly 15 weight of the atom of rhodium.

64. *Iridium.*—The oxides of iridium have not been determined;

but if the sulphuret, composed, according to Vauquelin, of 100 metal + 33.3 sulphur, be considered as a protosulphuret, the weight of the atom of iridium will be found as $33.3 : 100 :: 2$ (weight of an atom of sulphur) : 6 atom of iridium.

65. *Antimony*.—Antimony forms several oxides, the lowest of which, according to Berzelius, is composed of 100 metal + 18.6 oxygen; the next of 100 + 27.9; and a third of 100 + 37.2. Now, these numbers are to each other as 2, 3, 4; hence, the first appears to be a deutoxide; therefore, $18.6 : 100 :: 2$ (weight of 2 atoms oxygen) : 10.75 weight of the atom of antimony. But Dr. Thomson makes the atom of antimony 5.625, taken from his analysis of the sulphuret of antimony, which he finds composed of 100 antimony + 35.572 sulphur. Now, $35.572 : 100 :: 2 : 5.625$.

66. *Molybdenum*.—From the experiments of Bucholz there seems to be three oxides of this metal. The greatest, molybdic acid, composed of 100 metal + 50 oxygen; the next, molybdous acid, of 100 metal + 33.3 oxygen. The third has not been analyzed; but as oxygen combines in an arithmetical progression, the difference between 50 and 33.3 = 16.7 taken from 33.3 will give the next lowest term to 33.3 = 16.6 oxygen.

But independent of this step $33.3 : 50 :: 2 : 3$ nearly. Hence the oxide of 100 metal and 33.3 oxygen is a deutoxide, by means of which we may obtain the atom of molybdenum.

As $33.3 : 100 :: 2$ (weight of two atoms oxygen) : 6, weight of the atom of molybdenum.

67. *Tungsten*.—It is proved, by experiment of Messrs. D'Elhuyarts, Bucholz, and Berzelius, that tungstic acid is composed in the proportion of 100 tungsten and 25 oxygen; and Berzelius has also shown that brown oxide of tungsten contains very nearly two thirds the quantity of oxygen that exists in tungstic acid, or that it is composed of about

Tungsten	100.0
Oxygen	16.6

The brown oxide may, therefore, be properly considered a deutoxide, because 16.6 oxygen : 25 oxygen in tungstic acid :: 2 : 3. From this consideration the atom of tungsten will be 12; because $16.6 : 100 :: 2$ (atoms oxygen) : 12 the atom of tungsten.

68. *Columbium*.—This metal forms with oxygen but one known oxide, consisting of 100 metal + 5.485 oxygen. Then as $5.485 : 100 :: 1 : 18.23$, weight of the atom of columbium.

69. *Nickel*.—We are acquainted with two oxides of nickel; the first, from a mean of the experiments of Taputi, Rothoff, and Berzelius, is composed of 100 metal + 27.6 oxygen; the second, by the experiments of Rothoff, of 100 nickel + 41 oxygen. These numbers are in the following ratio: $27.6 : 41 :: 2 : 3$. Hence, 2 and 3 being the lowest proportional integers of 27.6 and 41, we may take the 2 as representing two atoms, and the

3, three atoms. The 27.6 is, therefore, a deutoxide, viz. 1 atom metal + 2 atoms oxygen, and the 41 a tritoxide of 1 atom metal + 3 atoms oxygen. Hence, these proportions are as

Oxygen.	Metal.	Weight of 2 ox.	Atom nickel.
27.6	: 100	:: 2	: 7.305
41.0	: 100	:: 3	: 7.317

Here, the weight of the atom of nickel comes out nearly identical. The mean of $7.305 + 7.317 \div 2 = 7.311$ weight of the atom of nickel. But others consider the compound 100 metal + 28.74 oxygen a protoxide; in that case the atom of nickel would be only half the weight assigned to it. Dr. Thomson says 3.375, and thinks 100 metal + 29.63 the proper proportions.

70. *Cobalt*.—There are two known oxides of cobalt, the blue and the black. From the analysis of Rothoff they appear to be composed of 100 metal + 27.3 oxygen and 100 metal + 40.95 oxygen. Now, these are to each other as 2 to 3, and the weight of the atom of cobalt may be had like that of nickel. It is 7.326.

It is to be observed of cobalt in like manner as of nickel, that its atom will either be 7.326 or half that number, according as we make 100 metal + 27.3 a protoxide, or a deutoxide. Dr. Thomson supposes the peroxide composed of two atoms metal + three atoms oxygen, which would give the number 3.625; but I think the supposition is inadmissible, because it resolves itself into 1 atom metal + 1.5 atom oxygen.

It will be seen that oxygen unites with nickel and cobalt nearly in the same quantity, and that the weight of the atoms of these metals is almost exactly the same. Generally, if not invariably, it will be found that the weights of atoms are to each other *inversely* as the quantity of oxygen with which equal quantities of each metal combines to form similar oxides. Thus, 13.71 weight of an atom of silver is to 25 weight of an atom of mercury as 3.99 oxygen in the protoxide of mercury to 7.272 oxygen in the protoxide of silver. The same will apply to the other states, as they are all multiples of the protoxides; whence we may infer that where the weight of oxygen is the same in any similar oxides, the weight of the atoms will be the same. The oxides and atoms of nickel and cobalt, considered in either way, are illustrations in point.

71. *Manganese*.—According to Dr. John, sulphate of manganese contains 100 parts acid combined with 92.06 parts protoxide of manganese. Hence, from what has been said before concerning the oxygen of acids and bases combined, 92.06 parts protoxide of manganese contain 20 parts oxygen, and being composed of 1 atom oxygen + 1 atom manganese, we have the following proportions to determine the weight of the atom, as 20 oxygen : $(92.06 - 20) 72.06$ manganese :: 1 atom of oxygen : 3.603 atom of manganese. From other considerations, Dr. Thomson considers 3.500 more near the truth.

72. *Cerium*.—The experiments of Hisinger establish two oxides of cerium, composed of 100 metal + 17.42 oxygen and 100 metal + 26.115 oxygen. Now $17.41 : 26.115 :: 2 : 3$; hence the 17.41 is a deutoxide, as 2 the lowest proportional integer must represent 2 atoms; and by its means the weight of the atom of cerium is determined as $17.41 : 100 :: 2$ weight of 2 atoms of oxygen : 11.494 the atom of cerium.

73. *Uranium*.—There are two oxides of uranium; the first, black; the second, yellow. According to the experiments of Schoubert, the former consists of 100 uranium + 6.373 oxygen; and the latter of 100 metal + 9.6 oxygen. These numbers are in the ratio of 2 : 3; the black is, therefore, a deutoxide, and to obtain the weight of the atom of uranium, we say as $6.373 : 100 :: 2$ (weight of 2 atoms oxygen) : 31.7 weight of the atom uranium; or else 15.8.

74. *Zinc*.—If the white oxide of zinc be considered a protoxide, we may, from its composition of 100 metal + 23.175 oxygen deduce the atom of zinc; as $23.175 : 100 :: 1 : 4.315$.

75. *Lead*.—If we take the protoxide of lead as composed of 100 metal + 7.692 oxygen, the atom of lead would come out 13. But if we attend to the different proportions in which oxygen combines with lead, viz. 7.692, 11.08, 15.38, which are to each other as 2, 3, 4, nearly, we shall be inclined to consider the 7.692 as constituting the deutoxide, and consequently the weight of the atom will be found twice as great, or $= 13 \times 2 = 26$ nearly.

Table of Proportional Numbers.

The numbers contained in this table are called *proportional*, because they express the ratio in which the substances to which they are affixed combine.

By adding together two simple proportional numbers we get the proportional number of the compound. Thus, 1.125, the proportional number for water, results from the addition of the proportional numbers 1 and 0.125, of oxygen and hydrogen. 2.62 is the proportional number of calcium; by adding 1 to it for oxygen, we get 3.62, which is the number for lime; and if to this last there be added 5, which represents sulphuric acid, we shall get 8.62, the proportional number of sulphate of lime, &c.

Oxygen, 1.00	+1 oxygen = Water	<i>Composition of the Salts.</i>
Hydrogen, 0.125	+1..... = Boracic acid	
Carbon, 0.765	+1..... = Oxide of carbon	
	+2..... = Carbonic acid	
	+0.125 hydrog. = Carbonated hydrogen	
Phosphorus, 1.5	+0.75 oxygen.. = Hypophosphorous acid	
	+1.5..... = Phosphorous acid	
	+2.5..... = Phosphoric acid	
Sulphur, 2	+6.6 chlorine.. = Protochloruret	
	+11.0..... = Deutochloruret	
	+2 oxygen = Sulphurous acid	
Sulphur, 2	+3..... = Sulphuric acid	
	+0.125 hydrog. = Hydrosulphuric acid	
	+0.765 carbon. = Carburet of sulphur	
Sulphur, 2	+4.4 chlorine.. = Chloruret of sulphur	
Iodine, 15.62	+5 oxygen = Iodic acid	
	+0.125 hydrog. = Hydriodic acid	
	+0.585 nitrog. = Ioduret of nitrogen	
Chlorine, 4.4	+1 oxygen = Protoxide of chlorine	
	+4..... = Deutox. ditto	
	+5..... = Chloric acid	
	+1.765 ox. of carbon..... = Chloroxicarbonic acid	
Nitrogen, 1.75	+0.125 hydrog. = Hydrochloric acid	
	+1 oxygen..... = Protoxide of nitrogen	
	+2..... = Deutoxide	
	+3..... = Hyponitrous acid	
	+4..... Nitrous acid	
Nitrogen, 1.75	+5..... Nitric acid	
	+1.63 carbon.. Cyanogen	
	+0.375 hydrog. Ammonia	

2.765 carbonic acid + 1 part of a base containing 1 oxygen, form a subcarbonate. The number 2.765 must be doubled for the saturated carbonates.

3 phosphorous acid + a quantity of any base, having 1 oxygen, form a neutral phosphite.

4 phosphoric acid + a quantity of base, having 1 oxygen, form a neutral phosphate. For the subphosphates or the acid phosphates, 4 the number for the phosphoric acid must be multiplied by 2, the quantity of base remaining in the same.

4 sulphurous acid + a quantity of base, having 1 oxygen, form a neutral sulphite.

5 sulphuric acid + a quantity of base, having 1 oxygen, form a neutral sulphate.

2.125 hydrosulphuric acid + a quantity of base, having 1 oxygen, form a neutral hydro-sulphate.

20.62 iodic acid + a quantity of base having 1 oxygen form a neutral iodate.

15.745 hydriodic acid + a quantity of base, having 1 oxygen, form a neutral hydriodate.

9.4 chloric acid + a quantity of base, having 1 oxygen, form a neutral chlorate.

4.525 hydrochloric acid + a quantity of base, having 1 oxygen, form a neutral hydrochlorate.

6.75 nitric acid + a quantity of base, having 1 oxygen, form a neutral nitrate.

Arsenic, 4·75	{	+ 1·5 oxygen... Oxide of arsenic	{	7·25 arsenic acid + a quantity of base, having 1 oxygen, form a neutral arseniate.
		+ 2·5..... Arsenic acid		
		+ 3 sulphur.... Sulphuret of arsenic		
		+ 6·6 chlorine.. Chloruret of ditto.		
		+ 23·43 iodine.. Ioduret of ditto		
Molybdenum, 6	{	+ 2 oxygen..... = Oxide of molybdenum	{	9 molybdic acid + a quantity of base, having 1 oxygen, form a neutral molybdate.
		+ 3..... = Molybdic acid		
		+ 4 sulphur.... = Sulphuret of molybdenum		
Chromium, 3·5	{	+ 1·5 oxygen... = Oxide of chromium	{	6·5 chromic acid + a quantity of base, having 1 oxygen, form a neutral chromate.
		+ 3..... = Chromic acid		
		+ 1·5 oxygen... = Oxide of tungsten		
Tungsten, 12.	{	+ 3..... = Tungstic acid	{	15 tungstic acid + a quantity of base, having 1 oxygen, form a neutral tungstate.
		+ 1..... = Columbic acid		
Columbium, 18·23	{	+ 1..... = Columbic acid	{	19·23 columbic acid + a quantity of base, having 1 oxygen, form a neutral columbate.
		+ 1..... = Silic		
Silicium,	{	+ 1..... = Zircon		
Zirconium, 4·625		+ 1..... = Alumine		
Aluminium, 1·115		+ 1..... = Glucine		
Glucinum, 2·205		+ 1..... = Yttria		
Yttrium, 4		+ 1..... = Lime		
Calcium, 2·62	{	+ 4·4 chlorine.. = Chloruret of calcium		
		+ 15·62 iodine.. = Ioduret of ditto		
		+ 1 oxygen = Magnesia		
Magnesium, 1·5	{	+ 4·4 chlorine.. = Chloruret of magnesium		
		+ 15·62 iodine.. = Ioduret of ditto		
		+ 1 oxygen = Strontian		
Strontium, 5·5	{	+ 4·4 chlorine.. = Chloruret of strontium		
		+ 15·62 iodine.. = Ioduret of ditto		
		+ 1 oxygen = Barytes		
Barium, 8·73	{	+ 4·4 chlorine.. = Chloruret of barium		
		+ 15·62 iodine.. = Ioduret of barium		
		+ 1 oxygen = Soda		
Sodium, 6·006	{	+ 1·5..... = Peroxide of sodium		
		+ 4·4 chlorine.. = Chloruret of do.		
		+ 15·62 iodine.. = Ioduret of ditto		
		+ 1 oxygen = Potassa, or potash		
Potassium, 5·0	{	+ 3..... = Peroxide of potassium		
		+ 4·4 chlorine.. = Chloruret of do.		
		+ 15·62 iodine.. = Ioduret of ditto		

Manganese, 3·603	}	+1 oxygen = Protoxide
		+1·5 = Deutoxide
		+2..... = Peroxide
		+4·4 chlorine.. = Chloruret of manganese.
Zinc, 4·315	}	+1 oxygen..... = Protoxide
		+2 sulphur = Sulphuret
		+4·4 chlorine.. = Chloruret
		+15·62 iodine.. = Ioduret
Iron, 3·45	}	+1 oxygen = Protoxide
		+1·5 = Peroxide
		+2 sulphur ... = Protosulphuret
		+4 = Deutosulphuret
		+4·4 chlorine.. = Chloruret
		+15·26 iodine.. = Ioduret
Tin, 7·352.	}	+1 oxygen = Protoxide
		+2..... = Deutoxide
		+2 sulphur = Protosulphuret
		+4..... = Deutosulphuret
		+4·4 chlorine.. = Protochloruret
		+8·8..... = Deutochloruret
Antimony, 10·75	}	+15·62 iodine.. = Ioduret
		+1 oxygen ... = Protoxide
		+1·41. = Deutoxide
		+2..... = Peroxide
		+4·4 chlorine.. = Chloruret
		+2 sulphur ... = Sulphuret
Uranium, 31·70	}	+15·62 iodine.. = Ioduret
		+1 oxygen = Protoxide
		+1·5 = Deutoxide
Cerium, 11·494	}	+1..... = Protoxide
		+1·5..... = Deutoxide
		+4·4 chlorine.. = Chloruret
Cobalt, 7·326	}	+1 oxygen = Protoxide
		+1·3..... = Deutoxide
		+4·4 chlorine.. = Chloruret
Bismuth, 8·758	}	+1 oxygen = Oxide
		+2 sulphur = Sulphuret
		+4·4 chlorine.. = Chloruret
		+15·62 iodine.. = Ioduret
Copper, 8·0	}	+1 oxygen..... = Protoxide
		+2..... = Deutoxide
		+2 sulphur = Sulphuret
		+4·4 chlorine.. = Chloruret (Proto.)
Tellurium, 3·593	}	+8·8..... = Deutochloruret
		+15·65 iodine.. = Ioduret
		+1 oxygen = Oxide
		+4·4 chlorine.. = Chloruret
Nickel, 7·311	}	+0·125 hydrog: = Hydrogeneted tellurium
		+1 oxygen = Protoxide
		+1·5 = Deutoxide
		+4·4 chlorine.. = Chloruret
Lead, 26·0	}	+1 oxygen..... = Protoxide
		+1·5 = Deutoxide
		+2..... = Tritoxide
		+2 sulphur = Sulphuret
		+4·4 chlorine.. = Chloruret
		+15·62 iodine.. = Ioduret
Mercury, 25·0	}	+1 oxygen = Protoxide
		+2..... = Deutoxide
		+2 sulphur = Sulphuret, (Proto.)
		+4..... = Deutosulphuret
		+4·4 chlorine.. = Protochloruret
	}	+8·8..... = Deutochloruret
		+15·62..... = Protoioduret
		+31·24..... = Deutoioduret

Silver, 13.71	$\left\{ \begin{array}{l} +1 \text{ oxygen} \dots = \text{Oxide} \\ +2 \text{ sulphur} \dots = \text{Sulphuret} \\ +4.4 \text{ chlorine} \dots = \text{Chloruret} \\ +15.62 \text{ iodine} \dots = \text{Ioduret} \end{array} \right.$
Palladium, 8.333	
Gold, 24.96	
Platinum, 23.625	
Iridium, 6	$\left\{ \begin{array}{l} +1 \text{ oxygen} \dots = \text{Protoxide} \\ +2 \dots \dots \dots = \text{Deutoxide} \\ +4 \text{ sulphur} \dots = \text{Deutosulphuret} \\ +8.8 \text{ chlorine} \dots = \text{Deutochloruret} \end{array} \right.$
	$\left\{ \begin{array}{l} +1 \text{ oxygen} \dots = \text{Protoxide} \\ +2 \dots \dots \dots = \text{Deutoxide} \\ +8.8 \text{ chlorine} \dots = \text{Deutochloruret} \\ +2 \text{ sulphur} \dots = \text{Sulphuret} \end{array} \right.$

The weight of each inflammable substance in this table is in general such, that in uniting to one part of oxygen, it passes to the first degree of oxidation. But the rule has been departed from in regard to phosphorus, sulphur, iodine, arsenic, molybdenum, chromium, tungsten. For each of these the representative number is taken from such a weight of its acid as can saturate a base containing one part of oxygen. This renders the table more short and convenient, because it leaves nothing to do except adding the number that represents the weight of an acid (found in the table) to the number representing any base whatever (found also in the table) in order to get the proportions of all the salts.

For example, by adding 5 sulphuric acid, or 4 of sulphurous acid to 3.62 lime, we get the sulphate or the sulphite of lime.

Dr. Wollaston has made an arrangement of proportional numbers, which he calls a *synoptic scale of chemical equivalents*, that exhibits in a very condensed form the constituents of a great many compound bodies.

When a chemist subjects a saline substance to analysis, the questions to be answered are so many and various, that he is seldom disposed to undertake, by himself, the necessary series of experiments, if he can rely with confidence on the labours of his predecessors.

Let the crystallized sulphate of copper be the subject of analysis, and the questions to be resolved are these. How much does it contain of sulphuric acid? How much oxide of copper? How much water? We may wish to know further the quantity of sulphur, of copper, of oxygen, of hydrogen.

It may be necessary to consider, likewise, how much of the different reagents must be employed to discover the proportion of sulphuric acid; for instance, how much barytes, carbonate of barytes, or nitrate of barytes; or how much lead in the form of nitrate. And after having obtained the precipitates of sulphate of barytes and sulphate of lead, it is still necessary to find the proportion of dry sulphuric acid which they respectively hold.

We may wish to verify these results by ascertaining the pre-

cise quantity of pure potash, or carbonate of potash, required for the precipitation of the copper. Lastly, zinc or iron may be used for the same purpose, and then it will, perhaps, be desirable to know how much sulphate of zinc or sulphate of iron remains in solution.

Were we to resolve many such questions, we should find it to be extremely fatiguing, and to waste a great deal of time; whereas a considerable deal of both is saved to the experimental chemist, whenever he can have recourse to a prior analysis that is executed with all due care and accuracy.

The synoptic scale of chemical equivalents resolves all these questions by bare inspection, as relates to many of the salts contained in the table. It not only gives the numerical proportions on which the desired solution is calculated, but it expresses the precise weights of the different constituents of a given quantity of the salt under examination, no less than the quantity of the reagents necessary for its analysis, and that of the precipitates which each of them throws down. Such a scale could not be formed without a previous determination of the proportions in which the different known bodies unite, and without their being expressed in such terms that the same substance would be always represented by the same number.

This mode of designation is due to Richter; he also was the first who observed the law of constant proportions on which alone these numerical representations can be founded.

By the theory of Dalton, which is the one that best explains the phenomena; chemical saturation results from the union of a single atom of each of the combining bodies; and where one of the constituents is in excess, then two or more atoms of this combine with a single atom of the other.

According to this view of the subject, when we count the relative weights of the equivalents, Mr. Dalton conceives that we count the united weight of a given number of atoms, and consequently include the proportion which exists between the ultimate molecules of each of these substances. But, in the case of two combinations of the same substances, since it is difficult to determine which of them is the compound of one pair of simple atoms, and since the decision of this question affects only the theory, without being at all necessary to the formation of a table intended for practical use, Dr. Wollaston has taken no pains to make his numbers correspond with the hypothesis of atoms. His object has been to render his table practical, and he considers the doctrine of simple multiples, on which the theory of atoms is founded, as only means of determining, by division, those quantities that do not precisely coincide with the law of Richter.

Dr. Wollaston took for the basis of his calculations and real measure of comparison for determining equivalents, a determinate quantity of carbonate of lime. This is a perfectly neutral

compound; it is easily obtained in a state of uniform purity, and easily analyzed as a binary compound. It is the most suitable measure of the power of the acids, and furnishes the most precise expression of the comparative neutralizing power of the alkalies.

The first thing, consequently, to be done was to determine the number that should represent the relative weight of carbonic acid, when oxygen is represented by 10. But it is a thing satisfactorily proved that a determinate quantity of oxygen gives an exactly equal volume of carbonic acid by uniting with carbon; and as the specific gravity of these gases is as 10 to 13.77, or as 20 to 27.54, the weight of carbon is given exactly equal to 7.54, which in this combination with 20 oxygen forms the deut-oxide. The protoxide of carbon is represented by 17.54.

Carbonic acid being, therefore, represented by 27.54, it results from the analysis of carbonate of lime, which, during calcination, loses 43.7 acid, and leaves 56.3 base per cent. that these two substances are combined in the proportion of 27.54 to 35.46; and consequently that lime should be represented by 35.46, and carbonate of lime by 63.

If the series of decompositions be now continued in order to verify the preceding analysis, 63 parts of carbonate of lime may be dissolved in muriatic acid, and after evaporating the solution to perfect dryness, we obtain 69.56 of muriate of lime; and by subtracting 35.46, weight of the lime, we get a difference of 34.1, which represents the amount of dry muriatic acid.

But lime is known to be a metallic substance united to oxygen; this salt may consequently be considered under another point of view, as a binary compound, an oxymuriate of calcium. In this case we must transfer the 10 oxygen to the muriatic acid, and the combination will read 44.1 oxymuriatic acid combined with 26.46 calcium, = 69.56. Or else, if we consider it as a chloride of calcium, its value, on the scale of numbers, will still remain 69.56, whether we call it oxymuriatic acid, dephlogisticated marine acid, or chlorine; for the portion of matter added to the calcium is always the same, and represented by 44.1. This number consequently represents a mere fact, which is independent of any theory; but which enables us to estimate the proportion of constituents in every muriatic combination, without discussing their simple or compound nature, a question still undecided.

In like manner the muriates of potassa and soda will find their place on the scale of equivalents, and the relative weights of pure potassa and soda can be determined with the greatest exactness, because they are not susceptible of an excess of acidity, or of decomposition by heat.

If to a quantity of muriatic acid, which can just dissolve 100 parts of carbonate of lime, there be added 100 grs. of crystallized carbonate of potassa, and that after this addition the acid can

no longer dissolve more than 49·8 of carbonate of lime, it follows that 100 crystallized carbonate of potassa are the chemical equivalent of 50·2 of carbonate of lime, and consequently that 125·5 are the equivalent of 63 on the scale. $50\cdot2 : 100 :: 63 : 125\cdot5$.

In the next place, if 125·5 crystallized carbonate of potassa be combined with muriatic acid to excess, and that the solution is evaporated until all the water and the redundant acid is driven off, there will remain 93·2 of neutral salt, whether it be called muriate of potassa, or chloride of potassium, or by whatever name designated, it will always yield 34·1 of dry acid; consequently, the value of the potassa is 59·1. This will contain but 49·1 potassium, which requires 1 of oxygen.

Another important consideration relates to the composition of the crystallized carbonate of potassa, which Dr. Wollaston, with great propriety, calls bicarbonate of potassa. This name distinctly marks the difference between it and the subcarbonate, and recalls the double dose of carbonic acid which it contains. When compared with the carbonate of lime, it is even necessary to consider it as a supercarbonate, for if a solution of this salt be added to a neutral solution of carbonate of lime, there arises a strong effervescence, owing to the carbonic acid exceeding the quantity necessary for the saturation of the lime. When 125·5 parts of bicarbonate of potassa are saturated with nitric acid, and all due care is taken to prevent any loss of fluid with the gas that escapes, the loss of carbonic acid is 55, or the double of 27·5. But if, before saturation, the salt has been exposed to a low red heat, it sustains a loss of 38·8, viz. 27·5 carbonic, and 11·3 of water, after which the addition of an acid drives off but 27·5, or a single proportion of carbonic acid.

The scale of chemical equivalents is now commonly sold in the book stores of this city, and I cannot too strongly recommend its use. A frequent inspection of it will remind the student of many proportions which he would otherwise forget, and, as an exercise, it will familiarize him with the most important chemical combinations.

To the practical chemist it gives the composition of any weight whatever of any of the salts contained on the scale, the quantity of any other salt necessary to decompose it, the quantity of the new salt that will be formed, and many other similar things that are perpetually occurring.

The scale itself has various simple and compound substances set down on both sides of a slider that moves through it longitudinally. To these substances numbers are annexed, and, by motion of the slider, numerous combinations may be quickly determined, without the intervention of calculation. There are two positions of the slider.

In the first, 10 on the slider is brought opposite to oxygen on the scale, and other bodies are in their due proportion to oxygen,

so that carbonic acid being 27·54, and lime 35·46, carbonate of lime is placed at 63.

In like manner, the weight of oxygen being 10, the proportional weight of sulphur is 20, azote 17·5, &c. and by simple addition of the proportional weights of the single bodies on the scale we get those of the compounds. Magnesia is 25, sulphuric acid 50, and sulphate of magnesia 75, on the scale; therefore, 75 dry sulphate of magnesia is composed in the proportion of 25 base and 50 acid.

Carbonic acid being 27·54, and ammonia 21·5, subcarbonate of ammonia is placed at 49·4, and is composed in this proportion.

Muriatic acid gas being 45·5, and ammonia 21·5, they combine in the gaseous state, and muriate of ammonia is 67.

Oxide of silver being 145·5, and dry muriatic acid 34·5, muriate of silver is 180.

Oxide of copper is 50, and sulphuric acid is 50, but crystallized sulphate of copper contains water of crystallization, and oxygen being 10, the proportion in which water stands to it is 11·4, which, multiplied by 5, the proportion in which the water of crystallization stands to dry sulphate of copper, makes 57 added to 100 = 157, the number on the scale.

Oxide of zinc is 51, and sulphuric acid 50, which + 79 water of crystallization, makes 180, the number on the scale.

Lead on the scale is placed opposite to 129·8 in this position of the slider. The addition of 10 oxygen and 34·4 dry muriatic acid give 174, the number for muriate of lead on the scale.

Lastly, mercury is 126·6, and oxygen being 10, dry muriatic acid 34·4, as above, corrosive muriate of mercury is placed opposite to 170 on the slider.

Second Position of the Slider.

If it be required to find the weights of the constituents of a given compound, say 100 oxide of iron, the slider is to be moved until 100 comes against oxide of iron. Then the numbers on the slider opposite the constituents, oxygen and iron, is the quantity of each in 100 of the compound. For oxygen, it is 22·5, and for iron 77·5.

It also gives the constituents of sulphate of iron per cent. as composed of oxide of iron 25·8, sul. acid 28·2, water of crystallization 46.

Let there be given a quantity of any salt, how much will be given of any other, having a constituent the same as one in the given salt?

Let bicarbonate of soda be the salt given. When the scale is adjusted, 100 being opposite to bicarbonate of soda, and salts having the same base (soda) are sought for, it will be seen immediately that these 100 parts bicarbonate contain as much soda as 84·5 dry sulphate of soda; 69·5 muriate of soda; 63 subcarbonate of soda; 101 nitrate of soda, &c.

Or, if salts having the same acid and the same quantity be required, the following will be found on the scale, viz. 46.5 sub-carbonate of ammonia; 75.5 bicarbonate of ammonia, 60 carbonate of lime; 118.8 carbonate of barytes, &c. Moreover, it will be seen on the scale by looking at soda, that *all* the first named salts contain 37 soda, and all the last named 26.2 carbonic acid.

It is not always necessary to place the slider at 100. For any acid, as 50 sulphuric acid, move the slider until sulphuric acid on the scale stands opposite 50 on the slider, and the numbers opposite the constituents of the acid, marked on the scale, will give their proportional weights in 50 of the acid; according to this the oxygen will be 30, and the sulphur 20.

For any salt, as 60 carbonate of lime, when 60 stands opposite the carbonate of lime, the constituents will be 26.3 acid + 33.7 base. If the slider had been placed at 100 carbonate of lime in the usual way, the acid would be 43, and the base 56, but these numbers for 100 are in precisely the same proportion as the former for 60.

I shall conclude this view of the valuable scale of chemical equivalents by an example taken from Dr. Wollaston's paper, in the *Phil. Trans.* for 1814, Part I.

“In the second figure, the slider is represented drawn upwards till 100 corresponds to muriate of soda; and accordingly the scale then shows how much of each substance contained in the table is equivalent to 100 of common salt. It shows, with regard to the different views of the analysis of this salt, that it contains 46.5 dry muriatic acid, and 53.4 of soda, or 39.8 sodium, and 13.6 oxygen; or if viewed as chloride of sodium, that it contains 60.2 chlorine, and 39.8 sodium. With respect to reagents, it may be seen that 283 nitrate of lead, containing 191 of litharge employed to separate the muriatic acid, would yield a precipitate of 237 muriate of lead, and that there would then remain in solution nearly 146 nitrate of soda. It may at the same time be seen, that the acid in this quantity of salt would serve to make 232 corrosive sublimate, containing 185.5 red oxide of mercury; or would make 91.5 muriate of ammonia, composed of 62 muriatic gas (or hydromuriatic acid) and 29.5 ammonia. The scale shows also, that for the purpose of obtaining the whole of the acid in distillation, the quantity of oil of vitriol required is nearly 84, and that the residuum of this distillation would be 122 dry sulphate of soda, from which might be obtained, by crystallization, 277 of Glauber salt containing 155 water of crystallization. These and many more such answers appear at once by bare inspection, as soon as the weight of any substance intended for examination is made by motion of the slider correctly to correspond with its place in the adjacent column.”

ARTICLE IV.

Researches into the Mathematical Principles of Chemical Philosophy. By J. B. Emmett. (With a Plate.)

(Continued from p. 188.)

SIR,

Hull, Sept. 6, 1820.

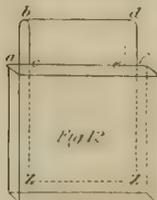
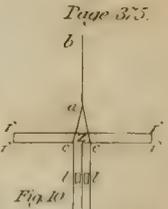
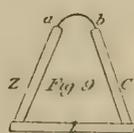
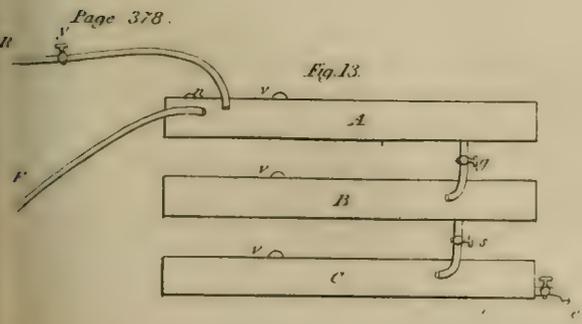
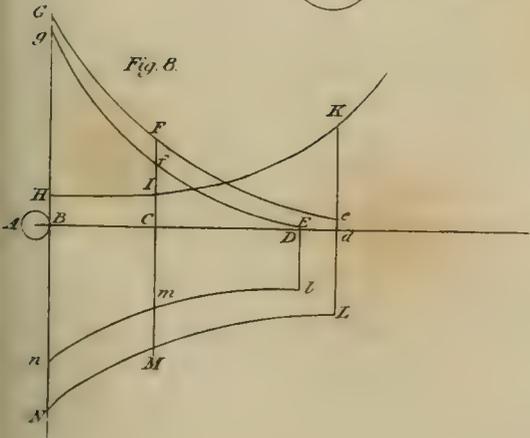
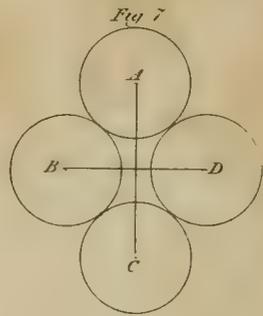
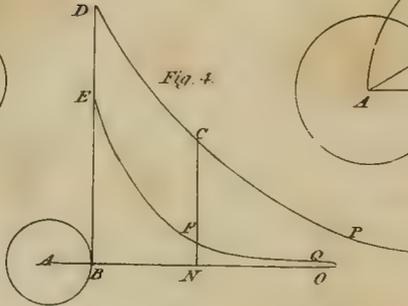
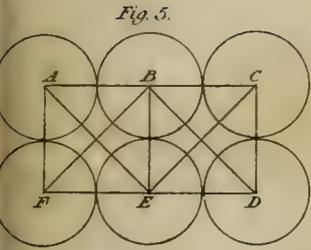
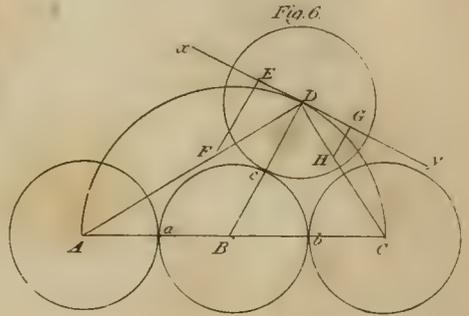
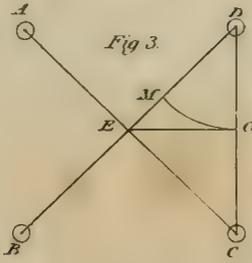
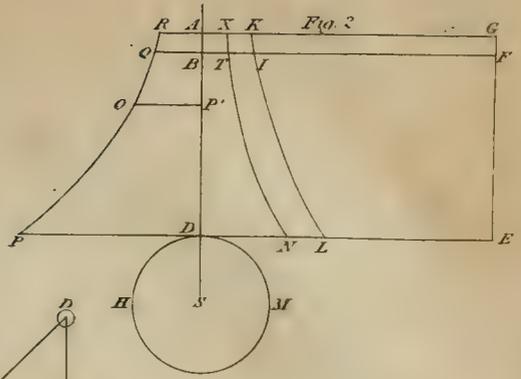
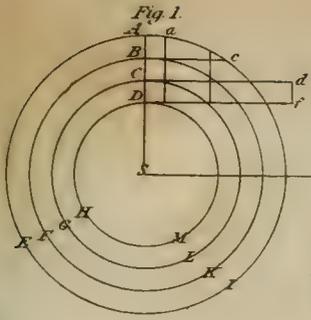
CALORIC appears to be an elastic fluid, of very great rarity, and possessing an eminent degree of elastic force; capable of condensation and rarefaction, attracted by every form of ponderable matter, but its own particles seem to be mutually repellent. That it is an elastic fluid appears highly probable from the phenomena of its absorption and emission; a given mass of one form of matter, in passing from any one to any other degree of temperature, will absorb or emit more heat than an equal mass of another; if, therefore, it be admitted to be a material agent, it must be susceptible of condensation and rarefaction, which may also be proved from the fact of the existence of various degrees of heat. That it is elastic appears evident from the facility with which it is reflected by polished surfaces, the angle of incidence being equal to the angle of reflection; this property is also rendered highly probable from the circumstance of its communicating an eminent degree of elastic force to those particles of ponderable matter with which it is combined. That it is attracted by ponderable matter is demonstrated by numerous experiments; when a mass of matter is heated above the temperature of the surrounding medium, the sensation of heat is perceived at a very considerable distance, and is the most intense at the nearest distances from the surface. Now since heat always endeavours to dilate itself into indefinite space, the phenomena are perfectly inexplicable, except we admit that that which produces the sensation of heat is attracted by the mass of ponderable matter. Since a full proof of this property of caloric is of primary importance, it may not be improper to dwell a little longer upon this subject, and confirm the statement by a variety of proper experiments. Let an ignited conical point of charcoal be held over a piece of ice, or other cold substance, the redness of the extremity will disappear when there is a very perceptible distance between it and the colder matter, and be renewed when it is withdrawn. If a small thermometer be used instead of the ice, it will indicate an elevated temperature; but if a heated mass of matter be employed, no sensible effect results; since in these experiments there can be no current of air established, by which the heat can be removed, there remains only an attracting force, by which the effect can be produced: besides the mere circumstance of heat attaching itself to solid matter, or to ponderable matter generally, is sufficient to prove that an attracting force is operative between them. Numerous cases of

chemical action demonstrate that caloric is attracted by, and enters into union or combination with, the ultimate particles of matter. Let concentrated sulphuric acid be dropped into a saturated solution of ammonia in water, intense heat, accompanied with an explosive ebullition, immediately succeeds, and solid matter is produced. Let now a solution of the carbonate of ammonia, containing the same proportion of the alkali, be employed; the heat will be less intense, and a large quantity of elastic matter will be liberated. In these experiments, since the materials employed were originally cold, the heat which is liberated must previously have existed in them; and since its presence was not indicated by the thermometer, its action, when in the materials before mixture, must have been suspended as much as that of the sulphuric acid is when it exists in combination with lime; and we know of no power or force capable of producing such an effect, except that which is called attraction; and besides in the latter experiment we have an example of the heat passing from one state of combination to another without ever being so far free and disengaged from the operation of this centripetal force as to affect the thermometer. Caloric is, therefore, attracted by the ultimate atoms of ponderable matter; were further proof wanting, its refrangibility would be sufficient.

Prop. 6.—If a mass or particle of matter be placed in a medium of uniformly diffused caloric, it will be surrounded by an atmosphere of heat, of which the density will be reciprocally as some power of the distance.

Let D H M (Pl. CIX) fig. 1, be a sphere, or particle of matter, placed in a medium of uniformly diffused caloric, of which the density is represented by the line A *a*, through *a* draw the line *a g*, parallel to the radius produced, S A, and let S A be the greatest distance at which the caloric is attracted by the sphere; divide the distance D A into innumerable equal evanescent portions A B, B C, &c. the stratum A B tends towards S, by reason of its own gravitation only, with this force then it compresses all the inferior strata; the density of the next stratum, B C, is, therefore, increased, and may be represented by B *c*, which tends towards the centre S, by its own gravitation; in like manner the next stratum sustains the pressure of these two, and also tends towards S; its density is C *d*; let now the number of strata be increased, and the altitude of each be reduced indefinitely; then since A *a*, B *c*, C *d*, &c. represent the densities at their respective distances, the ordinates of the curve that passes through the innumerable points, *a*, *c*, *d*, &c. will represent the density of the caloric atmosphere at the distances represented by their abscissæ; and since the centripetal force is equal at all equal distances from *s*, and the surrounding caloric has uniform density, the density of the several spherical strata will be uniform, and that of the inferior, greater than that of the superior strata.

Cor. 1.—Hence the elastic or repulsive force of this atmosphere



Page 375.

Fig. 11

will decrease more rapidly than the centripetal force of the particle, when the distance from the centre is increased; for if DHM , fig. 2, represent a sphere, draw the radius SD , and produce it indefinitely; at D draw the tangent PE : let this sphere be surrounded by a calorific atmosphere, of which the density is represented by the ordinates of the curve KIL , at the distances represented by their respective abscissæ; similarly describe the curve XTN such that its ordinates shall represent the centripetal force at the same distances. By Princip. lib. 2, prop. 20, the pressure upon the whole spherical surface DHM is equal to that of a cylinder whose base equals that surface, whose altitude equals that of the atmosphere, and density varies as the density of the atmosphere. Let, therefore, $DAGE$ represent this cylinder; draw BF indefinitely near and parallel to AG , and \therefore parallel to DE . Take AR , BQ , DP , respectively equal to $AX \times AK$, $BT \times BI$, $DN \times DL$, and through the points R , Q , P , describe the curve RQP , whose ordinates shall always be equal to the product of the corresponding ordinates of the curves XTN , KIL ; the pressure of the cylinder $BAGF$ is as the quantity of matter and force of gravity jointly; i. e. as the volume, density, and force of attraction. Now the quantity of matter is as the base $BF (= DE) \times AB \times AK$; wherefore the pressure of this cylinder is as the base $BF \times AB \times AK \times AX$; or as the base $BF \times AB \times AR$. Let the cylinder be divided into innumerable evanescent strata, and the entire pressure will be as the surface $DE \times \text{area } DARP$; also the pressure upon any part of the surface will be as that surface \times area $DARP$, and the force itself as the area $DARP$, and at any other distance P' , if $P'O$ be drawn perpendicular to AD , the force will be as the area $AROP'$; and since the area $ABT\cancel{X}$ of the curve XTN increases more rapidly than the ordinate AX , (the force varying as any inverse power of the distance from S) much more than will the area $ARQB$ of the curb RQP increase more rapidly than the ordinate AX of the curve XTN , but the areas $ARQB$, $AROP'$ are proportional to the compressing forces at B and P' , they must be proportional to the elastic force at the same distances.

Cor. 2.—The density of a calorific atmosphere may be supposed to be less in the lower than in the higher altitudes. Let it vary inversely as the centripetal force; then RQP becomes a right line parallel to AD , whence the elastic force will decrease less rapidly than the centripetal force: such cases cannot attain in nature.

Prop. 7.—Caloric being attracted by the particles of ponderable matter will have a tendency to separate them.

Let A, B, C, D , fig. 3, be four equal, similar, and equidistant particles of matter; join AC, BD, DC : with centre D and radius $DO = CO$ describe the arc OM . By Princip. lib. 2, prop. 20, the elastic force of the calorific atmosphere surround-

ing D will be the same as if the altitude DE were continued in every direction round it, and the same attains with respect to the other particles; consequently by the pressure of the column, or superior part of the atmosphere, EM , the atmosphere of the particle D , will have a tendency to dilate in every direction DO , where its altitude is less than DE ; by this tendency of the atmospheres of D and C , they will have a tendency to recede from each other, which, for the same particles, will be proportional to the area $AROP'$, see fig. 1 to prop. 6, cor. 1.

Prop. 8.—To find the ratio between the centripetal force and the repulsive force of heat, at different distances from the same particle.

Let A , fig. 4, represent a particle of matter, ABO the radius produced: describe the curve DCP such that the ordinate CN may always be inversely as AN^2 , the ordinate will be proportional to the centripetal force, at the distance represented by the corresponding abscissa. Describe the curve EFQ such that its ordinate FN shall be proportional to the area $AROP'$, prop. 6, cor. 1, the ordinates of this will, therefore, be proportional at the distances which are proportional to their respective abscissæ to the elastic force of the calorific atmosphere of the particle; and by prop. 6, cor. 1, the curve EFQ approaches the asymptote AO more rapidly than DCP .

Cor. 1.—Hence the effective force at any distance AN will be as the difference between the ordinates of the curves at that distance.

Cor. 2.—If EB be less than DB , the force will be centripetal at all distances, and in contact; and if N move from B towards O , the efficient centripetal force will increase to a certain distance, beyond which it decreases.

Cor. 3.—If EB be equal to DB , the effective force will be centripetal at all distances, with the variations of cor. 2.

Cor. 4.—If EB be greater than DB , at a certain distance, the curves will intersect each other, or have equal ordinates, the forces will, therefore, be equal; between that point and B , the effective force will be repulsive; beyond it, centripetal.

Prop. 9.—If the particles of matter, being spherical, were under the influence of a centripetal force only, their arrangement would be such that the straight lines which join their centres form equilateral triangles.

Let A, B, C , &c. fig. 5, be a number of equal and equally attractive particles of a solid so situated that the straight lines AB, BE, EF, FA , form a square. Since the forces of these particles are equal at equal distances, and $AE = BF$, these particles mutually balance each other; but if the most minute disturbing force cause the least change in the position of one particle, increase or diminish the force which two mutually exert upon each other, the equilibrium of the whole system is destroyed. Suppose A to be impelled towards E , the force act-

ing in the direction $A E$ becomes greater than that in the direction $B F$; A and E must, therefore, approach to, and B and F recede from, each other; the system can, therefore, become quiescent only when A and E have attained mutual contact, when $A E$, $E F$, $A F$, are mutually equal, or form an equilateral triangle, in which state the equilibrium of the system is permanent; for if any particle be disturbed, it returns to its place when at liberty.

Prop. 10.—The particles of solids being preserved in mutual contact by the force of cohesion, increase of temperature causes expansion, and diminution of heat, contraction.

Let A, B, C , fig. 6, be three particles of equal diameter, and of the same matter, placed in the same right line $A B C$. Since in solid matter the centripetal force exceeds the repulsive force of heat, these will be preserved in mutual contact in the points a, b , by the force of cohesion by prop. 1. Let D be another such particle, which touches B in the point C . By prop. 8, cor. 2, the forces with which D is acted upon by A and C will be centripetal. With centre B and radius $B A = B C$, describe the semicircle $A D C$; at the point D draw the tangent $x y$, and join $A D, B D, C D$. In $A D$ take $F D$ proportional to the excess of the centripetal above the repulsive force (i. e. to $C F$, prop. 8); at the distance $A D$, resolve this into $F E, E D$, the direction of the tangent, and the perpendicular to that direction. Take $H D$ proportional to the effective centripetal force (prop. 8) between D and C , at the distance $C D$, and resolve this into $H G, G D$; of these forces, only the resolved parts $E D, G D$, have any tendency to put the particle D into motion; and since they are both centripetal, and act in opposite directions, D is stationary when $E D = G D$. Let the temperature be increased, the centripetal force $H D$ is diminished more than $F D$ by prop. 8, $\therefore D G$ is less than $D E$; consequently D will move towards x (the locus of its centre being always the circumference $A D C$;) and since parallelograms upon equal bases and between the same parallels are equal, by completing the upper row of particles, the portion of space occupied by the system, or its volume, will be enlarged. If the temperature be diminished, by prop. 8, the force $D H$ being always centripetal will be more increased than $F D$; consequently D will move towards C till a state of equilibrium is attained, and contraction will result.

Or thus: A, B, C, D , fig. 7, being four equal and similar particles of a solid, since the effective forces which they mutually exert upon each other are entirely centripetal, and perfect contact is preserved by the force of cohesion, prop. 1, they will be in a state of rest, when the centripetal forces of the opposite particles, A and C, B and D , multiplied respectively into the ratio of their simultaneous motions, are equal to each other. Suppose them to be in this state of quiescence, by the mutual equilibrium of these quantities, and the temperature, and consequently the density of the calorific atmospheres of these particles to be increased. By prop. 8, the centripetal force mutually exerted

by the two nearest particles, B and D, will be more diminished than that of the more remote, A and C; consequently the former will recede from, and the latter approach nearer to, each other, and they will become quiescent when, by this change of distance, the forces again attain a mutual equilibrium; therefore, Euclid, book 1, prop. 36, an augmentation of volume is produced. In the same manner, diminution of temperature is the cause of contraction.

Cor. 1.—Hence a solid will continue to expand, by reason of this change which takes place in the arrangement of its particles, so long as the centripetal force upon the surface is greater than the force of repulsion.

Cor. 2.—The greatest possible expansion of a solid has taken place when B D is perpendicular to A C, first demonstration, or when A C = B D in the second: in many forms of matter, the cohesive force may be overcome before this is attained; in others, after this arrangement has taken place, a considerable increase of heat may be required to cause a separation of the particles, in which state such a mass will retain a considerable cohesive force, and at the same time may be in a semifluid state: such is melted glass.

Cor. 3.—Hence no form of matter with which we are acquainted can at any time have been exposed to the true zero of temperature; for then, by prop. 9, their arrangement must have been such that straight lines which join their centres form equilateral triangles: there could, therefore, be no expansion by the operation of heat, until its repulsive force upon the surface exceeds the force of cohesion. In such substances, the first sensible effect of heat must be a total separation of the atoms.

Prop. 11.—To find the specific heat and capacity for heat, of a particle of matter.

Let A, fig. 8, represent a particle of matter, which is immersed in a medium of uniformly diffused caloric; it is surrounded by a calorific atmosphere, by prop. 6, of which the altitude is half the greatest distance between two particles, i. e. is D E, prop. 7. Let this altitude be B *d*, and let the curve G F *e* be the curve K I L of prop. 6, cor. 1, or such that its ordinate F C is always proportional to the density of the caloric, at the distance to which its abscissa B C is proportional. Let the curve H I K be such that its ordinate C I is proportional to the square of the abscissa, i. e. to the concentric spherical superficies. As C moves from B to *d*, multiply together the ordinates C I and F C, and let the ordinate C M be proportional to this product, and there results a new curve N M L, whose area B N M L *d* B represents the whole quantity of caloric contained in a spherical atmosphere surrounding A, and having the altitude B *d*; this area, therefore, represents the specific or latent heat.

If the temperature be diminished, the altitude and density of the atmosphere will be diminished. Let *g f* E represent the new

curve, and multiply, as before, its ordinates into the corresponding ordinates of the curve $H I K$, and the resulting curve is $n m l$, whose area $B n m l D B$ represents the whole of the latent heat, and the difference, or the area $D d L N n m l D$, is proportional to the quantity of caloric liberated by this reduction of temperature, or to the capacity for heat.

Cor. 1.—Hence the capacity for heat must be greater at high than at low temperatures.

Cor. 2.—Hence all bodies having the same density may not have the same capacity for heat at the same temperature.

Cor. 3.—Hence the true capacities of bodies for heat must be those of weights which are proportional to those of their atoms.

Cor. 4.—Hence the reason why the quantity of caloric which is represented by the area $B N L D$ cannot affect the thermometer, that instrument indicating the elastic force with which the caloric endeavours to escape.

Cor. 5.—The real capacities of bodies for heat must be in the ratio of the evanescent increments of the area $B N L D$, belonging to each.

By help of these propositions, many curious phenomena admit of an easy explanation. By considering caloric an elastic fluid, which is attracted by every form of ponderable matter, we find that in solid matter, the particles are always preserved in mutual contact with each other by the cohesive force, by prop. 1 and 4: that variation of temperature, by causing a change to take place in the order of their arrangement, prop. 10, produces expansion and contraction, the utmost limits of which are small, compared with the entire volume, and that the cohesive force of solids is less at high than at low temperatures. From which facts it follows, that the true ratio of the expansibility of the various forms of solid matter is not that of equal lengths, but of lengths which are proportional to the diameters of their atoms; the ratio of the diameters of the atoms of simple solids is easily ascertained nearly by knowing the specific gravity and the atomic weights. We may also see the reason why all solids do not expand equally by the application of equal increments of heat; for the particles of different solids have various diameters and degrees of centripetal force, and consequently have calorific atmospheres of different density.

The whole quantity of caloric which is contained in any mass of matter being the sum of all the calorific atmospheres; and the capacity for heat being the sum of all the increments of these atmospheres by an increase of heat; it is evident that the real ratio of the capacities of bodies for heat is not that of equal weights or volumes, but of weights which are proportional to their atomic weights. It is also manifest, prop. 11, that the capacities will be somewhat greater at high than at low temperatures.

By prop. 11, masses of matter will be surrounded by calorific

atmospheres, which will become sensible when the temperature of the surrounding medium is lowered, the intensity of which, as well as its extent, depending upon the force with which the body attracts caloric; this is the cause why the temperature is less in the higher regions of the atmosphere than near the earth's surface; for the earth is a warm globe placed in a region of absolute cold.

Prop. 9 and 10 immediately apply to the curious phenomena of unannealed glass, and some other similar substances. If the force of cohesion be very great, the particles of the body may form such an arrangement that straight lines which join their centres form squares, and a considerable additional increment of heat required to separate them. In this situation of the atoms, the body will be in a semifluid state, and if cooled, this arrangement will remain, by prop. 9, if no disturbing force operate; but if but one particle be disturbed when the mass is cold, the equilibrium, and consequently the arrangement of the whole system is destroyed. Now glass is a substance which is in this semifluid state when fused, and which, when cold, has great cohesive force; and if cooled very gradually, the particles will assume a state of permanent equilibrium, by prop. 10; but if rapidly cooled, the intensity of the cohesive force will prevent this, and preserve the arrangement in which the particles were placed when in a state of fusion; when by prop. 9, any force disturbing some of the atoms, the whole system is deranged, which must produce a fracture of the mass in so hard and brittle a substance.

The primary laws of the radiation of heat and of crystallization are now easily explained. These will be investigated in the next.

(To be continued.)

ARTICLE V.

*An Essay on the Invention of Alphabetic Writing.** By A. Carmichael, M.R.I.A.

THE difficulty of accounting for the invention of alphabetic writing, and the impossibility of tracing any connexion between letters, which are the representatives of sounds, and hieroglyphics, which are resemblances of things, has induced men of extraordinary erudition and talents to ascribe the gift to a direct revela-

* This essay was published in a late volume of the Memoirs of the Royal Irish Academy. We have inserted it in the *Annals of Philosophy* to show the coincidence of the sentiments with those of Mr. Turner, of which an account was given in the review of the last volume of the Manchester Memoirs in vol. xv. p. 193, of the *Annals of Philosophy*.

tion from heaven. And when we consider the prodigious sagacity, the wonderful powers of discrimination, the profundity of thought, and the almost infinite comprehension requisite to analyze words into their component parts—sounds appearing simple, into sounds still more simple—to discover that the multitude of words in a language are composed of a very small number of sounds—to ascertain precisely this small number, and to annex a mark to each, we shall not be astonished that the eminent men to whom I have alluded, should deem the human mind incapable of such an effort, and esteem it necessary to cut the knot by a miraculous intervention of the Deity.

Hartley conjectures that the communication was first made to Moses on the delivery, at Mount Sinai, of the two tables which the sacred historian declares to have been written by the finger of God;* and Wakefield (who, still less than Hartley, can be suspected of weakness or credulity) supports a similar opinion by very convincing arguments,† without adverting, however, to the particular occasion on which the revelation was made to the Hebrews, or even referring to the discussion of Hartley on the subject. But so great are the achievements, so extensive the dominion attempted and attained by the mind of man, that I am more inclined to solve the difficulty in a natural way by ascribing the discovery to those exertions which have hitherto found a specific for every want, as soon as the want was felt, rather than assume, without manifest necessity, a departure from the course of nature, and those laws by which Providence visibly governs the world.

It may be said that when alphabetic writing was first adopted, there was not such a want of the means of recording events, opinions, and laws, as would spur the human mind to the exertions requisite to this great discovery. Mankind must have been in possession of picture writing, hieroglyphics, or that improvement upon them which exists at present among the Chinese, and these were sufficient for all their purposes. They are, it is true, greatly inferior to the alphabetic mode of writing; but as the former could never improve into the latter, as they are each perfectly distinct and unconnected in their kinds, as one represents things and ideas, and the other neither things nor ideas in the first instance, but sounds, how could the former mode be abandoned, and the latter adopted, when the one though inconvenient seemed fitted to all the wants of the writer; and the other was not only untried, but was even of such a nature, as that no sagacity could conjecture its utility until subjected to reiterated trial?

It has been ascertained that the nations bordering on China, and which speak a different language, can read and understand the Chinese when written, though they cannot comprehend a

* Hartley on Man, 1st. v. 308, 8vo. edition.

† Second Appendix to Gilbert Wakefield's Life.

word of it when spoken ;* and in perusing a Chinese work, it is their own language they pronounce, and not that of China ; and this because the characters represent things and ideas—not sounds. We should, therefore, be inclined to suppose that two natives of China might use different words, provided their meaning was similar, in reading the same passage. For example ; one man, according as his style was familiar, formal or easy, compressed or diffuse, might read certain characters thus :— “ The deeds of talented men weigh more than the precepts of wise ones.” Another : “ Genius that acts is of more importance than Wisdom that declaims.” A third : “ The actions of the able are superior in value even to the words of the wise.” And there can be little doubt but that this was the case in the infancy of their written language. It has now, however, arrived at the highest perfection of which, perhaps, it is susceptible. The prodigious number of its words, for each of which there is a separate character, comprehends a multitude of synonymes. Most of the words are monosyllables ; and as each is designated by a distinct character, the sound is as perfectly ascertained as if it was intentionally represented ; and the only inconvenience (but which is almost an insurmountable one) is the incredible number of characters that become necessary, when every word must have its own peculiar representative.

There was a time, however, among the Chinese, Egyptians, and other nations, using similar symbols, when the art of writing was yet in its infancy, and its progress in improvement but little advanced. At such a period, the characters must have been confined in the strictest sense to the representation of things and ideas only ; and in pronouncing them, any word might be used in the place of one which was synonymous. The meaning might still be certain, though not so precise as if sounds had been represented by those characters ; yet there could not be that strong necessity for the signs of sounds, which would be sufficient to instigate the mind to labour after so profound and recondite a discovery. What then, it may be asked, could create this strong necessity ? I reply, in a word, Poetry ; and Poetry alone.

Circumstances may be picturesque and ideas poetical, but they do not constitute poetry, unless they are clad in the language of the Muses. The harmonious flow of sounds is the very essence of a poem ; and to fix and consolidate their volatile and evanescent nature, to give them stability, and render them permanent, can only be accomplished by marks which represent them ; and not by the symbols of ideas, or the pictures of things. Hieroglyphics, or the improved characters to which hieroglyphics in the first instance gave birth, could never have become the record of an Iliad or Æneid. An alphabet was necessary to

* Staunton's Account of the Embassy to China, vol. iii. p. 420, second edit.

preserve them for succeeding ages. Nay, without an alphabet, they would perhaps have perished during the very life of the poet who produced them; or possibly the most admired of his episodes, would have continued their ephemeral existence only so long as his memory could retain them. Let us then imagine a Homer, a Virgil, or a Milton, carried away by his sublime conceptions and the melody by which he gave them utterance, satisfied that they were worthy to excite emotions of delight and wonder in the latest posterity, yet persuaded that they were destined to vanish from existence, even at the moment they flowed from his lips: let us imagine those circumstances, and we shall have no difficulty in conceiving how an enthusiastic individual impressed with these emotions would exert every power of his intellect, to preserve from annihilation compositions which ought to be immortal. The motive would be sufficient for any labour of invention however surprising; and it may not be an uninteresting employment to discover, if possible, the progressive steps by which the task might have been accomplished.

Whether the inventor was of India, Chaldea, Phœnicia, or Egypt, he has not succeeded in transmitting to our days the verses which he intended to immortalize; or even a name that might justly rank, among the most illustrious of mankind, beyond that of Homer himself. Yet in the place of those works, for whose celebrity he perhaps alone was interested, he has bestowed upon us a gift of more value and interest than an hundred Iliads. Not that it perpetuates those divine compositions which have humanised man, ameliorated his nature, and elevated his character with the traits of nobleness and magnanimity; not that it has enabled him to record with unequivocal precision his observations and sentiments, and to argue, discuss, and ascertain, with pre-eminent accuracy, every shade of probability, and limitation of truth within the cognisance of his faculties. Not that it facilitates his progress in natural, moral, or intellectual philosophy, and the discovery of those simple and admirable laws by which the earth and the universe, matter and mind, are so wonderfully governed; but because this extraordinary gift, which I am almost tempted to call divine, even while I am proving it to be human, opens at once the doors of knowledge to all mankind: roots up the labyrinths of darkness that surrounded every temple of science; and admits, not the philosopher, and the legislator only, but the citizen, the mechanic, the rustic, and the labourer; nay, the whole mass of society, civilized or dawning into civilization, within these portals, from which, without its simple but powerful assistance, it must have been helplessly and hopelessly excluded.

How little did the original inventor conceive the inherent powers of his invention, destined to be one day multiplied ten thousand fold, by the less profound, but no less important, dis-

covery of printing. Little did he imagine that he had set a force in operation which was to overturn ignorance and barbarism in every class of society, and we may venture to predict, in every horde of the species, however at present degraded, and pour over the surface of the globe in irresistible streams, the blessings of knowledge, liberty, and happiness.

But let us turn from the sublime effects to the lowly instrument and unconscious prime-mover. Let us endeavour to analyze the process, by which a human being might have contrived an arrangement, which, collectively regarded, might not unreasonably be deemed beyond the reach of human ingenuity, yet, when elucidated by the analysis which shall be immediately explained, the invention, it is believed, will appear not merely a matter of possibility, but an event of natural occurrence.

Let us begin the investigation by tracing, if we can, the mode of proceeding which might have been adopted by any of our great epic poets, if he had not the means of recording his verses, yet was anxiously bent on transmitting them to posterity. Let us suppose Homer, Virgil, or Milton, labouring under this predicament, and endeavouring distinctly to note down the successive sounds which compose the first verses of the Iliad, Æneid, or Paradise Lost. Let us select one of this immortal triumvirate; and as the Latin language is more uniform in its sounds than the English, and the Roman letter more convenient than the Greek for combining two or more characters into one, as will be found serviceable in explaining the process of the invention, let the Roman poet on the present occasion be our guide.

Having composed and committed to memory the first four lines of the Æneid, he would have no difficulty in dividing the words into syllables as he pronounced them.

Ar-ma vi-rum-que ca-no, Tro-jæ qui pri-mus ab o - ris
1. 2. 3. 4. 5. 6. 7. 8. 9. 10. 11. 12. 13. 14. 15.

I - ta - li - am, fa - to pro - fu - gus, La - vi - na - que ve - nit
16. 17. 18. 19. 20. 21. 22. 23. 24. 25. 26. 27. 28. 29. 30.

Li - to - ra: mul-tum il - le et ter - ris jac-ta - tus et al - to,
31. 32. 33. 34. 35. 36. 37. 38. 39. 40. 41. 42. 43. 44. 45. 46.

Vi su - pe - rum, sæ-væ me - mo-rem Ju-no - nis ob i - ram.*
47. 48. 49. 50. 51. 52. 53. 54. 55. 56. 57. 58. 59. 60. 61.

But as every syllable would seem a simple sound, he would be under the necessity of noting each by a separate character. In the lines just quoted, he would find 61 syllables, but as 13 of them are repetitions, 48 characters would suffice to designate them. Having sufficiently familiarized himself with these cha-

* Numbers are placed under the several syllables that the reader may trace them in the subsequent tables.

acters, so as to recollect the sound by the sign, and the sign by the sound, he would naturally group together those signs which represented sounds bearing the slightest degree of resemblance to each other. In this process he would find his 48 characters arrange themselves in five columns, perhaps in the order in which I shall represent them. He might have chosen different configurations, straight or curved, simple or complicated, for his characters; but I shall select for mine such as will remind my reader of the sounds which they are intended to represent.

| | | | | | | | | | |
|--------|-----|----------|------|-------|------|--------|------|-----------|------|
| 1 | AR. | 3.26.47. | VI. | 4.50. | RUM. | 5.23. | QUE. | 7.57. | NO. |
| 2. | ME. | 10. | QU. | 12. | MIS. | 9. | AE. | 8. | 'RO. |
| 6. | CA. | 11. | PRI. | 23. | FU. | 29.52. | VE. | 14 | O. |
| 13. | AB. | 15. 40. | RIS. | 24. | GUS. | 37. | LE. | 31.32.46. | TO. |
| 17.42. | T. | 16. 60. | I. | 34. | ML. | 38.44. | FI. | 22 | PRO. |
| 19. | AM. | 18. 31. | LI. | 35. | TM. | 39. | FR. | 54. | MO. |
| 20. | E. | 20. | NI. | 42. | SU. | 43. | PE. | 52. | OB. |
| 25. | A. | 26. | IL. | 43. | TUS. | 51. | SAE. | | |
| 27. | M. | 58. | NS. | 56. | JL. | 53. | ME. | | |
| 33. | RA. | | | | | 55. | REM. | | |
| 41. | AC. | | | | | | | | |
| 45. | A. | | | | | | | | |
| 61. | RM. | | | | | | | | |

It is scarcely necessary to observe that in forming this arrangement he would naturally place, as I have done, the characters in the order in which the sounds occurred in the verses he was endeavouring to analyze. For example, he would place *Ar* in the first column of the table; *ma* in the same column, *vi* in the second column, *rum* in the third, *que* in the fourth, *ca* in the first again, and *no* in the fifth; and the prevailing sounds in the respective columns would not be in the order of our vowels, but thus: a. i. u. e. o. and with respect to the situation of the characters in each column, they would be nearer the top or bottom in proportion as the sounds they represented were near the beginning or end of the verses submitted to this process. I have only to add that a person accustomed to pronounce the Latin language after the English fashion, may object to my arranging together sounds so dissimilar as *vi* and *ris*, *fu* and *mus*; but there are strong grounds to believe that they were not so pronounced by Virgil. The Italians, the French, the Spanish, the Portuguese; in fact, all those nations whose languages are derived to any extent from the Latin, for the most part agree in pronouncing *a* as in *all*, *i* as in *ill*, *o* as in *ore*, and *u* as in *pure*; *e* sometimes as in *revere*, and sometimes as in *revery*; and their evidence on this point has the force of four witnesses,

testifying and corroborating the testimony of each other, that the Romans, from whom they derive their languages, transmitted to them also this mode of pronouncing them.

But to return to Virgil, and the task we have assigned him :—In separately studying the columns, as above arranged, he would perceive several sounds very closely resembling each other, yet at the same time distinctly different. I allude to the syllables which rhyme to each other, such as *ma, ca, fa, la* ;—*no, to, pro, mo, &c.* He would clearly discover the simple sound *a* or *o* which he could distinctly pronounce, pervading all those rhymes ; and he would at the same time ascertain that it was in every instance combined with another and different sound, which he could not separately articulate, but which, like an invisible elastic fluid in a chemical investigation, would satisfy him of its presence by its effects.

Now for the first time he would entertain sanguine hopes of success ; and in the ardour of discovery he might possibly hasten to dissect, and assign distinct characters to the sounds in his several columns : or perhaps he would more systematically make a new arrangement of those columns, placing together in separate groups the several rhymes, for the purpose of entering on a more minute comparison of their component sounds. If such was a necessary step in the process, the following table may exhibit his new arrangement :

| | | | | | | | | | |
|--------|-----|----------|------|-------|------|--------|------|-----------|------|
| 1. | R. | 3.26.47. | VI. | 4.50. | RUM. | 5.28. | QUE | 7.57. | NO. |
| | | 10. | QU. | 35. | TM. | 9. | Æ. | 8. | RO. |
| 2. | M. | 11. | PR. | | | 23.52. | VE. | 14. | O. |
| 6 | CA. | 16. 60. | I. | 12. | MIS. | 27. | LE. | 21.32.46. | TO. |
| 17.42. | T. | 13.31 | LI. | 24. | GUS. | 49. | PE. | 22. | PRO. |
| 20. | F. | | | 43. | TUS. | 51. | SÆ. | 54. | MO. |
| 25. | L. | 15.40. | RIS. | | | 53. | ME. | | |
| 27. | N. | 58. | NS. | 23. | FU. | | | 59. | OB. |
| 33. | RA. | | | 42. | SU. | 32.44. | ET. | | |
| | | 50. | NT. | 56. | JI. | | | | |
| 13. | B. | | | | | 39. | TR. | | |
| | | 36. | IL. | 32. | ML | | | | |
| 19. | M. | | | | | 55. | REM. | | |
| 61. | RM. | | | | | | | | |
| 41. | AC. | | | | | | | | |
| 45 | A. | | | | | | | | |

In considering the first of the five columns in this table, he would scarcely in his early essays be able to ascertain whether the sound *Ar* at the head of the column were simple or com-

pound, not having another sound of sufficient resemblance with which to compare it. He would, therefore, proceed to the next sound *ma*, and would find ample scope for his sagacity in comparing it with the six succeeding sounds: *ca, ta, fa, la, na, ra*; and surely it would not require the inspiration of heaven to enable him to discover on comparing them that the sound *a* existed in each, combined with another sound which could not be articulated without it. These indistinct sounds his mind could, however, embrace and discriminate. To make sure of his discovery he would affix a character to each, as well as to the articulate sound with which it was combined; and the first fruits of his labour would be the letters *a, m, c, t, f, l, n, r*. The discovery of the last mentioned letter would enable him to return to the analysis of the sound which he passed over at first; and as he had succeeded in finding that *ra* was composed of the sounds indicated by an *r* and an *a*, he would readily perceive that *Ar*, the first syllable of *Arma*, was but a transposition of the same sounds in the order *a, r*.

This minute circumstance can be but little interesting at present; yet no doubt it was of the highest importance to the inventor of the alphabet; for it furnished him with a test to discover whether a sound were simple or compound. In attempting to repeat in every possible way any one of those sounds that have since been called vowels, it still remains the same, and cannot be inverted. It is, therefore, a simple sound. But combine it with another, and the compound admits of an easy inversion: *ma* becomes *am*, *la* becomes *al*, &c. After this discovery, the only difficulty that could occur in considering the first column would be the analysis of syllables composed of three sounds. The first that presents itself is *ram*, the last syllable of *iram*, and this is no longer a difficulty. It has already been analyzed; and the sounds of which it is composed arranged in the incipient alphabet under the characters *r, a, m*.

The same simple procedure would unravel the mysteries of the remaining four columns. To every sound he would affix its appropriate mark; and to every mark he would assign but the power of representing one sound. No diphthongs would enter into his system. They appear it is true in the verses selected from Virgil. But the Romans did not invent their own alphabet; they borrowed it from the Greeks, as the Greeks borrowed theirs from the Phœnicians; who, it may be said, are indebted for their alphabet to the Hebrews, as the old Hebrew or Samaritan characters are nearly the same as the Phœnician. It may still, however, be considered a disputable point, whether these people did not communicate their alphabet to the Hebrews, as well as to the Greeks. But the original inventor of the alphabet, whatever was his country, would naturally mark every sound in his language by a single character; and those which are distin-

guished at present by the diphthongs *æ* and *æ* he would indicate simply by the character *e*.

It is remarkable that the analysis of the first four verses of the *Æneid* would have furnished its author, not only with all the vowels, but with 14 consonants—a number almost equal to that which exists in the modern alphabet. Were the same process to be exercised on the succeeding verses, the deficiency no doubt would soon be supplied, and all the Latin characters appear in perfect array. Those elicited by the process we have just been supposing would naturally occupy the following order, if taken as they appear in the last arrangement of the columns, beginning with the first and terminating with the fifth.

| | | |
|-------|-------|--------------|
| 1 A. | 14 I. | |
| 2 M. | 15 P. | |
| 3 C. | 16 S. | |
| 4 T. | 17 G. | |
| 5 F. | 18 E. | |
| 6 L. | 19 O. | |
| 7 N. | | |
| 8 R. | 20 D. | } Deficient. |
| 9 B. | 21 H. | |
| 10 J. | 22 K. | |
| 11 V. | 23 W. | |
| 12 Q. | 24 X. | |
| 13 U. | 25 Y. | |
| | 26 Z. | |

And a glance of the eye will convince us that even these without any addition constitute an alphabet very nearly perfect: for with respect to the characters deficient, *d* is but the softened sound of *t*: *h* but an aspiration: *k* has the same power as *c* hard: *w* is not necessary in Latin, or the languages originating from it: *x* is a compound of *c s*: *y* differs little in its powers from *i*: and *z* is but a softened *s*.

Every step the inventor advances, the easier he finds the succeeding. Having travelled through the analysis of the first two columns, he has already discovered three vowels and 13 consonants; in the remaining three columns he has only two vowels and one consonant to add to his possessions, for all the other sounds, whether simple or complex, to be found in those columns, have already been examined and arranged in his alphabet.

At this stage of the process, we cannot but reflect on the astonishment he must have experienced on thus discovering the paucity of simple sounds existing in the complicated variety of a language. Sixty-one syllables, containing 48 distinct sounds, are reduced and simplified into 19; and these 19 are found in prosecuting the investigation to be nearly sufficient to represent the entire of a language. He might have previously reasoned

himself into a conjecture that the simple sounds were much fewer in number than the compound ; but until the completion of the experiment, the most sanguine imagination could never have approached within many degrees of the truth.

Thus might a single individual have brought to perfection this wonderful discovery. Indeed we are almost compelled to admit from its nature, that it could only have been achieved by a single individual. Any progress one man might make before his ideas were completely developed could not possibly tend to assist any other. It is then natural to presume that we owe to the self-same mind, the conception of the plan ; every stage of the process ; and its perfect and final accomplishment, in so far at least as the compass extended of the sounds which he had occasion to represent : and from this original invention, it can scarcely be doubted, were copied all the alphabets entitled to the name which ever existed.

Astle, it is true, whose laborious and extensive researches on the subject entitle his opinion to the utmost attention and deference, asserts that several of the Asiatic alphabets, which differ in the names, number, and power of the characters from the Phœnician, and those of which it was the source, must have been altogether of a separate and undervived origin ; and that it is most liberal as well as rational to suppose that different men at different times thought of making marks for sounds, instead of marks for things.* But strong as are the facts which he adduces, I am unable to divest myself of the opinion that the author of any alphabet, posterior to that originally invented, must have been previously acquainted either with it or some other derived from it ; and that where no traces of similitude are observable that he adopted at least the principle on which those prior alphabets were formed ; rejecting their characters either on account of some difficulty in applying them to a language requiring very different powers to designate its sounds ; or perhaps from the vanity of encountering greater difficulties, and a wish to be considered an original inventor. Yet any man's ambition ought to be sufficiently gratified by the mere act of bestowing so inappreciable a gift upon his nation. What would not the Chinese owe to the individual who could persuade their government to encourage the use of our characters in place of that cumbersome and unmanageable machinery, which, if not the sole, has been the principal obstacle to their progress in the fine arts, literature, and science, the cultivation of the mind, and the intellectual embellishment of society.

It may be said that what has been done once may be done again. But so many circumstances must have concurred in the discovery of alphabetic writing, even in the simplified view we have been considering, that it seems to present at least one

* See the fourth chapter of Astle on the Origin and Progress of Writing.

obstacle to the general application of the remark. If there be any plausibility in the process I have detailed, we can scarcely suppose that a plurality of individuals could have arisen in different ages and nations, qualified for the accomplishment of such a task : all of them poets, all producing poems worthy, at least in their own estimation, of descending to after ages, all eager and enthusiastic to find a certain and permanent record for their verses : all, thus prompted to the undertaking, in possession at the same time of the means of carrying it into execution : a comprehension which could survey the intricate mazes of a language, and suspect that the whole might be reduced to a few simple sounds—a judgment that could decide on the possibility of designating those sounds, which inferior powers of discrimination would deem as difficult to delineate as pictures of odours, and tastes, and internal sensations : a sagacity and genius fit to discover seeming impossibilities to be possible—and an indefatigable and pertinacious perseverance, *that* most efficient attribute of great minds, which demonstrates the possibility of an enterprise by its actual performance.

If this combination of circumstances, motives and qualifications must have concurred in the production of alphabetic writing, is it probable that such a conjunction should occur more than once ? I admit that it is possible, but to my understanding, it is equally possible that a knowledge of the circulation of the blood, and the laws of gravitation, might also be discovered in different countries and times ; and that Harveys and Newtons are to be esteemed but common productions of nature.

If it be admitted that the elementary alphabet may have owed its birth to poetry, it must also be confessed that the syllabic alphabets of the Ethiopians and Tartars * must have had a similar origin. It might, therefore, be supposed that long after the invention of a syllabic alphabet, it might have served as a step to the invention of the other. But this conjecture, though a natural one, seems not to be well founded. The powerful mind that invented alphabetic writing could have derived but little assistance from so weak an auxiliary. It would have been but an impediment to his progress ; and by furnishing him with the means, however operose and unwieldy, of transmitting his verses to posterity, would have deprived him of the strongest incentive to the attempt.

This, however, must remain a doubtful question ; but it is easy to perceive that in refining on the discovery of the original inventor other ingenious persons may have contributed additional letters if other sounds should be detected which he had omitted to note ; or adopting the principle and rejecting the characters, applied a new set of those arbitrary signs to represent some other language with which they harmonized better ;

* See Goguet's Origin of Laws, Arts, and Sciences, vol. i. p. 178 ; and Rees's Encyclopedia, article Alphabet.

while others may have formed a new arrangement of the original characters in support of some system, or for the purpose of more easily instructing the ignorant. Yet when we look over the generality of alphabets, nothing like system or arrangement appears. Vowels and consonants, liquids and mutes, the representatives of every sort of sound, whether labial, dental, palatine, or nasal, are hustled together, without distinction or order, apparently as they issued at first from the brain that conceived them. Nor would this be an unimportant observation were we fortunate enough to possess many of the writings of the primitive times; because it would furnish a test for discovering the most ancient production in which alphabetic writing was used if it happened that such a production were still in existence. For if on an analysis of the first lines of the work into their elementary sounds, by the process which I have endeavoured to describe, the characters representing those sounds should arrange themselves in the order of the alphabet—I mean the alphabet of the language in which the work was composed—little doubt could remain that alphabetic writing was used for the first time in recording that very composition; and was invented in the anxiety of its author to snatch from dissolution the perishable sounds of which it consisted.

Such an expectation is not now to be entertained; and even if it might, the investigation could scarcely be desirable, except to a mere antiquarian. A similar remark, perhaps, may be made upon the discussion that has already detained us so long. I have nothing to say in its defence. It examines a question of mere idle curiosity; and is scarcely interesting even to a few. It is useless and unnecessary to any purpose or end; unless, indeed, it may be supposed of advantage to open a more favourable view of the powers of the human mind, and that it shall be considered, not altogether fruitless or unserviceable to convince the unprejudiced, even by a single instance, that the best and only mode of overcoming similar difficulties is to persuade ourselves that they are not insurmountable; and to encounter them by patient discrimination, and gradual, slow, and circumspect induction, satisfied that it is unphilosophic, inconsiderate, and puerile to disentangle every perplexity, by resorting to miraculous interposition, where a little sagacity will reduce the achievement to the exertions of that reason, and those energies with which the Creator in his munificence has endowed mankind.

ARTICLE VI.

Comparative View of Mean Temperature at different Places in Great Britain. By Dr. John Forbes.

(To Dr. Thomson.)

DEAR SIR,

Penzance, Sept. 12, 1820.

IN composing a paper on the Medical Topography of this place, and the neighbouring district of the *Land's End*, in which I have lately been engaged, I was naturally led to compare the temperature of Penzance with that of other places in different parts of the island. The following table exhibits a few of the results to which this inquiry led; and will, I hope, appear sufficiently interesting to you to entitle it to a place in your journal. I likewise send a note of the variation of temperature observed by me during the eclipse of the sun on the 7th, which, I doubt not, will be satisfactory, on account of the geographical position of the place of observation.

I am, dear Sir,

Your faithful servant,

JOHN FORBES.

Note of the Variation of Temperature during the Eclipse of the Sun, Sept. 7, 1820, observed at Penzance.

| Time of observation. | Fahrenheit's thermometer. |
|----------------------|---------------------------|
| 11 ^h 34' | 65·0° |
| 12 30 | 65·0 |
| — 40 | 65·0 |
| — 45 | 65·0 |
| — 55 | 66·0 |
| 1 — | 66·0 |
| — 10 | 65·7 |
| — 25 | 65·0 |
| — 30 | 64·5 |
| — 40 | 64·2 |
| — 50 | 64·0 |
| 2 05 | 64·5 |
| — 15 | 64·0 |
| — 25 | 64·5 |
| — 45 | 65·0 |
| — 55 | 65·5 |
| 3 — | 65·7 |
| — 30 | 65·0 |

Temperature by Register Thermometer.

| | Max. | Min. |
|---------|------|------|
| Sept. 6 | 64° | 58° |
| 7 | 65 | 52 |
| 8 | 65 | 50 |

Comparative View of Mean Temperature at different Places in Great Britain.

| | Time of observation. | MEAN TEMPERATURE. | | | | | | | | | | | | MEAN TEMPERAT. | | | VAR. OF TEM. § | | | | |
|--|----------------------|-------------------|------|------|---------|--------|--------|---------|-------|-------|---------|-------|------|----------------|---------|---------|----------------|---------|-------------|---------------|--------------------------------------|
| | | WINTER. | | | SPRING. | | | SUMMER. | | | AUTUMN. | | | Winter. | Spring. | Summer. | | Autumn. | Whole year. | Annual range. | Mean variation of successive months. |
| | | Nov. | Dec. | Jan. | Feb. | March. | April. | May. | June. | July. | August. | Sept. | Oct. | | | | | | | | |
| Isle of Wight, 10 yrs. (1809—1818)* | a.m. 9 | 44 | 39 | 37 | 41 | 44 | 46 | 56 | 62 | 65 | 62 | 51 | 40 | 43 | 61 | 57 | 50 | 25 | 4.66 | | |
| + Penzance, 11 yrs. (1807—1817) † | 8 + | 45 | 42 | 40 | 42 | 43 | 46 | 53 | 57 | 59 | 59 | 50 | 42 | 43 | 56 | 55 | 49 | 19 | 3.16 | | |
| Sidmouth, 2 yrs. (1813—1814) § | 8 | 44 | 41 | 34 | 40 | 43 | 50 | 54 | 60 | 64 | 63 | 59 | 39 | 39 | 59 | 57 | 50 | 30 | 5.00 | | |
| Exeter, 5 yrs. (1814—1818) | 8 | 43 | 37 | 34 | 38 | 41 | 44 | 51 | 57 | 59 | 57 | 53 | 47 | 38 | 41 | 55 | 46 | 25 | 4.16 | | |
| Gosport, 4 yrs. (1816—1819) * * . . . | 8 | 43 | 37 | 39 | 39 | 41 | 46 | 52 | 60 | 62 | 61 | 57 | 49 | 39 | 42 | 58 | 48 | 25 | 4.16 | | |
| London, 3 yrs. (1817—1819) † | 8 | 45 | 36 | 38 | 39 | 40 | 45 | 52 | 59 | 61 | 60 | 56 | 44 | 39 | 41 | 57 | 48 | 25 | 4.16 | | |
| Kinfauns, 6 yrs. (1813—1818) † † . . . | 8 | 39 | 34 | 32 | 36 | 37 | 42 | 48 | 54 | 57 | 55 | 51 | 35 | 38 | 53 | 50 | 43 | 25 | 4.16 | | |

* Original journal, by — Kirkpatrick, Esq.
 † I have stated the time here at eight, a. m. but the observations were taken at seven, a. m. in all the months except December and January. Of course, some allowance ought to be made on account of the earlier (and colder) period of observation at Penzance.
 ‡ Original journal, by Thomas Giddy, Esq.
 || Original journal, by E. P. PITCHER, Esq.
 * * Mr. Cary, in Philosophical Magazine.
 † † These two columns are formed from the mean temperature as given in the preceding part of the table.
 § Dr. Clarke, in *Annals of Philosophy*.
 * * Dr. Burney, in *Annals of Philosophy*.
 † † Thomson's *Annals*.

ARTICLE VII.

Observations on the late Solar Eclipse. By Mr. James Fox.

(To Dr. Thomson.)

SIR,

Plymouth, Sept. 23, 1820.

Lat. $50^{\circ} 22' 24''$ N. Long. $4^{\circ} 12' 45''$ W.

BELIEVING the recent great eclipse of the sun must have excited general attention, I am induced to send you the result of some observations I made with a view of ascertaining its effect on the temperature of the atmosphere, as well as to note the precise time of its commencement and duration, as seen from this place. The method taken to obtain the temperature was as follows :

For three successive days, viz. the 6th, the 7th (the day of the eclipse), and the 8th inst. I placed a Fahrenheit's thermometer, as well as a Leslie's differential one, *in the sun*, and also another of Fahrenheit's *in the shade*, and noted their respective indications at intervals of five minutes. The great coincidence of the thermometer in the shade on these days (except during the time of the eclipse) afforded a very satisfactory proof that the temperature of the period was nearly equal, and consequently became a tolerably good standard by which the loss of heat at the time of greatest obscuration might be ascertained. By this it appeared that the loss of heat at the time of greatest obscuration,

| | |
|--|------|
| By Fahrenheit in the shade, was | 4.5° |
| By ditto in the sun | 13.0 |
| By Leslie's differential thermometer in the sun. | 16.0 |

By means of a three and a half feet telescope, and a watch accurately adjusted by repeated solar observations, I found the eclipse to *end* precisely at $2^{\text{h}} 58' 56''$ p. m. *apparent* time. I am sorry clouds intervened at its commencement so as to prevent my seeing it until too late to note the time.

The loss of light did not appear so great as was expected : the darkness did not exceed that of a very cloudy day.

I am, Sir,

Your obedient servant,

JAMES FOX.

ARTICLE VIII.

Letter from Mr. Thomas Gill, Chairman of the Committee of Mechanics in the Society for the Encouragement of Arts, &c. &c. of London, to Prof. Thomson, on a pretended Self-moving Engine.

*Patent Agency Office, No. 125, Strand, London,
Oct. 4, 1820.*

SIR,

MY attention has been lately directed to a gross imposition upon the public in a pretended *self-moving engine*, which is now exhibiting in this metropolis, and which I think it highly proper to expose; and thereby, as far as lies in my power, prevent the delusion from being continued.

It is announced in the following hand-bill:

“The newly-discovered Patent Self-existing Engine for propelling ships at sea, carriages on the road, and all kinds of machinery, without the aid of horse, steam, water, or other power, now in use. To be seen at work, every day, Sundays excepted, from ten in the morning till six at night, at No. 32, Burlington Arcade, Piccadilly. Admittance, Two Shillings.

“N.B. Gentlemen intending to have their machinery driven by the above power are desired to apply as above, if by letter, post paid.”

I found the machine to consist of a light brass wheel, about two feet in diameter, turning upon an horizontal axis, which is supported at each end in square blocks upon the tops of two brass columns, which are affixed to a mahogany table mounted upon a thick pillar of the same wood, with feet and rolling castors, so that it may be moved about, and thereby show that it has no communication through the floor of the room it is exhibited in. The thickness of the table is about an inch and a half, and the pillar is about six inches in diameter. Around the periphery of the wheel are fixed at equal distances by screws a number of small cylindrical rods or bars of metal which are placed parallel to the axis of the wheel; and at one end of the frame surrounding the wheel (and which frame is besides supported on two other brass columns) is a brass pillar, which has an arm or bracket, on which is fixed near the periphery of the wheel a ball, which the inventor pretends is formed of a new combination of metals, the composition of which he keeps a profound secret, and which, he says, possesses a new species of attraction, which is not magnetical, for the metal bars on the periphery of the wheel so as to draw each of them in succession continually towards it; but as this attraction takes place equally above and below the ball, he says that he cuts it off below by means of a plate of a different composition of metals, which is placed beneath the ball, and thus he pretends that the wheel is continually turned round by this new attempt at reviving the long

since exploded doctrine of attraction and repulsion, and with considerable force, and says that he has in the country a machine of two horses' power. On being asked if he had any pieces of the compound metals with him besides those on the machine, he said he had not, and would not suffer any person to possess them, lest they should take them to pieces, and thereby discover their composition. On being questioned as to his patent, he said that in fact he had none, having merely entered a caveat to prevent any persons from taking out a patent for a similar machine without his being informed thereof. So much then for his pretended patent; and I verily believe that his new invented power rests on no better a foundation; and that in fact his wheel is turned by a spring mounted on a barrel, as usual in spring clocks, and other pieces of mechanism, and which is concealed, either in the substance of the table itself, or in the thick pillar which supports it, the barrel having a ring of teeth around it, working in a pinion, affixed upon the lower end of an upright axis concealed in one of the brass columns that support the horizontal axis of the wheel, and having at its upper end another pinion which works into a small toothed contrate wheel fixed on the end of the horizontal axis, but also concealed in the brass block on the top of the column, and in another pillar which is screwed upon the top of the block, and is quite large enough to contain it, and thus gives motion to the wheel, and indeed the artifice is but very clumsily concealed. I should have added that the brass pillar last mentioned supports an upright axis having a pinion at its lower end, which is driven by a contrate wheel on the horizontal axis of the wheel, and which upright axis has at the top of it a fly with wings to regulate the motion of the machine, as in other spring movements.

This barefaced imposition reminds me of another which was practised upon the public several years since, and was detected, and very properly exposed, by my friend Mr. J. T. Hawkins, which put an end to the trick: this consisted of what was said to be a *self-moving pendulum*, and beneath the ball of it a bottle was placed, out of which, as the exhibitor pretended, an elastic fluid was continually proceeding, which gave an impulse to the ball as it passed over it. Mr. Hawkins, however, found that the impulse was in fact given to the pendulum at its upper end; and that the axis on which it hung communicated with another pendulum which was kept in motion by a weight or other maintaining power; for in like manner the real cause of the wheel's motion in this new imposition is concealed; and the attention of the spectator is directed to another pretended first mover, and which is besides attended with the additional advantage of affording him matter for wonder, which constitutes the chief pleasure of the multitude, and contributes greatly to the profit of the exhibition. I am, Sir, your most obedient servant,

THOMAS GILL.

ARTICLE IX.

New Electromagnetic Experiments. By Prof. Oersted.

SINCE the publication of my first experiments on the magnetic action of the galvanic battery, I have multiplied my researches on that subject as much as a multitude of other important avocations put it in my power.

The magnetic effects do not seem to depend upon the intensity of the electricity, but solely on its quantity. The discharge of a strong electric battery transmitted through a metallic wire produces no alteration in the position of the magnetic needle. A series of interrupted electric sparks acts upon the needle by the ordinary electric attractions and repulsions, but as far as can be perceived, the sparks produce no electromagnetic effect. A galvanic pile composed of 100 discs of two inches square each metal, and of paper moistened with salt water to serve as a fluid conductor, is likewise destitute of sensible effect upon the needle. On the other hand we obtain the effect by a single galvanic arc of zinc and copper having for a conductor a liquid possessed of great conducting power; for example, of one part sulphuric acid, as much of nitric acid, and 60 parts of water. We may even double the quantity of water without much diminishing the effect. If the surface of the two metals is small, the effect is likewise small. But it augments in proportion as we augment the surfaces. A plate of zinc, of six inches square, plunged into a vessel of copper containing the liquid conductor of which I have spoken, produces a considerable effect. But an arrangement of this kind in which the zinc plate has a surface of 100 inches square acts upon the needle with such force that the effect is very sensible at the distance of three feet, even when the needle is not very moveable. I have not observed greater effects from a galvanic apparatus composed of 40 similar troughs; indeed the effect seemed less great. If this observation, which I have not investigated expressly, is just, I shall be of opinion that the small diminution of the conducting power produced by increasing the number of the elements of the apparatus weakens its electrochemical effect.

To compare the effect of a single galvanic arc with that of an apparatus composed of several arcs or elements, let us make an observation. Let fig. 9 (Pl. CIX), represent a galvanic arc composed of a piece of zinc *z*, of copper *c*, of a metallic wire *ab*, and of a liquid conductor *l*. The zinc always communicates a portion of its positive electricity to the water as the copper does of its negative electricity. This would occasion an accumulation of negative electricity in the upper part of the zinc, and of positive electricity in the upper part of the copper, unless the communication *a b* re-established the equilibrium by affording a free

passage for the negative electricity from z to c , and for the positive electricity from c to z . We see then that the wire ab receives the negative electricity of the zinc, and the positive electricity of the copper, while a wire that constitutes the communication of the two poles of a pile, or of another compound galvanic apparatus, receives the positive electricity of the zinc pole, and the negative electricity of the copper pole.

By attending to this distinction, we may, with a single galvanic arc, repeat all the experiments which I had at first made with a compound galvanic apparatus. Employing a single galvanic arc gives this great advantage, that it enables us to repeat the experiments with little preparation and expense. But it presents another advantage still more considerable; namely, that we may establish a galvanic arc sufficiently powerful for the electromagnetic experiments, and yet sufficiently light to be suspended to a small metallic wire, in such a manner that the small apparatus may be made to turn round the prolonged axis of the wire. We may in this way examine the action which a magnet exerts on the galvanic arc. As a body cannot put another in motion without being moved in its turn, when it possesses the requisite mobility, it is easy to foresee that the galvanic arc must be moved by the magnet.

I made use of different arrangements of the simple galvanic apparatus to examine the motion impressed on it by the magnet. One of these arrangements is represented in fig. 10, which represents a perpendicular section of it in the direction of the breadth. $cccc$ is a trough of copper, three inches high, four inches long, and half an inch broad. These dimensions doubtless may be varied to infinity. It is only necessary to observe that the breadth ought not to be great, and thus the trough should be made of plates as thin as possible. zz is a plate of zinc. ll are two pieces of cork which keep the plate in its position. $ffffz$ is a brass wire, of a quarter of a line at least in diameter. ab is a brass wire as fine as possible, so as to be able to bear the weight of the apparatus. cac is a linen thread uniting the wire to the apparatus. The trough contains the liquid conductor. The conducting wire of this apparatus will attract the north pole of the needle when it is placed on the left side of the plane $ffffz$, considered in the direction fz . On the same side the south pole will be repelled. On the other side of this plane, the north pole will be repelled, and the south pole attracted. That this effect may take place, we must not place the needle above ff , nor below fz or fc . If instead of presenting a small moveable needle to the conducting wire we present near one of the extremities ff one of the poles of an energetic magnet, the attraction or repulsion indicated by the needle will put the galvanic apparatus in motion, and will turn it round the prolonged axis of ab .

If instead of the conducting wire we take a large ribbon of

copper of the same breadth as the plate of zinc, the effect differs from that which we have just mentioned only in being much feebler. On the other side we increase the effect a little by making the conductor very short. Fig. 11 represents the perpendicular section of this arrangement in the direction of the breadth of the trough. Fig. 12 exhibits the same arrangement in perspective. It is obvious that *a b d e f* represents the conducting plate, and *c z z f* the plate of zinc. In this arrangement the north pole of the needle will be attracted towards the plane of *a b c*, and the south pole will be repelled from the same plane. *e d f* will have contrary effects. Here we have an apparatus whose extremities act like the poles of the needle. But it must be acknowledged that only the faces of the two extremities, and not the intermediate parts, have this analogy.

We may likewise make a moveable galvanic apparatus of two plates, one of copper and one of zinc, twisted into a spiral, and suspended in the fluid conductor. This apparatus is more moveable; but more precautions are necessary not to be deceived when we make experiments with it.

I have not yet found a method of making a galvanic apparatus capable of directing itself towards the poles of the earth. For this object it would be necessary to possess apparatus much more moveable.

ARTICLE X.

Observations on the Ventilation of Mines.

By Joseph M'Sweeny, M.D.

(To Dr. Thomson.)

SIR,

July 21, 1280.

THE safety lamp of Sir H. Davy has enabled miners to descend into an explosive atmosphere with impunity; but when we reflect that only a wire gauze liable to accident screens so many from destruction, we must admit that every precaution should be taken to prevent the accumulation of noxious gas. I am not aware that water blowing engines* have been proposed for ventilating mines. The water escaping from crevices of a mine could be conducted by pipes to supply these engines, the air extricated could be conveyed from them up the shaft by tubes, and atmospheric air would descend to occupy its place. In this way even carbonic acid gas may be got out. It is well known that gas can be got out of a mine by a tube communicating with the under valve of a large bellows; but if by the act of getting out gas we could raise the water out of the mine, it

* Vide Ferguson's Lectures by Brewster.

would be a matter of some importance. It may be thus accomplished. The main tube coming from the valve of the bellows should divide into a number of branches, each branch near its commencement being furnished with a cock. We shall suppose that the tube coming from the bellows divides into three branches, and for the sake of perspicuity shall call what conveys off the gas, tube, and what conveys up the water, pipe. If there be an air-tight box, or cistern, placed about 30 feet above the level of the water in the mine furnished with a pipe descending from its bottom into the water below, it is evident that if we exhaust the air out of this cistern, the water will ascend up the pipe, and will fill the cistern, and may be prevented from returning by valves placed in the pipe. By this plan we get the water up one lift. Now to get it up a second, the cistern should be provided with a valve on its top, opening upwards. When this valve is pulled up, the atmospheric pressure on the surface of the water in the cistern will force it up a second similar pipe (which rises through the water from near its bottom) to a second similar cistern placed about 30 feet above the bottom of the first one, provided an exhaustion is taking place in this second cistern. By this process the water may be raised from the second cistern to a third, and from a third to a fourth, and so on to any height.

The means of exhausting the cisterns of air now remains to be explained. We suppose the large bellows to be at work, and that the cocks of all the branches of the main tube coming from the valve of the bellows are shut, except the cock of the first branch. The first branch descends down to the top of the first cistern; the exhaustion, by means of the bellows, takes place through this branch, consequently the water rises to fill the first cistern. When this cistern is full, the cock of the first branch is shut, and that of the second branch (which descends down to the top of the second cistern) is opened; of course an exhaustion takes place, and the water rises from the first to fill the second cistern, the valve on the top of the first cistern being pulled up to allow of the atmospheric pressure. When the cock of the second branch is shut, and that of the third opened, the water can be made to rise from the second cistern in like manner to fill a third one above it, and so on. Water raised from a mine, or any other water, provided there be some fall, may be employed for freeing a mine from gas by the following contrivance: Let R (Pl. CIX) fig. 13, be a pipe coming from a reservoir of water, and F a tube going down to the bottom of a mine. When the cock *y* is turned, the water flows into the long air-tight cistern A, and drives the air out, which escapes through the valve *v*, not being able to escape through the tube F, on account of the valve *n*. When the cistern A is full of water, *y* is to be shut, and the cock *g* is to be opened; the water now flows into the cistern B, and A becomes full of gas from the mine through the

tube F. When the cock *s* is opened, the water flows into the cistern C, and B becomes full of gas from A through the open cock *g*. When the cock *e* is opened, the water escapes, and in like manner gas takes its place from the cistern B. Here with a trifling fall, the water can draw off three times its bulk of carbonic acid gas from the bottom of the mine, and may be made to draw off a hundred times its bulk by increasing the number of long cisterns, if the fall would permit it. When the water escapes from the last cistern, the cocks are to be shut, and the process is to be renewed by allowing the water to flow from the reservoir into the upper cistern. A similar apparatus could be placed below in the mine, and could be supplied with water from the feeders. Tubes placed over the valves *v v v* would, in this case, conduct the gas up the shaft.

JOSEPH M'SWEENEY.

ARTICLE XI.

ANALYSES OF BOOKS.

Philosophical Transactions of the Royal Society of London, for 1820, Part I.

THIS is one of the thinnest and most meagre volumes of the Society's Transactions which has appeared for these many years. Indeed it indicates, in pretty conspicuous language, the declining health of the late President. It contains only seven papers, which are as follows :

I. *A further Investigation of the component Parts of the Blood.* By Sir Everard Home, Bart. V.P.R.S.—The author has already published two papers upon this subject, and he is of opinion that in this paper he has made out the greater number, if not the whole, of the component parts of the blood. The object of a preceding paper was to show that when blood coagulates, carbonic acid gas is disengaged from it, which, shooting into horizontal tubes, forms vessels, which are gradually filled with red blood. The only facts, or alleged facts, contained in the present paper, as far as I can perceive, are : 1. That besides the red globules of the blood, there are smaller globules destitute of a red colour, to which the author has given the name of *lymph globules*. These last constitute the coagulum of what is called the *buffy coat* of the blood. The buffy coat is owing in the author's opinion to the blood coagulating so slowly that the red globules have time to precipitate to the bottom. These lymph globules are first formed of all the parts of the blood, and make their appearance in the duodenum. 2. The carbonic acid which acts so notable a part in the formation of blood-vessels is supplied

to the blood by digestion. The reader will be curious to see the proof of this. Here it is: A pauper was bled one hour after dinner, and his blood after its coagulation was put under the exhausted receiver of an air-pump. It was enclosed in a vessel from which a glass tube proceeded, and plunged into barytes water. Carbonic acid gas escaped from the coagulum in torrents, and precipitated the barytes in the state of carbonate. This experiment was made by Mr. Brande. 3. In the clot of blood usually found in aneurismal sacks, crystals were observed which were examined by Mr. Faraday. They consisted of sulphate of lime, muriate and phosphate of soda.

The reader will perceive from the preceding enumeration that the component parts of the blood are very few, and very simple. Henceforth chemists may neglect the fibrin, albumen, and colouring matter; the blood consists of red globules and lymph globules swimming in a colourless liquid. I forgot to mention the reason assigned by this author why no blood-vessels are formed in the coagulum of aneurismal sacs. The carbonic acid gas in this case cannot form blood-vessels, because it is carried away by the circulating blood. It is rather unfortunate that the blood will not on this occasion allow the carbonic acid to perform its usual function. New blood-vessels in an aneurismal sac would be desirable things.

II. *On the Composition and Analysis of the Inflammable Gaseous Compounds resulting from the destructive Distillation of Coal and Oil, with some Remarks on their relative heating and illuminating Powers.* By William Thomas Brande, Esq. Sec. R. S. Prof. Chem. R. I.—This paper is divided into two sections. The object of the first section is to endeavour to prove that carbon and hydrogen unite only in one proportion constituting the substance usually called *olefiant gas*, and that the *carburetted hydrogen gas* of chemists is merely a mechanical mixture of olefiant gas and hydrogen gas. I read over this section with a good deal of surprise, and not a little regret and mortification. Carburetted hydrogen gas was known long before the time of Mr. Dalton's appearing as a scientific chemist. It had been an object of experiment to Dr. Higgins, Dr. Ingenhousz, and even Dr. Austin. Dr. Ingenhousz was in the habit of collecting it from stagnant pools, and he published an account of his method. Mr. Cruikshanks made many experiments on it, and subjected it and the other gaseous compounds of carbon to the experiments with chlorine, which Mr. Brande in this paper ascribes to Mr. Faraday. I myself witnessed these experiments in Mr. Cruikshank's laboratory in 1802, and unless my memory misleads me, an account of them was published in the fourth or fifth volume of Nicholson's Quarto Journal. As I have not a copy of these volumes at hand, I cannot refer to them. It is true that Mr. Dalton was the first person who ascertained that carburetted hydrogen gas requires twice its volume of oxygen for combus-

tion, and that it forms its own volume of carbonic acid gas. From this he deduced its composition, viz. two volumes or atoms of hydrogen and one of carbon. It is to Mr. Dalton then that we are indebted for all the accurate knowledge which we possess of this gas, though he was not its discoverer.

The only mode of procuring it which I am acquainted with is to collect it from stagnant waters in the way first practised by Dr. Ingenhousz. I have examined it very often from stagnant water in the neighbourhood of Edinburgh, and London, and Glasgow, and have always found it exactly the same.

Nothing is easier than to demonstrate that carburetted hydrogen gas is not a mixture of olefiant gas and hydrogen gas; but a true, definite, chemical compound. Its specific gravity, as I have shown in the last number of the *Annals of Philosophy* is 0.5555, and it is a compound of

2 volumes hydrogen gas }
 1 volume vapour of carbon } condensed into one volume.

The true specific gravity of olefiant gas, as may be seen in the paper just referred to, is 0.9722, and it is a compound of

2 volumes hydrogen gas }
 2 volumes vapour of carbon } condensed into one volume.

Suppose now we wish to make a mixture of olefiant gas and hydrogen such that it will require for complete combustion exactly twice its volume of oxygen gas, it is obvious that we have only to mix together one volume of olefiant gas and two-thirds of a volume of hydrogen gas; for such a mixture would be equivalent to

2.666 volumes of hydrogen gas
 2 volumes vapour of carbon

Oxygen gas.

| | |
|--|--------------|
| Now 2.66 volumes of hydrogen gas require | 1.33 volumes |
| 2.00 volumes carbon | 2.00 |
| | 3.33 |

But $1.66 \times 2 = 3.33$. Thus it is obvious that such a mixture would just require twice its volume of oxygen gas for complete combustion. Let us see what the specific gravity of such a mixture will be.

Let A = volume of olefiant gas, a = specific gravity of olefiant gas; let B = volume of hydrogen gas, b = specific gravity of hydrogen gas; x = specific gravity of a mixture of A + B of the two gases. It is easy to demonstrate from the common principles of pneumatics that

$$x = \frac{B b + A a}{A + B}$$

In the present case,

$$A = 1; a = 0.9722$$

$$B = 0.66; b = 0.0694$$

Consequently,

$$x = \frac{0.66 \times 0.0694 + 0.9722}{1.66} = 0.86178$$

But this specific gravity is quite different from 0.5555, the true specific gravity of carburetted hydrogen.

A mixture of 1 volume of olefiant gas and 0.66 volume of hydrogen would leave two volumes of carbonic acid gas, which exceeds 1.66, the volume of the gas before combustion, by one-sixth part. It is obvious that a mixture of equal volumes of olefiant gas and hydrogen gas would, after combustion, leave exactly its own volume of carbonic acid gas. The specific gravity of such a mixture, determined by the preceding formula, is 0.5208, approaching nearer indeed to the true specific gravity of carburetted hydrogen gas, but not the same with it. But such a mixture would not require so much as twice its volume of oxygen gas to consume it. And it may be easily demonstrated that no mixture of olefiant gas and hydrogen gas can be made which possesses at the same time all the three characteristic properties of carburetted hydrogen gas. These three are the following:

1. A specific gravity of 0.5555.
2. Requiring twice its volume of oxygen gas for complete combustion.
3. Leaving after combustion its own volume of carbonic acid gas.

We are absolutely certain then that carburetted hydrogen gas is not a mixture of olefiant gas and hydrogen gas, but a chemical compound of hydrogen and carbon.

After this demonstration, it would be needless to examine Mr. Brande's reasoning. It is founded upon loose analogies. His specific gravities and his atomic weights are, without any exception, inaccurate. I am surprised at the low specific gravity of coal gas which he assigns, viz. 0.443. I infer from it that the London gas companies have greatly increased the volume of gas obtained from a given weight of coal. Coal gas is never pure carburetted hydrogen, but a mixture of three or more gases, and is always or almost always contaminated with the vapour of naphtha to which it owes its smell, and not a little of its luminous properties. Neither can pure carburetted hydrogen gas be obtained by distilling acetate of potash, or any other acetate which I have ever tried. Such distillations always produce mixtures, and not chemical compounds.

Mr. Brande informs us that the specific gravity of the gas from whale oil is 0·769; but I find that the specific gravity of oil gas is just as various as that of coal gas. I have got it as high as 1·0958, and very often as high as 0·8846. Notwithstanding this high specific gravity, it was not olefiant gas; for the carbonic acid which it formed very little exceeded the volume of the gas, and the oxygen consumed was but little more than double the volume of the inflammable gas. My gas was procured by passing whale oil through a red-hot iron tube. Such gases are mixtures, and always contain a portion of the vapour of Dippel's oil, to which much of their inflammability is owing.

The object of the second section of this paper is to compare the illuminating and heating powers of olefiant gas, oil gas, and coal gas. He found that olefiant gas does not issue from a small orifice with the same rapidity as oil gas. Through an orifice of $\frac{1}{8}$ th inch in diameter, 640 cubic inches of olefiant gas passed in an hour, and 800 cubic inches of oil gas. This is in the proportion of 4 to 5.

To produce a light equal to 10 wax candles for an hour, the consumption was as follows :

| | |
|------------------------|--------------------|
| Olefiant gas | 2600 cubic inches. |
| Oil gas | 4875 |
| Coal gas | 13120 |

The author found, as Count Rumford had done before him, that the light given out by gases was much increased when a number of burning jets were placed near each other.

To raise a quart of water from 50° to 212, he found it necessary to burn of

| | |
|------------------------|------------------|
| Olefiant gas | 870 cubic inches |
| Oil gas | 1300 |
| Coal gas | 2190 |

The light from an Argand's lamp with olefiant gas when concentrated by a lens raised a thermometer $4\frac{1}{2}$ ° in five minutes. It is well known that a mixture of equal volumes of chlorine and hydrogen gas explodes when exposed to the direct rays of the sun. The author produced the same effect by the light from a galvanic battery; but could not accomplish the combustion or union of these gases by any other artificial light whatever.

III. *On the Elasticity of the Lungs.* By James Carson, M.D. —The subject of this paper, or at least one intimately connected with it, has occupied the attention of the author ever since he began his medical studies. In the year 1799 he received along with myself the degree of Doctor of Medicine from the University of Edinburgh, on which occasion he defended a very ingenious thesis on the circulation of the blood. The opinions contained in this thesis, which were new and important, he expanded in

1815, and published under the title of "An Inquiry into the Causes of the Motion of the Blood." This publication attracted the attention of physiologists, and the views which were given in it have been generally acknowledged to be both important and new. The object of the present paper is to show that the lungs possess a considerable degree of elasticity, and that this elasticity, together with the muscular action of the diaphragm, constitute the great agents of respiration.

In the living body, the lungs fill the chest, and are pressed against its walls by a pressure nearly equivalent to the whole weight of the atmosphere. When an opening is made into the thorax, the lungs immediately collapse in consequence of their elasticity, because now the pressure of the air is removed, as it acts both upon the inside and outside of the lungs at the same time. The author inserted a glass tube into the windpipe of a recently killed animal. This tube terminated in a glass globe capable of holding about two quarts. From its extremity, another glass tube proceeded in a vertical direction, and about three feet in length. Water was poured into this upright tube till it stood about a foot above the level of the water in the globe. The chest of the animal was then opened. The lungs collapsed, and the water rose higher in the tube. An additional quantity of water was poured into the tube till the elasticity of the lungs was overcome, and they again completely filled the thorax. The height of this column of water was observed, and considered as indicating the amount of the elasticity of the lungs. In oxen it exceeded a foot and a half; in calves, sheep, and large dogs, it varied from a foot to a foot and a half; and in rabbits and cats from six to ten inches.

IV. *On the Action of Crystallized Bodies on Homogeneous Light, and on the Causes of the Deviation from Newton's Scale in the Tints which many of them develope on Exposure to a Polarized Ray.* By J. F. W. Herschel, Esq. F.R.S. Lond. and Edin.—This paper appears to me of too much importance to be abridged. It will, therefore, be given entire in a future number of the *Annals of Philosophy*.

V. *A Case of the Human Fetus found in the Ovarium of the Size it usually acquires at the End of the fourth Month.* By A. B. Granville, M.D. F.R.S.—The lady in whom this remarkable conception occurred had been the mother of seven children, and was in the 39th year of her age. The fœtus was found in the left ovary. From the drawing it appears to have been perfect. Both the fallopian tubes were impervious, except about half way. The uterus was enlarged, and nearly of the same size as when it contains a fœtus of four months. The fœtus was surrounded by a chorion and amnios, and attached to a placenta, as in other cases. Portions of the corpus luteum surrounded the fœtus. The death of the mother seems to have been occasioned by hæmorrhage. The increased size of the fœtus burst the

ovarium, and tore the adhesions of the placenta: this produced the sudden hæmorrhage which proved fatal.

This case is undoubtedly curious. It is difficult to conceive how impregnation was produced. Doubtless the changes in the falopian tubes must have taken place at a subsequent period.

VI. *On some Combinations of Platinum.* By Edmund Davy, Esq. Prof. of Chemistry, and Secretary to the Cork Institution. —Sulphate of platinum is soluble in alcohol and ether. If an aqueous solution of this sulphate be mixed with alcohol, and set aside for some time, a black powder precipitates, and the liquid becomes colourless. The same black matter is obtained in a few minutes by boiling together sulphate of platinum and alcohol.

This powder is black, in small lumps, which are soft, and easily reduced to an impalpable powder. It soils the fingers or paper, is destitute of lustre, tasteless, and insoluble in water both cold and hot. It is not altered by exposure to the air. When heated, a feeble explosion is produced, with a flash of red light, and the platinum is reduced. It is insoluble in nitric, sulphuric, and phosphoric acids; but it dissolves slowly in muriatic acid. It is scarcely affected by chlorine. When put into liquid ammonia it gradually acquires fulminating properties. When placed in contact with ammoniacal gas, it becomes red-hot and scintillates. It is immediately decomposed by the agency of alcohol. If a bit of paper, or cork, be moistened with alcohol, and a little of the powder placed on it, a hissing noise is heard, and the platinum is reduced and ignited, and continues in a state of ignition till the whole of the alcohol is consumed. When the powder is boiled in alcohol, it is partially decomposed, and becomes lighter. If it be then thrown upon a filter, the odour of acetic acid is perceived, and in a few hours the platinum is reduced, and the paper charred. When mixed with flowers of sulphur, and heated, a blue coloured sulphuret of platinum is formed. This powder, according to the experiments of Mr. Davy, is composed of

| | |
|---------------------------------------|--------|
| Platinum | 96·25 |
| Nitric acid, oxygen, and carbon. | 3·75 |
| | 100·00 |

It is not easy to form a conception of such a compound. It must of necessity be at least in part a mechanical mixture.

Mr. Davy finds that sulphate of platinum is capable of precipitating gelatine from its aqueous solution. He recommends it as a test for that substance, and thinks it better and more delicate and certain than the infusion of nutgalls.

Mr. Davy described in a former paper a mode of preparing *grey oxide of platinum*. The method is to add strong nitric acid

to fulminating platinum, to boil the mixture to dryness, and expose the residue to a heat just under redness to expel the nitric acid.

The colour of this acid is dark iron grey. It has the metallic lustre, is hard enough to cut and polish brass; but does not touch steel. It is not acted on by water, nitric, sulphuric, or phosphoric acid. It is insoluble in nitromuriatic acid, and in cold muriatic acid; but this last acid dissolves it by the assistance of heat. It is insoluble in the fixed alkalies, and not altered by ammonia. When heated with sulphur, sulphurous acid gas is emitted, and sulphuret of platinum formed. When mixed with zinc filings, and heated, the oxide is decomposed with a vivid ignition, and white oxide of zinc formed. According to Mr. Davy, seven grs. of this oxide, when heated, are decomposed into six grs. of platinum, and 2.1 cubic inches of oxygen gas; while some moisture and nitric acid were disengaged. According to this result, it is a compound of

| | |
|----------------|---------|
| Platinum | 100.000 |
| Oxygen | 11.861 |

Mr. Davy considers this oxide as the protoxide, and thinks that the black oxide contains $1\frac{1}{2}$ times as much oxygen. He informs us that he repeated some of Mr. Cooper's experiments on this oxide, and found them inaccurate.

VII. *On the Methods of cutting Rock Crystal for Micrometers.* By William Hyde Wollaston, M.D. F.R.S.—This paper being of considerable practicable utility, and very short, I conceive that it will be better to print it entire than to attempt an abridgment which might leave out something of importance to the artist. It is as follows:

For the mere purpose of examining the phenomena of double refraction, it is extremely easy for any skilful workman to combine a wedge of rock crystal, or any other doubly refracting substance, with another wedge of crown glass opposed to it, in such a manner that a luminous object seen through them shall appear in its true place by ordinary refraction, accompanied by a second image at a small distance, produced by the extraordinary refraction of the crystal.

In consequence of the dispersion of colours which occurs in employing different substances, the above combination is not suited for the purpose of the micrometer invented by the Abbé Rochon; but it is not difficult to obtain such a section of rock crystal as may be substituted for the wedge of glass, so that the pencil of light shall be restored to its original direction void of colour without diminishing the separation of the images occasioned by the first wedge.

But since the degree to which the double refraction of rock crystal separates the two portions of a beam of light transmitted through it, is not so great as may frequently be wished, it becomes desirable to increase this effect beyond what can be produced by the most obvious method of employing that substance; and it does appear from M. Rochon's own account of his contrivance,* that he fully succeeded in accomplishing this end. But although he informs us that the means employed, as best suited to his views, had exactly the effect of doubling the amount of devia-

* Journal de Physique, An. 9.

tion produced by ordinary means, he has not chosen to explain the mode of construction he adopted, and has merely referred to a certain artist living at that time in Paris, who was in possession of his secret, and skilful in applying it to the construction of micrometers.

As I have reason to think that the method to which he alludes in his memoir has never yet been described, I design, in the present communication, to explain a combination which I have found advantageous, and which I think must be the same as that of M. Rochon.

I shall hope to render the principles of this construction intelligible to every one acquainted with the original observation of Huygens on the properties of polarised light, and to enable any competent artist to cut wedges from hexagonal prisms of rock crystal, in the positions requisite to produce, by their combination, the double effect to which I allude.

There are three principal directions in which a crystal may be cut specifically different from each other, which require to be distinctly understood.

In the first place, let us suppose a prismatic crystal to be placed with its axis in a vertical position, and a portion to be cut off from the base by a plane surface at right angles to the axis, and sufficient to form a wedge of 20 degrees, by giving it a second surface duly inclined to the former. For distinction, this may be called the *horizontal wedge*.

Next, let the crystal be bisected vertically by a plane passing through two opposite edges of the prism, in order to make two other wedges which are to be cut in different directions from the two portions, and to have each the same angle of 20 degrees.

Let one of the halves thus obtained be slit in a plane which meets the surface of bisection in one of the edges of the original prism, and consequently in a line parallel to the axis. The wedge thus formed may be called a *lateral wedge*.

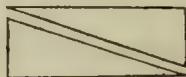
Let the remaining half be cut by another plane not vertical, but inclined to the vertical plane at an angle of 20°, and meeting it in a line parallel to the base, or at right angles to the axis. This may be called a *vertical wedge*.

We have thus three wedges cut in different directions at right angles to each other, and, accordingly, having their axes of crystallization differently placed in each.

In the first, or horizontal wedge, the axis is at right angles to the first surface. In the second, or lateral wedge, the axis is parallel in the first surface, and parallel to its acute edge. In the third, or vertical wedge, the axis is also in the first surface, but it is at right angles to the acute edge.

An object seen through the first wedge in the direction of the axis does not appear double; but since rays transmitted through the second or third pass at right angles to the axis, both of these wedges give two images of any object seen through them.

There are obviously three modes in which these wedges may be combined in pairs, by placing two of them together with their acute edges in opposite directions. The first pair may be represented by L H; the second by V H; the third by V L. In the two first cases, the separation of the images will be the same, since the angles of all the wedges are supposed to be made equal, the compound medium will be comprised under parallel surfaces, so that a ray ordinarily refracted by both, emerges in its original direction; but since the extraordinary ray is made to deviate about 17' from the ordinary course by the wedge which refracts doubly, this difference is not corrected by the horizontal wedge, so that an object seen through either of the combinations L H or V H, appears doubled to the amount of 17'.



The third combination, consisting of the vertical and lateral wedges combined, as in the former cases, with their acute edges in opposite directions, produces an effect perfectly distinct from either of the former combinations; for by reason of the transverse position of their axes of crystallization, the separation of the two images becomes exactly doubled. The consequence of that position is, that the pencil ordinarily refracted by the first wedge is refracted extraordinarily by the second, and that which has been refracted extraordinarily by the first suffers a similar interchange, and is now ordinarily refracted, so that neither of the divided pencils returns to its true place; and since one falls as much short of the mean as the other exceeds the truth, they emerge ultimately separated twice the usual difference between the ordinary and extraordinary refractions, and thus present two

images separated 34', just double of that which is effected by either of the preceding combinations.

Though it could scarcely be doubted that this is essentially the construction which was employed by M. Rochon, there is an additional circumstance concerning the effect of such a pair of wedges when otherwise combined, which fully establishes the identity of the method here proposed with his. If the two wedges be placed with their edges together, so as to form by their union a wedge of 40° , the consequence is, that though a pencil of light is in fact divided into two parts by the first wedge, both parts in the end emerge together; the refraction of one being $o + e$, and of the other $e + o$; they both deviate from their original direction by exactly the same quantity, and present only a single image of the luminous object; but it is coloured, as usual, in proportion to the amount of deviation occasioned by the sum of the wedges. This, without doubt, is the first of two opposite directions mentioned by M. Rochon, in which he says the double refraction was not perceptible.

“Pour cet effet,” says M. Rochon, “j’employai deux prismes égaux taillés dans le sens le plus favorable a mes vues, et en les présentant dans les deux sens opposés je trouvai, que dans la première disposition la double réfraction n’étoit pas perceptible, mais, en faisant prendre à mes prismes un sens inverse, la double réfraction de chaque prisme étoit presque doublée.”

The correspondence in the effect which I have described renders this passage from M. Rochon perfectly intelligible; and I hope the directions above given will be sufficient to enable any one to cut a crystal to the greatest advantage for making this sort of micrometer. But it must be observed that in attempting such a construction, great nicety is requisite not only in cutting the wedges so that the refraction in each shall take place at right angles to the axis, but also in cementing them together, so that the axes of the two wedges shall be at right angles to each other. And it may farther be remarked, that even then, unless the pencil of light pass truly in the common plane of refraction of the wedges, four images will be formed, so as to destroy the effect of the combination.

The part terminates as usual with a meteorological journal of the weather at London during the year 1819.

The mean height of the thermometer was 52.7° ; of the barometer (not corrected for temperature) 29.81 . The quantity of rain was 13.727 inches. The mean of Leslie’s hygrometer was 16.8° .

ARTICLE XII.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS
CONNECTED WITH SCIENCE.

I. Remarkable Petrification.

THE city of Glasgow is built upon the coal formation which extends round it in every direction. The sandstone beds belonging to this formation are in many places, just at the surface of the earth, of great thickness, and almost every where constitute excellent building stones. The quarries of this sandstone, out of which the city has been built, are situated chiefly on the north-west side of the town below port Dundas. These quarries are very extensive. The uppermost beds of sandstone are usually of a reddish hue, and some of them abound in iron

galls, which soon oxidize and decay, when the stone is exposed to the weather. The deeper beds are light coloured, free from iron galls, and constitute a very beautiful and very durable building stone. They are composed chiefly of grains of quartz, varying in size from that of very fine sand to that of a pigeon's egg. These large grains are all rounded, and quite smooth on the surface, so that no reasonable doubt can be entertained that they have been all water worn. Casts of unknown species of trees are very common in these quarries, as indeed is usually the case in the sandstone belonging to the coal formation.

About a month ago, in a quarry belonging to Mr. Lee, and situated on the north side of Sauchyhall-street, and about a quarter of a mile west from the road leading from Buchanan-street to Port Dundas, the quarry men came upon the cast of a tree in situ just as it had been growing. The trunk is about 26 inches in diameter, not quite round, but somewhat oval, in consequence of the south side of the tree having grown more rapidly than the other three sides; so that the north and south diameter is several inches longer than the east and west diameter. The body of the tree itself is composed of sandstone, precisely similar to the rest of the quarry. But the bark has been converted into perfect cherry coal, which adheres firmly to the tree, and renders it easy to remove the rock with which the tree is incrustated. About three feet of the bottom part of the tree has been uncovered. This portion is situated about 40 feet below the surface of the earth in a solid quarry of sandstone. The upper part of the trunk and branches has not been discovered. Indeed it is some time since the upper portion of the quarry was removed. The roots may be seen dipping down into the earth precisely as the roots of living trees do. Four very large roots may be seen issuing from the trunks, and extending some of them about a foot before they are lost in the surrounding stone. Indeed I have no doubt that were the requisite pains taken, the whole roots might be exposed to a considerable depth.

There is nothing to indicate the species of tree of which the mould has been here preserved. From the appearance of the roots, it is obvious that it was not a fir. It had more resemblance to a beech. The bark has been so completely bituminized that its usual characters are effaced. The petrification, however, is not without its value. It demonstrates that the sandstone has been formed at a period posterior to the existence of large trees, and that the water-worn appearance of the quartz pebbles of which the sandstone is composed is not a deceitful indication, as some geologists would persuade us that it is, but quite correct. But if the sandstone, which constitutes so great a proportion of the coal beds, be a formation posterior to the earth being covered with wood, we can entertain no doubt that this is the

case also with the slate clay and the coal which alternate with this sandstone. Indeed if the coal formation exists as a portion of the old red sandstone, we can entertain no reasonable doubt that the old red sandstone itself has been formed after the earth was covered with wood; so that the fancies (for I can give them no other name) lately advanced respecting its being an original deposit cannot be supported with any regard to the rules of correct reasoning. If it turn out to be true, as there is some reason for believing, that the transition and some of the primitive rocks alternate with the old red sandstone, we must conclude that these rocks also have been formed after the earth had been covered with wood.

II. Remarkable Instance of Spontaneous Combustion.

It is the custom with many of the merchants and manufacturers of Glasgow to spend several of the summer months at sea-bathing quarters, leaving the care of their town houses to a single servant, or sometimes shutting them up altogether. A gentleman, a neighbour of mine, removed with his family to Largs in May last, carried with him all his servants, and shut up his house. It was opened for the first time about the end of August. The house stands on the side of a pretty steep declivity, so that the kitchen, which is in the back part of the house, though sunk considerably below the level of the street, is entirely above ground. It is remarkably well lighted and ventilated. I was in it on the day that the house was opened without perceiving any unusual appearance of dampness. In an opening of the wall near the kitchen fire, originally intended, I believe, for an oven, there was placed a wooden barrel, bound with iron hoops, and filled with oatmeal. This meal had heated of itself during the absence of the family, had at last caught fire, and was totally consumed, together with the barrel which contained it, nothing remaining but the iron hoops, and a few pieces of charcoal. I presume that the meal had been somewhat moist, and that it had heated precisely in the same way as hay does when stacked moist. The great avidity which oatmeal has for moisture, and the heat generated by its absorption of it, must be familiar to every one who has been in the habit of seeing oatmeal. Indeed Mr. Leslie found that its avidity for moisture was so great that it could be substituted for sulphuric acid in his well-known method of freezing water by confining it over sulphuric acid under the exhausted receiver of an air-pump.

III. Urea.

The well-known substance in urine to which the name of *urea* was given by Fourcroy, seems to have been originally noticed by Rouelle. Fourcroy, in deference chiefly to the opinion of Dr. Pearson, gave the name of *uric acid* to that peculiar substance,

which was first detected by Scheele in urine, and which he found to constitute the essential constituent of those urinary calculi which he examined. M. Guyton de Morveau had previously distinguished this substance by the name of *lithic acid*. In consequence of this change there are two very distinct substances in urine distinguished by nearly the same name; viz.

Urea,
Uric acid.

The consequence which one would at first be disposed to draw from this is, that *urea* constitutes the base of *uric acid*, and that the two substances are intimately related in their properties and composition. This is a conclusion which I have little doubt has been drawn by young chemists. On that account it is of importance to prevent it as much as possible. Dr. Marcet, in his Treatise on Urinary Calculi, has alluded to this defect in the present nomenclature, and has endeavoured to correct it by resuming the old name *lithic acid* instead of the newer term *uric acid*, which has now got into general use.

This undoubtedly gets rid of the ambiguity of names; but I do not think that it entirely cures the defect of nomenclature. When the chemical nomenclature was contrived by Lavoisier, Berthollet, Morveau, and Fourcroy, in 1787, they laid it down as a law that all vegetable and animal principles were to be denoted (as far as possible) by names ending in *in*; as, for example, gelatin, fibrin, asparagin, ulmin. I have sometimes thought that this law might be followed, and yet vegetable substances distinguished from animal by making the former terminate in *in*, and the latter in *ine*; but I have not uniformly adhered to this distinction. It is curious that Fourcroy, one of the contrivers of the new nomenclature, should have deviated so much from laws of his own making as to give the term *urée*, *urea*, to an animal principle which exists in urine. Perhaps this deviation may enable us to form a pretty shrewd guess at those chemists who had the chief share in the actual formation of the nomenclature; or perhaps Fourcroy found it difficult to contrive a term from urine which should agree with the laws of the nomenclature; for *urin* or *urine* would neither have suited the French nor the English language. Be that as it may, I think that the manifest impropriety of the term *urea*, and its inconsistency with the laws of the chemical nomenclature, ought to be considered as a sufficient reason for discarding it altogether, and introducing a new one. I am induced in consequence to propose the term *nephrin*, as a very suitable appellation for the substance which has been hitherto distinguished by the name of *urea*. This name, as the reader will perceive, is derived from the *kidneys*, the organs which secrete the peculiar substance in question. The two bodies hitherto called *urea*, and *uric acid*, may be henceforth

called *nephrin*, and uric acid. The term *nephrin* is systematic, and it has the advantage over *urea* of neither being ambiguous nor offensive.

IV. *Laccin*.

I have given this name to a peculiar substance described some years ago by Dr. John, as one of the constituents of *stick lac*, though I am not aware that any account of it has hitherto appeared in the English language. It is obtained by digesting stick lac repeatedly in alcohol and water till nothing further can be removed. The residual matter is *laccin*. It possesses the following properties :

It is hard and brittle, has a yellow colour, and a certain degree of transparency. It is insoluble in cold water, but in hot water, though it does not dissolve, it becomes soft. In cold alcohol, it softens, increases in bulk, and acquires a slippery feel. Even hot alcohol is unable to dissolve it. In ether and essential oils it swells a little, and becomes quite transparent, but does not dissolve. It dissolves very readily in potash ley, and the solution has a light-brown colour. Muriatic acid renders the solution milky, and the *laccin* slowly precipitates. Concentrated sulphuric acid dissolves it very rapidly; the solution has an amethyst colour, and becomes muddy when mixed with water. When long boiled with water containing from $\frac{1}{10}$ th to $\frac{1}{8}$ th of its weight of sulphuric acid, only a small portion of it is dissolved. This portion is obtained in the state of a yellowish gum, when the acid is separated by means of lime, and the solution evaporated. This property distinguishes *laccin* readily from *cerasin*. Concentrated nitric acid dissolves it slowly when assisted by heat. The solution is clear, and has a yellow colour without any bitter taste. It gradually deposits some crystals of oxalic acid. Diluted nitric acid has no effect on it either cold or hot.

Laccin, when heated, gives out an aromatic odour, and becomes soft. It does not melt, but is gradually charred. When distilled, it gives out water, an acid which, when saturated with soda, throws down muriate of iron white, and a yellow and brown oil. No ammonia can be detected in the products of the distillation.—(John's *Chemische Untersuchungen*, iv. 12.)

V. *Dry Rot*.

Col. Gibbs, of the United States, is of opinion that the reason why the dry rot is so much more frequent now-a-days than it was formerly, is, that in consequence of the great consumption of wood during the last century for naval and architectural purposes, all the old wood has been consumed, and nothing is now left for these purposes but comparatively young wood, in which the *albuminum* bears a much greater proportion to the heart than

in old trees. He mentions some facts that have been stated to him by Col. Perkins, of Boston, and which seem entitled to attention. Several ships built at Boston have been salted, or filled in between the timbers with salt, while on the stocks, and after the lapse of 10 or 15 years the timbers have in every case been found to be perfectly sound. A large ship belonging to Col. Perkins which had been salted (14 years old) required repairs, new decks, and new iron work. Considering the age of the ship, it was important to examine the frame in every part. The ceiling was, therefore, ripped up, and a complete examination took place. The result was, that the timber and plank were found completely sound in every part. A vessel of 500 tons required 500 bushels of salt, and two years after being built, 100 bushels were added to fill up the space of the salt dissolved.—American Journal of Science and the Arts, ii. 114.)

It would be of importance to know whether the iron works in such ships did not sooner decay than in ships not salted. It would be desirable likewise to ascertain whether salted ships have any bad effects upon the health of the crews.

VI. *Cantharadin.*

It appears from a set of experiments made by Dr. J. Freeman Dana, of which he gives an account in Silliman's American Journal of Science and the Arts, vol. ii. p. 137, that the *lytta vittata*, or common potatoe fly of North America, contains a quantity of cantharadin as well as the *Meloe vesicatoria*. The *lytta* of America, Dr. Dana informs us, possesses vesicating powers in a higher degree than the Spanish fly. The experiments were made on rather a small scale in consequence of the difficulty of procuring the fly in sufficient quantity; but a sensible portion of cantharidin seems to have been obtained.

VII. *Fossil Bones in Old Red Sandstone.*

In North America, the old red sandstone formation, as we are informed by Prof. Silliman, extends from the sea shore at New Haven to the state of Vermont, and intersects the states of Connecticut and Massachusetts. It is more than 110 miles long, and varies in breadth from three miles to twenty-five. On both sides it is bounded by primitive rocks: the northern boundary consists of mica slate and clay slate. Mr. Solomon Ellsworth, Jun. of East-Windsor, in Connecticut, while blasting this rock for a well 23 feet below the surface of the earth met with a number of fossil bones completely enclosed in the sandstone. Unfortunately before Mr. Ellsworth came to the knowledge of what was going on, the skeleton had been blown to pieces with the rock which contained it, and several pieces of bones had been picked up and lost. The specimens seen by Prof. Smith, of Yale College, were still enclosed in the rock.

From their appearance, it is possible that they may be human bones, but they are not sufficiently characteristic to enable him to determine that point. They were examined likewise by Professors Ives and Knight, of the Medical Institution of Yale College. They all admitted that they might be human bones; but they did not consider the specimens as sufficiently distinct to form the basis of a certain conclusion. This is understood also to be the opinion of Prof. Mitchell, of New York.—(American Journal of Science and the Arts, ii. 146.)

VIII. *Explanation of the Word Calomel.*

(To Dr. Thomson.)

SIR,

Glasgow, Oct. 6, 1820.

In your number of the *Annals of Philosophy* for the present month, you propose some queries relative to the origin of the word *calomela*, or *calomel*. Is this word not evidently the old Arabic name of the substance, and composed of the words ملنظي (multazi) burning, and كلاج (kalâj) rust? So that Spielman, in the year 1766, seems to have adopted the Arabic name for the substance instead of the Latin name, which in time has become Frenchified, and modernized into calomel.

Yours,

ABUBEKER-AL-RHASI.

ARTICLE XIII.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

The sixth edition of Dr. Thomson's System of Chemistry will be ready in a few days.

Mr. Kerrigan, of the Royal Navy, is about to publish *The Young Navigator's Guide to the Sidereal and Planetary Parts of Nautical Astronomy*; being the practice of finding the latitude, the longitude, and the variation of the compass by the fixed stars and planets.

Mr. William Fell Harris has in the press, *Remarks made during a Tour through the United States of America, in the Years 1817, 1818, 1819.*

P. E. Laurent, Esq. is about to publish *Recollections of a Classical Tour made during the Years 1818, 1819, in different Parts of Turkey, Greece, and Italy, in one volume, 4to. with plates of the costumes of each country.*

Shortly will appear in one volume, 8vo. a *History of the various Species of the Palsy, with the Method of Cure, being the first part of the second volume of Dr. Cooke's Treatise on Nervous Diseases.*

The Travels of Cosmo III. Grand Duke of Tuscany, through a large Part of England, in 1669, translated from the Italian MS. with above 40 Engravings, are preparing for publication.

G. L. Chesterton, Esq. late Captain and Judge Advocate of the British Le-

gion, will soon publish a Narrative of his Voyage to South America, with Observations on that Country.

A new Edition with considerable Additions will shortly appear of Dr. Ayre's work, on the Disorders of the Liver, and other Organs of Digestion.

The third volume of the Transactions of the Association of the Fellows and Licentiates of the King's and Queen's College of Physicians in Ireland is in the press.

JUST PUBLISHED.

An Analytical Calculation of the Solar Eclipse for Sept. 7, 1820. By D. M'Gregor. 8vo. 3s.

Annals of Glasgow, comprising an Account of the Public Buildings, Charities, &c. By James Cleland. 2 vols. 8vo. 1l. 1s. boards.

The same work abridged. 8vo. 10s. 6d. boards.

The Rise and Progress of the Public Institutions of Glasgow. By the same Author. 8vo. 7s. 6d.

Illustrations of the capital Operations of Surgery, Trephine, Hernia, Amputation, Aneurism, and Lithotomy. By Charles Bell. Imperial 4to. Part I. To be completed in five parts, with plates, plain or coloured.

A Chemical and Medical Report of the Properties of the Mineral Waters of Buxton, Matlock, Tunbridge Wells, Harrogate, Bath, Cheltenham, Leamington, Malvern, and the Isle of Wight. By Chas. Scudamore, M.D. Member of the Royal College of Physicians, &c. &c. 8vo. 9s.

Medico Chirurgical Transactions, published by the Medical and Chirurgical Society of London. Vol. XI. Part I. 8vo. 9s.

A Treatise on the Plague, designed to prove it contagious, from Facts collected during the Author's Residence in Malta when visited by that Malady in 1813, &c. By Sir Arthur Brooke Faulkner, M.D. Fellow of the Royal College of Physicians. 8vo. 12s.

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ARTICLE XIV.

Magnetical, Meteorological, and Astronomical Observations.

By Col. Beaufoy, F.R.S.

*Bushey Heath, near Stanmore.*Latitude $51^{\circ} 37' 44.21''$ North. Longitude West in time $1^{\circ} 20.93''$.*Magnetical Observations, 1820. — Variation West.*

| Month. | Morning Observ. | | | Noon Observ. | | | Evening Observ. | | |
|---------------------|-----------------|------------|------|--------------|------------|------|-----------------|------------|------|
| | Hour. | Variation. | | Hour. | Variation. | | Hour. | Variation. | |
| Sept. 1 | 8h 35' | 24° 32' | 57'' | 1h 40' | 24° 40' | 48'' | 6h 55' | 24° 33' | 46'' |
| 2 | 8 40 | 24 33 | 06 | 1 15 | 24 41 | 23 | 6 55 | 24 32 | 53 |
| 3 | 8 40 | 24 34 | 20 | 1 30 | 24 40 | 03 | 6 50 | 24 30 | 13 |
| 4 | 8 45 | 24 33 | 01 | 1 25 | 24 47 | 01 | 6 40 | 24 34 | 04 |
| 5 | 8 45 | 24 31 | 37 | 1 15 | 24 42 | 02 | 6 40 | 24 36 | 59 |
| 6 | 8 40 | 24 32 | 38 | 1 15 | 24 40 | 27 | 6 10 | 24 34 | 58 |
| 7 | 8 35 | 24 29 | 47 | 1 25 | 24 38 | 53 | 6 30 | 24 33 | 08 |
| 8 | 8 35 | 24 29 | 50 | 1 20 | 24 37 | 47 | 6 25 | 24 34 | 05 |
| 9 | 8 40 | 24 30 | 20 | 1 20 | 24 41 | 58 | 6 30 | 24 33 | 25 |
| 10 | 8 45 | 24 32 | 24 | 1 45 | 24 41 | 13 | 6 25 | 24 32 | 29 |
| 11 | 8 35 | 24 30 | 37 | 1 25 | 24 41 | 24 | 6 30 | 24 32 | 57 |
| 12 | 8 40 | 24 33 | 30 | 1 20 | 24 41 | 20 | 6 25 | 24 32 | 07 |
| 13 | 8 35 | 24 29 | 21 | 1 25 | 24 40 | 54 | 6 20 | 24 30 | 30 |
| 14 | 8 35 | 24 30 | 44 | 1 30 | 24 39 | 58 | — | — | — |
| 15 | 8 40 | 24 30 | 30 | 1 25 | 24 39 | 59 | — | — | — |
| 16 | 8 35 | 24 29 | 34 | 1 30 | 24 40 | 33 | 6 15 | 24 33 | 42 |
| 17 | 8 35 | 24 30 | 42 | 1 25 | 24 39 | 30 | 6 15 | 24 32 | 30 |
| 18 | — | — | — | 1 10 | 24 40 | 41 | 6 10 | 24 32 | 21 |
| 19 | 8 35 | 24 30 | 22 | 1 15 | 24 40 | 05 | 6 15 | 24 32 | 13 |
| 20 | 8 40 | 24 31 | 00 | 1 15 | 24 39 | 52 | — | — | — |
| 21 | 8 40 | 24 30 | 32 | 1 15 | 24 42 | 01 | 6 05 | 24 32 | 41 |
| 22 | 8 45 | 24 32 | 22 | 1 30 | 24 41 | 05 | — | — | — |
| 23 | 8 35 | 24 31 | 23 | 1 20 | 24 41 | 32 | 6 05 | 24 32 | 22 |
| 24 | 8 40 | 24 30 | 01 | 1 20 | 24 38 | 33 | 6 05 | 24 33 | 01 |
| 25 | 8 40 | 24 30 | 36 | 1 15 | 24 40 | 50 | — | — | — |
| 26 | 8 35 | 24 31 | 55 | 1 25 | 24 40 | 17 | 6 00 | 24 32 | 12 |
| 27 | 8 35 | 24 30 | 47 | 1 25 | 24 39 | 31 | 6 00 | 24 33 | 02 |
| 28 | 8 35 | 24 30 | 46 | 1 25 | 24 39 | 14 | 6 00 | 24 33 | 06 |
| 29 | 8 35 | 24 30 | 46 | 1 20 | 24 40 | 38 | 6 00 | 24 32 | 44 |
| 30 | 8 35 | 24 46 | 04 | 1 20 | 24 41 | 32 | — | — | — |
| Mean for the Month. | 8 38 | 24 31 | 16 | 1 23 | 24 40 | 29 | 6 21 | 24 32 | 59 |

The noon observation on the 4th, and the morning observation of the 30th, are not included in taking the mean monthly observation, they being so much in excess for which there was no apparent cause.

Meteorological Observations.

| Month. | Time. | Barom. | Ther. | Hyg. | Wind. | Velocity. | Weather. | Six's. |
|--------|----------|---------|-------|------|---------|-----------|-----------|--------|
| Sept. | | Inches. | | | | Feet. | | |
| 1 | Morn.... | 29.621 | 54° | 64° | NE by E | | Very fine | 45½ |
| | Noon.... | 29.620 | 60 | 51 | NE | | Very fine | 63 |
| | Even.... | 29.620 | 56 | 56 | NE | | Very fine | 48½ |
| 2 | Morn.... | 29.589 | 55 | 70 | NNE | | Fine | |
| | Noon.... | 29.590 | 61 | 56 | NE | | Fine | 62 |
| 3 | Even.... | 29.593 | 55 | 58 | NNE | | Fine | 49 |
| | Morn.... | 29.669 | 54 | 73 | N | | Gloomy | |
| 4 | Noon.... | 29.692 | 62 | 59 | NNW | | Fine | 65 |
| | Even.... | 29.692 | 57 | 63 | NNW | | Very fine | 49 |
| 5 | Morn.... | 29.703 | 56 | 65 | N | | Clear | |
| | Noon.... | 29.711 | 63 | 55 | NE | | Cloudy | |
| 6 | Even.... | 29.689 | 57 | 60 | E | | Fine | 50 |
| | Morn.... | 29.661 | 57 | 55 | ESE | | Very fine | |
| 7 | Noon.... | 29.649 | 64 | 52 | E by N | | Fine | 65½ |
| | Even.... | 29.613 | 57 | 55 | E by S | | Fine | 47 |
| 8 | Morn.... | 29.582 | 56 | 72 | ENE | | Fine | |
| | Noon.... | 29.600 | 64 | 57 | E by S | | Very fine | |
| 9 | Even.... | 29.600 | 58 | 59 | E | | Clear | 51 |
| | Morn.... | 29.642 | 57 | 67 | S by E | | Fine | |
| 10 | Noon.... | 29.658 | 63 | 54 | SSW | | Fine | 65 |
| | Even.... | 29.669 | 60 | 58 | S by W | | Fine | 53½ |
| 11 | Morn.... | 29.767 | 58 | 73 | NW | | Fine | |
| | Noon.... | 29.803 | 66 | 50 | NNW | | Fine | |
| 12 | Even.... | 29.875 | 62 | 55 | NNW | | Fine | 53 |
| | Morn.... | 29.921 | 58 | 67 | Var. | | Very fine | |
| 13 | Noon.... | 29.915 | 68 | 48 | SSW | | Very fine | 69½ |
| | Even.... | 29.915 | 63 | 53 | SSW | | Very fine | 50 |
| 14 | Morn.... | 29.845 | 55 | 75 | WSW | | Cloudy | |
| | Noon.... | 29.843 | 69 | 52 | W | | Fine | |
| 15 | Even.... | 29.833 | 65 | 57 | W by S | | Very fine | 55 |
| | Morn.... | 29.819 | 62 | 68 | SE | | Clear | |
| 16 | Noon.... | 29.819 | 72 | 53 | E by S | | Hazy | 72½ |
| | Even.... | 29.810 | 65 | 59 | E | | Hazy | 57 |
| 17 | Morn.... | 29.779 | 61 | 89 | ESE | | Foggy | |
| | Noon.... | 29.783 | 71 | 55 | S by E | | Fine | |
| 18 | Even.... | 29.770 | 64 | 65 | E by S | | Very fine | 52 |
| | Morn.... | 29.735 | 59 | 71 | ESE | | Very fine | |
| 19 | Noon.... | 29.713 | 69 | 53 | Var. | | Clear | 69½ |
| | Even.... | 29.680 | 54 | 62 | E by S | | Clear | 54 |
| 20 | Morn.... | 29.504 | 60 | 71 | S by W | | Very fine | |
| | Noon.... | 29.445 | 72 | 52 | SSW | | Fine | |
| 21 | Even.... | — | — | — | — | | — | 56 |
| | Morn.... | 29.248 | 61 | 68 | SW by W | | Cloudy | |
| 22 | Noon.... | 29.245 | 59 | 67 | W by S | | Showery | 64 |
| | Even.... | — | — | — | — | | — | 48 |
| 23 | Morn.... | 29.506 | 55 | 64 | WSW | | Fine | |
| | Noon.... | 29.525 | 62 | 51 | W | | Cloudy | |
| 24 | Even.... | 29.525 | 59 | 54 | W by N | | Cloudy | 51½ |
| | Morn.... | 29.520 | 56 | 66 | WSW | | Cloudy | |
| 25 | Noon.... | 29.483 | 61 | 55 | W by S | | Cloudy | 63 |
| | Even.... | 29.432 | 56 | 69 | W | | Showery | 49 |
| 26 | Morn.... | 29.097 | — | 95 | NE | | Rain | |
| | Noon.... | 29.120 | 54 | 76 | NNW | | Cloudy | |
| 27 | Even.... | 29.200 | 50 | 68 | W by N | | Fine | |

| Month. | Time. | Barom. | Ther. | Hyg. | Wind. | Velocity. | Weather. | Six's. | |
|----------|---------------|---------|-------|------|---------|-----------|-----------|--------|-------|
| | | Inches. | | | | Feet. | | | |
| Sept. | Morn.... | 29.397 | 45° | 69° | NW by W | | Fine | 40° | |
| | 19 { Noon.... | 29.462 | 53 | 57 | N by W | | Fine | 54 | |
| | Even.... | 29.535 | 48 | 59 | W by N | | Fine | } 37 | |
| Morn.... | 29.477 | 45 | 65 | SSW | | Fine | } 54½ | | |
| 20 { | Noon.... | 29.337 | 53 | 58 | SW | | | Rain | } 41 |
| | Even.... | 29.148 | — | — | SSW | | Rain | | |
| 21 { | Morn.... | 29.989 | 47 | 75 | W | | Fine | } 57 | |
| | Noon.... | 28.962 | 56 | 55 | W | | Showery | | |
| | Even.... | 28.967 | 46 | 69 | SW | | Showery | | } 41 |
| 22 { | Morn.... | 29.240 | 48 | 70 | NW | | Very fine | } 57½ | |
| | Noon.... | 29.340 | 57 | 59 | NW | | Very fine | | |
| 23 { | Even.... | — | — | — | — | | — | } 46½ | |
| | Morn.... | 29.524 | 55 | 82 | SW | | Sm. rain | | |
| | Noon.... | 29.533 | 64 | 63 | WSW | | Fine | | } 65½ |
| 24 { | Even.... | 29.530 | 60 | 63 | WSW | | Cloudy | } 57 | |
| | Morn.... | 29.283 | 58 | 85 | SW | | Rain | | |
| 25 { | Noon.... | 29.262 | 62 | 65 | WSW | | Rain | } 64 | |
| | Even.... | 29.268 | 55 | 63 | WNW | | Fine | | } 45½ |
| | Morn.... | 29.198 | 50 | 67 | W | | Very fine | | |
| 26 { | Noon.... | 29.198 | 54 | 56 | W by S | | Thun. hl. | } 55 | |
| | Even.... | — | — | — | — | | — | | |
| | Morn.... | 29.422 | 46 | 69 | NW | | Cloudy | | } 42 |
| 27 { | Noon.... | 29.514 | 51 | 63 | NNW | | Cloudy | } 53 | |
| | Even.... | 29.612 | 47 | 61 | NW by W | | Cloudy | | |
| 28 { | Morn.... | 29.670 | 43 | 69 | WSW | | Cloudy | } 37 | |
| | Noon.... | 29.616 | 50 | 70 | WSW | | Rain | | } 52½ |
| | Even.... | 29.592 | 49 | 72 | W by N | | Showery | | |
| 29 { | Morn.... | 29.625 | 48 | 75 | SSW | | Fine | } 42 | |
| | Noon.... | 29.605 | 58 | 65 | WSW | | Fine | | } 59½ |
| | Even.... | 29.572 | 50 | 66 | SSW | | Fine | | |
| 30 { | Morn.... | 29.554 | 51 | 84 | N | | Rain | } 50 | |
| | Noon.... | 29.627 | 57 | 65 | N | | Cloudy | | } 57½ |
| | Even.... | 29.682 | 51 | 63 | NE | | Very fine | | |
| 30 { | Morn.... | 29.594 | 49 | 78 | E by S | | Rain | } 42½ | |
| | Noon.... | 29.621 | 58 | 67 | SSW | | Fine | | } 58½ |
| | Even.... | — | — | — | — | | — | | |

Rain, by the pluviometer, between noon the 1st of Sept. and noon the 1st of Oct. 2.282 inches. Evaporation, during the same period, 3.400 inches.

Astronomical Observations.

| | | | |
|----------|--|-------------|-------------------------|
| Sept. 4. | Immersion of Jupiter's second satellite..... | } 8 20' 07" | Mean Time at Bushey. |
| | | | Mean Time at Greenwich. |
| 5. | Immersion of Jupiter's first satellite..... | } 14 04 27 | Mean Time at Bushey. |
| | | | Mean Time at Greenwich. |
| 21. | Lunar eclipse beginning. | } 17 06 17 | Mean Time at Bushey. |
| | | | Moon set eclipsed. |
| 29. | Emergence of Jupiter's second satellite..... | } 8 09 50 | Mean Time at Bushey. |
| | | | Mean Time at Greenwich. |

ARTICLE XV.

METEOROLOGICAL TABLE.

| 1820. | Wind. | BAROMETER. | | THERMOMETER. | | Evap. | Rain. | Hygr. at
9 a. m. |
|---------|-------|------------|-------|--------------|------|-------|-------|---------------------|
| | | Max. | Min. | Max. | Min. | | | |
| 9th Mo. | | | | | | | | |
| Sept. 1 | N E | 30·12 | 30·10 | 71 | 45 | — | | 60 |
| 2 | N | 30·16 | 30·10 | 67 | 42 | — | | 59 |
| 3 | N | 30·19 | 30·16 | 68 | 38 | — | | 59 |
| 4 | N E | 30·19 | 30·17 | 71 | 39 | 34 | | 60 |
| 5 | N E | 30·17 | 30·10 | 72 | 39 | — | | 69 |
| 6 | Var. | 30·15 | 30·10 | 70 | 39 | — | | 66 |
| 7 | S E | 30·25 | 30·15 | 71 | 49 | — | | 73 |
| 8 | N W | 30·43 | 30·25 | 71 | 42 | — | | 59 |
| 9 | N E | 30·43 | 30·36 | 74 | 42 | 49 | | 60 |
| 10 | W | 30·36 | 30·32 | 74 | 45 | — | | 59 |
| 11 | N | 30·32 | 30·28 | 82 | 53 | — | | 70 |
| 12 | N W | 30·28 | 30·22 | 78 | 48 | — | | 76 |
| 13 | E | 30·22 | 30·01 | 76 | 42 | 40 | | 64 |
| 14 | S E | 30·01 | 29·77 | 78 | 56 | — | | 66 |
| 15 | S W | 30·01 | 30·00 | 70 | 42 | — | 02 | 59 |
| 16 | N W | 30·01 | 30·01 | 69 | 47 | 40 | | 56 |
| 17 | W | 30·01 | 29·61 | 68 | 50 | — | 1·52 | 57 |
| 18 | N W | 29·91 | 29·61 | 56 | 37 | — | 20 | 58 |
| 19 | N W | 30·01 | 29·91 | 66 | 32 | — | | 60 |
| 20 | W | 29·91 | 29·55 | 59 | 40 | — | 37 | 55 |
| 21 | W | 29·91 | 29·75 | 59 | 38 | 35 | 04 | 69 |
| 22 | N W | 30·05 | 29·91 | 59 | 46 | — | — | 63 |
| 23 | S W | 30·05 | 29·80 | 71 | 58 | — | 05 | 71 |
| 24 | S W | 29·80 | 29·74 | 67 | 46 | — | — | 72 |
| 25 | N W | 29·97 | 29·80 | 61 | 38 | 37 | — | 63 |
| 26 | N W | 30·17 | 29·97 | 53 | 29 | — | — | 63 |
| 27 | S W | 30·17 | 30·15 | 56 | 40 | — | 14 | 66 |
| 28 | S W | 30·15 | 30·07 | 64 | 51 | — | 12 | 71 |
| 29 | N | 30·19 | 30·07 | 60 | 34 | — | — | 71 |
| 30 | S E | 30·19 | 30·19 | 66 | 48 | 23 | 03 | 91 |
| | | 30·43 | 29·55 | 82 | 32 | 2·58 | 2·49 | 55—91 |

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Ninth Month.—1. *Cirrus* : *Cirrocumulus*. 2. Fine. 3. *Cirrus* : *Cirrocumulus*. 4. *Cirrus*. 5, 6, 7. Fine : foggy mornings : solar eclipse on the 7th, during which there was a depression of temperature, the particulars of which are given in last number. 8—14. Fine. 15. Cloudy. 16. Fine. 17. Cloudy. 18. Rainy morning : afternoon, fine. 19. Fine. 20. Morning, fine : evening, very wet. 21, 22, 23. Cloudy. 24. Cloudy in the morning : afternoon, very fine. 25. Cloudy. 26. Fine. 27. Rainy. 28. Fine. 30. Cloudy.

RESULTS.

Winds : N, 4 ; NE, 4 ; E, 1 ; SE, 3 ; S, 0 ; SW, 5 ; W, 4 ; NW, 8 ; Var. 1.

Barometer : Mean height

| | |
|---|----------------|
| For the month. | 30·067 inches. |
| For the lunar period, ending the 28th | 30·059 |
| For 14 days, ending the 8th (moon north). | 30·021 |
| For 13 days, ending the 21st (moon south) | 30·030 |

Thermometer : Mean height

| | |
|---|---------|
| For the month. | 55·366° |
| For the lunar period, ending the 28th | 55·65 |
| For 31 days, the sun in Virgo. | 56·693 |

Evaporation 2·58 inch.

Rain 2·49

Mean of hygrometer 64°

The fore part of the lunar eclipse on the 21st was well seen at Tottenham, and it was remarked that the approach of the earth's shadow was preceded by a slight *iridescence*, forming, as it were, part of a halo on that side the moon : this was evidently owing to haze in our own atmosphere. When the earth's shadow had proceeded so far as to bisect the moon vertically, the lower half of the disc having passed behind the edge of a dense *Cirrostratus*, it presented, for a few minutes, the singular appearance of the luminous circle suddenly reduced to a quadrant.

ANNALS
OF
PHILOSOPHY.

DECEMBER, 1820.

ARTICLE I.

*Chemical Analysis of the Needlestone from Kilpatrick, in Dum-
bartonshire.* By Thomas Thomson, M.D. F.R.S.

THE term *zeolite* was first introduced into mineralogy by Cronstedt, and applied by him in the Transactions of the Academy of Sciences of Stockholm for the year 1756, to a mineral possessing the following characters :

1. A little harder than fluor and calcareous spar. Scratched by steel, and not giving sparks with that metal.
2. Melts easily by itself in the fire, and froths up as borax does into a white porous slag, which cannot, without great difficulty, be brought to a solid and transparent state.
3. It is more easily dissolved in the fire, when mixed with soda, than with borax or microcosmic salt.
4. It does not effervesce with microcosmic salt as calcareous spar does, nor with borax as is the case with gypsum.
5. It dissolves very slowly, and without any effervescence in acids, as in sulphuric and nitric acids. If concentrated sulphuric acid be poured on pounded zeolite, a heat arises, and the powder unites into a mass.
6. In the very moment of fusion, it gives out a phosphoric light.*

When Cronstedt published his paper on Zeolite, he does not seem to have been acquainted with that mineral in a regularly crystallized form ; but some years afterwards several new varie-

* Magellan's Cronstedt's Mineralogy, p. 244.

ties were discovered in the mine of Adelfors, in Smoland, some of which did not accurately agree with Cronstedt's description, though they were all confounded together under the general appellation of *zeolite*. And many fine crystallized specimens were brought from the Ferro Islands, and distributed through the different mineral cabinets of Europe. Werner, when he translated Cronstedt's system into German, satisfied himself with subdividing the species *zeolite* into subspecies. These were gradually increased in number; and in Hoffman's *Handbuch der Mineralogie*, published at Freyberg in 1811, 1812, 1815, 1816, 1817, and 1818, which exhibits the Wernerian system of mineralogy in its latest and most perfect form, *zeolite* (purporting to include the *mesotype* and *stilbite* of Haiiy) is subdivided as follows:

| | |
|-----------------------------|---------------------------------------|
| First subspecies | mealy zeolite, |
| Second subspecies | fibrous zeolite, |
| | first kind common, |
| | second kind needle zeolite, |
| Third subspecies | radiated zeolite, |
| Fourth subspecies | foliated zeolite. |

Besides these, Werner introduces into his system as separate species, *cubizite*, *shabasite*, *lomonite*, *natrolite*, and one or two others which had been at first confounded under the name *zeolite*.

In the year 1796, M. Haiiy published in the fourteenth number of the *Journal des Mines* (p. 86), a short paper entitled "Observations sur les Zeolithes." In this paper he subdivides zeolites into four distinct species, distinguished by the shape of the crystals of each. These were *mesotype*, *stilbite*, *analcime*, and *chabasite*.

Mesotype is the zeolite of Cronstedt. Its primitive form, according to Haiiy, is a rectangular prism with square bases. The ratio between a side of the base and the length of the prism is $\sqrt{5} : 2$. The specific gravity of *mesotype* is 2.0833. It becomes electric by heat, like the *tourmalin*, the summits of the crystals being plus, and the part where they were attached to the matrix minus. It gelatinizes in acids, froths when heated, and melts into a spongy enamel.

Stilbite is the radiated and foliated zeolite of Werner. Its primitive form is a rectangular prism with a rectangular base. The breadth, height, and length, of this prism are to each other, as the numbers $\frac{5}{2}$, $\sqrt{3}$, $3\sqrt{2}$. It does not become electric by heat, nor does it gelatinize in acids. Its specific gravity is 2.5.

It is unnecessary to give Haiiy's characters of *analcime* and *chabasite*, as they are universally admitted to be distinct species, and have nothing to do with our present subject.

Natrolite was named and described by Klaproth in 1803. It occurs either amorphous, or in crystals as fine as hairs, the form

of which of consequence cannot be ascertained. Its specific gravity is 2.2. It contains 16.5 per cent. of soda, and 9 per cent. of water. The other constituents are silica and alumina, with a very little iron.*

In the year 1811, Mr. Smithson published a paper in the Philosophical Transactions to demonstrate that *mesotype* and *natrolite* are identical. He made a comparative analysis of each, and found the constituents the same. The following table exhibits his analysis of mesotype, and Klaproth's analysis of natrolite :

| | Mesotype. | Natrolite. |
|-------------------------|----------------|------------|
| Silica | 49.0 | 48.00 |
| Alumina | 27.0 | 24.25 |
| Oxide of iron | — | 1.75 |
| Soda | 17.0 | 16.5 |
| Water | 9.5 | 9.0 |
| | <hr/> 102.5 | <hr/> 99.5 |

There cannot be a doubt that the two minerals analyzed belonged to the same species.

In the year 1816 a very elaborate paper on *Zeolite* was published in Schweigger's Journal by Dr. Fuchs, Professor of Chemistry and Mineralogy at Landshut, vol. xviii. p. 1. In this paper he gives an account of a set of experiments which he and Gehlen had undertaken to determine the composition of all the different varieties of mesotype which they could procure. These experiments occupied them three years, and led them to the conclusion that three different species of mineral are confounded together in the mesotype of Häüy. These they have distinguished by the names of *natrolite*, *skolezite*, and *mesolite*.

1. *Natrolite*, as understood by Fuchs, comprehends under it not merely those minerals called *natrolite* by Werner, but likewise a portion of the mesotype of Häüy ; thus verifying Mr. Smithson's determination of the identity of these two minerals. It occurs occasionally crystallized in four-sided prisms, terminated by low four-sided pyramids. These prisms are not rectangular, as had been supposed by Häüy ; for the faces meet at angles of $91^{\circ} 5'$, and $88^{\circ} 55'$. The angle which a face of the terminating pyramid makes with the corresponding face of the prism is $116^{\circ} 5'$. Natrolite occurs likewise in six-sided and eight-sided prisms.

Specific gravity, 2.256. It scratches glass slightly. Before the blow-pipe it becomes opaque, and then melts in the exterior flame into a transparent and colourless bead. It does not become electric by heat. Its powder dissolves readily in nitric or muriatic acid, and forms a stiff jelly when assisted by a

* Klaproth's Beitrage, v. 44.

moderate heat; and this happens equally whether the mineral has been previously exposed to a red heat or not. It dissolves readily, and almost completely, in oxalic acid. The constituents of this mineral are as follows:

| | |
|---------------|-------|
| Silica | 48·0 |
| Alumina | 26·5 |
| Soda | 16·2 |
| Water | 9·3 |
| | 100·0 |

Thus natrolite contains no lime: it is obviously a compound of three atoms of silicate of alumina, one atom of trisilicate of soda, and two atoms of water; so that its constitution may be thus denoted.



2. *Skolezite* is a much rarer mineral than *natrolite*. The only specimens which Fuchs and Gehlen could procure were from Ferro, Iceland, and Staffa; but they are of opinion that it was known to Häuy, because the characters of his mesotype apply very well to it; and Werner they think classed it with his *needle zeolite*.

The crystals of skolezite bear a close resemblance to those of natrolite, and they are of opinion that the primitive form is either very nearly or absolutely the same. The only crystals in their possession, or which they had seen in the possession of others, were six-sided unequal angular prisms, terminated by a low four-sided pyramid, the faces of which in their opinion corresponded with a prism nearly similar to that of natrolite. The angles of the six-sided prism were $91^\circ 20'$, and $134^\circ 20'$. The angles of the four-sided prism were $91^\circ 20'$, and $88^\circ 40'$. The angle which a face of the terminal pyramid makes with the corresponding face of the prism is $116^\circ 35'$. Fuchs mentions that the faces of the crystals upon which they operated were not so perfectly smooth as to ensure complete accuracy; though they employed the reflecting goniometer. He considers it, however, as demonstrated that the base of the prism is not strictly rectangular, as Häuy supposed. The crystals of skolezite are all thin, and never complete, one extremity only being terminated by a pyramid. Two of the faces of the prism are almost always broader than the remaining four, and these two are strongly streaked longitudinally. It splits in the same direction as natrolite; but the faces thus produced are not sufficiently smooth to admit of the angles being measured.

Specific gravity, 2·214. It is hard enough to scratch glass feebly. When a small portion of it is placed in the exterior flame from the blow-pipe, it becomes opaque, and twists itself up like a worm.* If the exterior flame be continued for some

* Hence I presume the term *skolezite* from $\sigma\kappa\omega\lambda\eta\zeta$, a worm.

time longer, the fragment melts, and is converted into a very bulky and very shining slag. When this slag is exposed to the interior flame, it falls down, and forms a very vesicular and slightly translucent bead.

Skolezite in powder dissolves readily in nitric and muriatic acid before it has been exposed to a red heat, and the solution, like that of natrolite, assumes the form of a stiff jelly. But after being heated to redness, it is no longer soluble in these acids, yet it is decomposed by them, and the silica separates in the state of a fine powder. Oxalic acid only dissolves a portion of skolezite. The undissolved portion is not oxalate of lime.

Skolezite becomes electric when heated, the point of the terminal pyramid being positive, and the other extremity negative.

The constituents of this mineral, as determined by a number of analyses, are as follows :

| | |
|-------------------|-------|
| Silica | 46.5 |
| Alumina | 25.7 |
| Lime. | 14.2 |
| Water..... | 13.6 |
| | 100.0 |

The chief difference between skolezite and natrolite is that the former contains lime in place of the soda which enters into the composition of the latter. It is obviously a compound of three atoms of silicate of alumina, one atom of trisilicate of lime, and three atoms water; so that its constitution may be thus denoted.



3. *Mesolite* is much more common than skolezite. Most of the specimens from Iceland and Ferro belong to it. Tyrol likewise yields it, though not in such abundance as natrolite. By Werner it is arranged under the heads of fibrous and needle zeolite. Haüy confounds it under the general term mesotype.

Its crystals resemble those of natrolite, consisting of four-sided prisms terminated by a low four-sided pyramid. Fuchs and Gehlen met with no specimens of six and eight-sided prisms, though they suppose that such exist. The angles of the four-sided prism were found by the reflecting goniometer to be $91^\circ 25'$, and $88^\circ 35'$. They were not able to determine the angle which a face of the terminating pyramid made with a corresponding face of the prism for want of good specimens.

The specific gravity is stated to be 2.63; but I take it for granted that there is an error of the press, and that the true number ought to be 2.263; for Dr. Freyssmuth took the specific gravity of a Bohemian mesolite, of which he published an analysis last year, and found it 2.333.* And I am not aware that

* Schweigger's Journal, xxv. 426.

any of the minerals hitherto reckoned mesotypes have a specific gravity higher than 2.38. Even the specific gravity of *stilbite*, which Haiiy fixes at 2.5, has not been verified by subsequent experiments. The reader may see a list of the specific gravities of all the zeolites hitherto taken in Hoffmann's *Handbuch der Mineralogie*, vol. ii. part i. p. 242.

The hardness, the structure, the electric properties, and the action of acids on mesolite, agree precisely with the same properties in skolezite. When treated with oxalic acid, it leaves rather a smaller sediment than skolezite. Before the blow-pipe it becomes opaque, and curls up, but not so much as skolezite; neither does it emit quite so much light. It does not form a frothy slag, but melts with the extrication of many air bubbles into a porous and almost opaque bead.

Its constituents, according to the analyses of Fuchs and Gehlen, are as follows:

| | |
|---------------|-------|
| Silica | 47.0 |
| Alumina | 25.9 |
| Lime. | 9.8 |
| Soda. | 5.1 |
| Water | 12.2 |
| | 100.0 |

We see from this analysis that mesolite lies intermediate between natrolite and skolezite. The proportion of silica and alumina in both is nearly the same. The proportion of lime is about two-thirds of that which occurs in skolezite, and the proportion of soda about one-third of what occurs in natrolite. Hence it seems to be a mixture or compound of two parts of skolezite and one part of natrolite.

The constituents of a mineral composed of two parts skolezite and one part natrolite would be as follows:

| | |
|---------------|--------|
| Silica | 47.0 |
| Alumina. | 25.96 |
| Lime | 9.46 |
| Soda | 5.4 |
| Water | 12.16 |
| | 100.00 |

Now this is so very nearly the constituents of mesolite derived from the analyses of Fuchs and Gehlen that it seems at first sight perfectly satisfactory. I confess, however, that I entertain some doubts whether mesolite be any thing more than a mere mixture of the two species. All the analysis of Fuchs and Gehlen indeed approach so near each other that the differences may be fairly ascribed to errors in the experiments; but they do

not furnish us with any historical details respecting the specimens which they examined, excepting merely the localities, viz. Ferro, Iceland, and the Tyrol. Now if these specimens happened to be taken from rocks possessing the very same characters and situation, it might very well happen that they contained nearly the same proportion of constituents, though the natrolite and skolezite of which they were composed were only mechanically mixed.

The specific gravity of a mechanical mixture of two parts skolezite and one part natrolite would be 2.228.

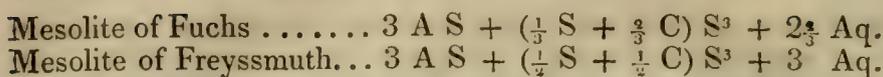
Dr. Freyssmuth lately examined a fibrous zeolite from Bohemia, which he showed to belong to the mesolite of Fuchs and Gehlen. Its specific gravity, as I have already mentioned, was 2.333, and its constituents were as follows :

| | |
|---------------------|---------|
| Silica | 44.562 |
| Alumina | 27.562 |
| Lime. | 7.087 |
| Soda. | 7.688 |
| Water | 14.125 |
| Oxide of iron | Trace |
| | 101.024 |

Here the proportion of soda is increased, and that of lime diminished, and both are nearly what they would be on the supposition that the mesolite analyzed was a mixture or combination of equal parts of natrolite and skolezite ; for the constituents of such a mixture or combination would be

| | |
|---------------|--------|
| Silica | 47.25 |
| Alumina. | 26.10 |
| Lime | 7.10 |
| Soda | 8.10 |
| Water. | 11.45 |
| | 100.00 |

Now this approaches very near the above analysis as far as the lime and the soda are concerned. The quantity of water present in the mineral analyzed by Freyssmuth is greater than it ought to have been while there is a corresponding diminution in the silica and alumina ; yet this analysis appears to me sufficient to show that mesolite is not always exactly the same in its constitution. We must either admit that there are two species of mesolite, one composed of two parts of skolezite and one part of natrolite, and another composed of equal parts of these two minerals, or we must acknowledge that it is merely a mechanical mixture of natrolite and mesolite in various and indefinite proportions. The symbols for the mesolite of Fuchs and Gehlen and of Freyssmuth are obviously the following :



In the *Annals of Philosophy* for September, 1820, (vol. xvi. p. 193,) Mr. Brooke, so well known for his profound knowledge of crystallography, and the enthusiasm with which he has devoted himself to the study, inserted a short paper on Mesotype. In this paper he distinguishes three distinct species from the structure of the crystals, to which he gives the names of mesotype, needlestone, and thomsonite. I have little doubt that Mr. Brooke's mesotype is the natrolite of Fuchs and Gehlen. Neither the specific gravity indeed, nor the measurement of the angles of the crystal, as given by Mr. Brooke, coincide with the previous determinations of Fuchs and Gehlen. But I am disposed to ascribe this to the imperfection of the crystals in the possession of these latter gentlemen, and to the superior dexterity of Mr. Brooke in measuring angles by means of the reflecting goniometer in consequence of his great practice. His goniometer too is, I believe, more sensible than the common ones, being capable of estimating a variation not exceeding 1'. Perhaps the specific gravity of Fuchs, 2.256, is nearer the truth than that of Mr. Brooke, namely, 2.24.

The needlestone of Brooke is obviously the skolezite of Fuchs and Gehlen, and the differences that occur in their descriptions must be ascribed, I believe, to the same causes as those which I have stated when speaking of natrolite.

I was of opinion at first when I perused Mr. Brooke's paper that the needlestone of Dumbarton, which Mr. Brooke has done me the honour to distinguish by my name, was the mesolite of Fuchs and Gehlen; but a careful examination of it, which I have just finished, has satisfied me that this first impression was ill founded, and that it is entitled to rank as a distinct species.

The only specimens which I have seen are from Kilpatrick, near Dumbarton, the same place from which Mr. Brooke obtained his specimens. The crystals are beautifully white, seem perfectly pure, and are of a considerable size. It occurs in considerable quantity in the amygdaloidal rocks, near Kilpatrick, from which it is easily extracted by blasting the rocks with gunpowder. Some years ago the mineral was plentiful. It has now become scarce, though I have no doubt that if a few more rocks were again blasted, it would be found as abundant as ever. I have not been able to meet with a single crystal terminated by a pyramid, so that I am unable to add any thing to what Mr. Brooke has said respecting its crystalline form. It seems to be the only mineral hitherto found to which Haüy's mesotype corresponds nearly. Perhaps, therefore, it might be proper to confine the term mesotype to this species, and to distinguish the other two by the names natrolite and skolezite already given them by Fuchs and Gehlen. Unless this be done, the name

mesotype must be discarded altogether. This, however, I leave to Mr. Brooke's discretion.

The specific gravity at 60° I find 2.36966. This corresponds perfectly with 2.37, assigned by Mr. Brooke. Indeed, as the specimens were pure, and from the same place, any difference between our determinations could only have been ascribed to error on one side or the other.

It is hard enough to scratch fluor spar and stilbite, but is itself scratched by felspar. It is brittle. Easily frangible. The fragments columnar, and the colour snow-white.

Before the blow-pipe, it swells up like borax, and becomes opaque, and snow-white, but does not melt. When exposed to a red heat, it becomes opaque, very white, and shining, similar to enamel, or the finest porcelain. The edges are rounded, but it does not altogether lose its shape. By this treatment, it loses 13 per cent. of its weight, and its specific gravity becomes 2.7166.

I shall now relate the experiments which I made to determine the constituents of this mineral.

Fifty grains of it previously reduced to a fine powder were digested for 24 hours in nitric acid upon the sand-bath. The powder by this treatment was converted into a jelly; but not completely dissolved. I diluted it with water, and poured the whole on a filter. The white matter that remained on the filter, after being well washed, dried, and heated to redness, weighed 18.1 grs.

To ascertain whether this powder was pure silica, I mixed it with thrice its weight of dry carbonate of soda, and exposed it to a red heat in a platinum crucible. It fused into a liquid which on cooling concreted into an opaque mass having a slight shade of green. Muriatic acid dissolved it completely. The solution being concentrated assumed the form of a jelly. It was evaporated to dryness, mixed with water, and thrown upon the filter. The liquid which passed through the filter was colourless; but when mixed with carbonate of ammonia, it let fall a slight reddish coloured precipitate in flocks, weighing 0.6 gr. Of this one half, or 0.3 gr. was dissolved in caustic potash ley. It was therefore alumina. The remaining 0.3 gr. was oxide of iron. Hence the silica amounted to 17.5 grs.

The acid solution was precipitated by carbonate of ammonia. After the white precipitate had subsided, the clear liquid was drawn off, and the white powder was repeatedlyedulcorated with distilled water till the liquid drawn off was quite tasteless. All these aqueous liquids were mixed together, and evaporated to dryness. The dry mass was placed upon a sand-bath in a Wedgewood evaporating dish, and exposed to a heat sufficient to volatilize the sal ammoniac of which it chiefly consisted. There remained behind a very small quantity of a white matter,

which deliquesced in the air, except 0·4 gr. of sulphate of lime.

The liquid being diluted with water was mixed with oxalate of ammonia. A precipitate fell, which was washed, dried, and heated to whiteness. There remained a white powder weighing 0·58 gr. and possessing the properties of lime.

The residual liquid had a slightly yellow colour. It, therefore, contained iron. It was mixed with a liquid of the same nature derived from the white powder, which still remains to be examined. It appears from the preceding statement that a little muriate of lime and muriate of iron were held in solution by the excess of carbonate of ammonia contained in the liquid. The chief object of the preceding experiments was to see whether the needlestone contained soda; but I could discover no traces of any such alkali.

The precipitate thrown down from the acid solution by means of carbonate of ammonia was digested for 24 hours in a moderately strong ley of caustic soda. The greatest part of it was dissolved, but not the whole. The clear soda ley was drawn off by a syphon, and the undissolved portion was repeatedly edulcorated by distilled water. All these liquids were mixed together, and a solution of sal ammoniac poured into them as long as any precipitate continued to appear. The precipitate thus obtained was alumina. It weighed, when dried at a temperature not exceeding 212° , 26·8 grs.; but this weight, on exposure to a red heat, was reduced to 14·56, which was alumina.

The portion which had resisted the action of the soda ley was digested in muriatic acid. It dissolved completely, except 0·9 gr. which was a grey powder, readily fusing into a glass with potash. It was, therefore, silica slightly tinged by iron.

The muriatic acid solution was evaporated to dryness, and redissolved in water, in order to get rid of the excess of acid. It was then mixed with oxalate of ammonia; a white precipitate fell; it was washed, dried, and exposed to a white heat. It now weighed seven grains, and possessed the characters of pure lime.

Into the liquid thus freed from lime a solution of sal ammoniac was poured, a quantity of alumina precipitated, which, after being washed, dried, and heated to redness, weighed 1·08 gr. It seems to have been protected by the lime from the action of the soda ley.

Ammonia being dropped into the residual liquid, a slight precipitate in flocks appeared. At first it was whitish, but it became yellow when collected at the bottom of the vessel; and when edulcorated and dried, it was of a dark red. It possessed the properties of peroxide of iron, though probably, from the mode of obtaining it, some magnesia was mixed with it. The weight was 0·3 gr.

Phosphoric acid was now dropped into the liquid, a very

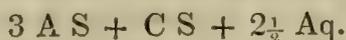
small portion of white precipitate appeared. It weighed when dry 0.3 gr. and was phosphate of magnesia. It is equivalent to about 0.1 gr. of magnesia.

From the preceding analysis, it follows that the Dumbarton needlestone is composed of the following constituents :

| | | | |
|---------------------|-------|--------------|-------|
| Silica. | 18.4 | or per cent. | 36.8 |
| Alumina | 15.68 | | 31.36 |
| Lime. | 7.7 | | 15.4 |
| Magnesia | 0.1 | | 0.2 |
| Peroxide of iron .. | 0.3 | | 0.6 |
| Water | 6.5 | | 13.0 |
| | <hr/> | | <hr/> |
| | 48.68 | | 97.36 |

I suppose the sulphate of lime which made its appearance during the analysis to be derived from a small trace of sulphuric acid contained in the nitric acid employed. This acid was not free from muriatic acid. Hence doubtless the origin of the muriates of lime and iron held in solution by the ammoniacal liquid.

It is obvious enough from the preceding analysis that the Dumbarton needlestone can neither belong to the species natrolite nor skolezite. It contains no soda, which is an essential constituent of natrolite. The lime is nearly the same in it as exists in skolezite ; but its proportion of silica is nearly a fourth less, while its alumina is somewhat more. It is obviously a combination of three atoms of silicate of alumina and one atom of silicate of lime, together with $2\frac{1}{2}$ atoms of water ; so that its symbol is



This is a much simpler and more satisfactory combination than either natrolite or skolezite.

It seems from the observations in the preceding part of this paper to follow that the mesotype of Häüy must hereafter be divided into at least three species ; namely, natrolite, skolezite, and thomsonite, the constitution of which is as follows :

| | |
|----------------------|---|
| Natrolite | $3 \text{ Al S} + \text{ S S}^3 + 2 \text{ Aq.}$ |
| Skolezite. | $3 \text{ Al S} + \text{ C S}^3 + 3 \text{ Aq.}$ |
| Thomsonite | $3 \text{ Al S} + \text{ C S} + 2\frac{1}{2} \text{ Aq.}$ |

The difference between the two last species consists chiefly in the silicate of lime. In skolezite there exists one atom of trisilicate of lime ; but in thomsonite the combination is a simple silicate. As to the very minute quantity of iron and magnesia present, it can only be considered as mechanically mixed, and not chemically combined.

As for the *mesolite*, it seems still doubtful whether it is to be considered as a distinct species, or only a mechanical mixture of

natrolite and skolezite. At present I rather incline to the latter opinion; but as it is of much more frequent occurrence than either natrolite or skolezite, mineralogists cannot find any great difficulty to determine the point. I intend, as soon as I have leisure, which will not be till next summer, to examine the different zeolites in my collection, which are pretty numerous, in order to determine to which of these species each of them belong. If any thing should occur during the course of that examination worthy of attention, I shall take care to lay it before the public.

ARTICLE II.

Observations on the late Solar and Lunar Eclipses, and the Planet Venus. By Dr. Burney.

Eclipse of the Sun, and its Influence on the Diurnal Temperature, Pressure, and Humidity of the Atmosphere.

ANY increase in the diurnal temperature on the earth must be sensibly felt and considered, as caused by the radiation from the sun, of whatever substance that luminary may be composed; and in a solar eclipse like the present, we naturally imagined that a considerable diminution of heat and light would happen, under favourable circumstances, by the interposition of the moon between the sun and the earth. To determine this experimentally, it was found necessary to make previous observations on the progress of the diurnal temperature, in order to have a standard wherewith to compare the results, and accordingly the preceding day, September 6, was chosen, being very fair, for that purpose.

In referring to our notes, we observe that at half-past five, a.m. of that day, the thermometer stood at 51° ; at six, 52° ; at seven, 54° , the sky being then clear, a *stratus*, or low level cloud on the ground, and a fresh N.E. breeze; by eight o'clock it had risen to 58° ; at nine, to 60° ; at ten, to $64\frac{1}{2}^{\circ}$; at eleven, to $66\frac{1}{2}^{\circ}$; at 12, to 67° ; at one, p. m. to $67\frac{1}{2}^{\circ}$: and at two, to 69° being the *maximum* for the day: by three, it had sunk to $65\frac{1}{2}^{\circ}$; at four, $64\frac{1}{2}^{\circ}$; at five, 62° ; and at sunset, to 60° . The day and night were fair, and mostly clear, and the lowest temperature in the night was 50° . This being only 2° *minus* of the lowest temperature in the night after the eclipse, and as the *medium* temperatures of the two days and nights accord with each other within $1\frac{1}{2}^{\circ}$, these preparatory observations may, therefore, be considered a fair standard wherewith to compare the depression of the diurnal temperature during the eclipse. We shall now proceed in detailing the progress of the temperature of Sept. 7, also

occasionally touch upon the pressure and humidity of the atmosphere, and attend to such other circumstances as came under our observation relative to the effects produced on the weather, &c. before, particularly during the eclipse, and after:

Mean time. Therm.

- 5^h 30^m — 52° At sun-rise the *cirrus* clouds were very red, and interspersed about the sky in tufts. Thermometer 2° above the *minimum* for the night. Barometer, 30·10 inches, and an easterly wind.
- 6 0 — 53 A dense *stratus* overspread a great extent of the neighbouring fields and water.
- 7 0 — 55½ *Cirrus* with a slow motion from the W. while the wind came from the E. which indicated opposite currents.
- 8 0 — 59 Extensive beds of *cirrostratus* followed the last modification of cloud, and nascent *cumuli* appeared to the eastward.
- 9 0 — 62 The sky appeared of an azure hue, and the clouds dispersed.
- 10 30 — 64 Lofty *cirri* transformed into beds of *cirrocumulus*; and the wind veered round from E. to S.E. by S.
- 11 0 — 66 The last modification was composed of beds of small round lofty well-connected clouds in an azure sky, and passed slowly to the eastward. De Luc's hygrometer at 50°, being the driest state of the atmosphere for the day.
- 12 0 — 69 Attenuated *cirrostratus* beneath *cirrocumulus* screened the sun's disc, it was impelled eastward by the force of an upper current; the lower current from S.S.E. thus following the sun. Barometer, 30·14 inches; and hygrometer, 51°.
- 12 10 — 69½ Attentive at the telescope, the sun being scarcely perceptible for clouds, which increased our anxiety to observe the beginning of the eclipse.
- 12 14 — 70 The sun's disc now got visible in the attenuated parts of the passing clouds; and the thermometer at its *maximum* height for the day.
- 12 16½ — 70 A clearish space presented itself, and the lower limb of the moon had just come in contact with the sun's upper limb. This we considered the beginning of the eclipse.
- 12 20 — 69¾ The moon's limb advanced slowly over the sun, with a well-defined edge.
- 12 25 — 69¼ The clouds passed off to the eastward, and only a *cirrocumulus* was left under the sun. With

Mean time. Ther.

- a common burning glass, woollen cloth, thin paper, and tinder were kindled, the same as at this time yesterday.
- 12 30 — 69 The clouds generally dispersed by a freshening breeze, and the barometer rose slowly.
- 1 0 — 65 The solar heat and light rapidly diminished; and the eclipse went on favourably.
- 1 20 — $63\frac{3}{4}$ The clouds all dispersed near the horizon, except a bed of *cirrocumulus* to the eastward. The planet Venus, of a silvery colour, was now conspicuous to the naked eye, nearly in the western point of the compass, and by a sextant $40^{\circ} 55'$ distant from the centre of the sun. Viewed through an inverting achromatic telescope, she presented only an illuminated crescent, like the moon three or four days after change. She continued in sight one hour and twenty minutes, and was then hidden by a cloud.
- 1 30 — 62 The western sky appeared much darker than the eastern, not from clouds, but from the deprivation of solar light: indeed, the general darkness visibly increased, and the decrease in temperature was sensibly felt. Barometer, 30.15 inches, being the *maximum* pressure for the day.
- 1 45 — 62 The greatest obscuration of the sun's disc very near; and a fresh breeze sprung up from the S.E. The burning-glass would not now kindle tinder; but the freshness of the breeze might have had some effect on this experiment.
- 1 $46\frac{2}{3}$ — 61 Greatest obscuration. The sun now only presented to us through a smoked glass a small (red) crescent, like that of the moon two or three days old. The moon preserved a definite edge, and her body appeared like a spherical mass of *cooling* iron. The nearest comparison we could draw on the obscuration or deprivation of solar light was, that it represented the *crepusculum*, when the sun has verged 2° or 3° under the horizon, with this difference, that the rays were stronger, and the mingled shade considerably darker, but the clouds not tinged with prismatic colours, as by the horizontal radiation from the setting sun.
- 1 50 — $60\frac{1}{2}$ The planet Mars now presented himself to the

Mean time. Ther.

- naked eye, was about 36° to the E. of the sun, and shone with a full orb of a bright gold colour nearly 10 minutes. The breeze continued fresh. So interesting was a sight of this great-eclipse and the planets at this moment, that all classes of people here were looking through their smoked and tinged glasses, both from their gardens and house tops.
- 2 0 — $60\frac{1}{2}$ The difference in the increment of light was scarcely distinguishable. The sky remained clear, and of a dark blue colour; and the distant clouds near the horizon lost part of their light, and descended into the lower atmosphere, as is frequently observed at or soon after sunset. The birds too, both small and great, were observed flying over, as if hastening to their respective nocturnal resting places.
- 2 15 — 60 A Fahrenheit's thermometer in the neighbourhood that was more exposed to the S.E. breeze, sunk to 58° about this time, being the *minimum* diurnal temperature. The index of the hygrometer advanced to 57° . The light perceptibly increased, and cast curious but imperfect shadows of objects on the leads. A lofty *cirrocumulus* sprung up in the vicinity of the sun, and stretched to the N. and S. horizon.
- 2 30 — $60\frac{1}{2}$ The temperature rose again; and the light was about the same strength as at one, p.m. The breeze died away; but a broad band of *cirrus* appeared to the westward.
- 3 0 — $63\frac{1}{4}$ The eclipse drew to a close. Barometer, 30.12 inches.
- 10 — 64 End of the eclipse. The sun at the same time lightly shaded by a passing *cirrus*, but not sufficient to prevent a clear view of the separation of the limbs of the sun and moon. It appeared that the solar light was not now so brilliant as at 15 minutes after the beginning of the eclipse.
- 4 0 — 66 A faint solar halo in an attenuated *cirrostratus*.
- 5 0 — 63 *Cumulostratus* formed near the western horizon.
- 6 0 — 61 The whole hemisphere overcast by the last modification.
- 6 30 — 60 The temperature the same as at 15 minutes past two, p.m. The *minimum* temperature in the night was 52° , that is, 2° above that of the preceding night.

The sky presented the same modifications of cloud yesterday as to day, except the *cumulostratus*; but the *cirrocumuli* were more perfectly formed, and more prevalent to day. As this is an indication of a rising temperature, and as the diurnal temperature was evidently checked in its rising an hour and a half before it generally arrives at its *maximum* height here, we are inclined to think that the thermometer would have risen 2° higher, which, if added to the *maximum* for the day, will make 72° ; so that admitting the state of the thermometer that was not so much exposed to the fresh breeze, namely, 60° , soon after the greatest obscuration, we obtained by experiment a depression of 12° of Fahrenheit's thermometer that was placed in a northern aspect, and in the shade 10 feet above the ground, by the influence of the solar eclipse on the temperature of the day. Indeed, we still feel more confident in this opinion, when we consider that the *minimum* temperature in the night of the 6th was 2° lower than in the night of the 7th. It is stated in the newspapers that at the Royal Exchange in London the thermometer had fallen but 6° at the time of the greatest obscuration; but it must be recollected that the greatest depression did not occur for some time after that period, and that the thermometer ought to have been watched longer.

To determine this point in previous solar eclipses, it appears that thermometrical observations were made by Dr. John Bevis on April 1, 1764; and by Luke Howard, Esq. of Tottenham, near London, on Nov. 19, 1816; the former gentleman stated that "Fahrenheit's thermometer placed without door to the N. stood at 50° when the eclipse began, and fell but one division" (or one degree) "while it lasted;" and the latter ingeniously shows in his Meteorological Observations lately published, that his thermometer on Six's construction did not sink more than one degree; but on account of the forenoon temperature having been checked in its rising by a considerable portion of the sun's rays being intercepted, he therefore allows a depression of 3° as a fair result.

It will, however, be really seen, that neither the months, nor the hours in which those eclipses happened were so favourable for ascertaining the best results of such an inquiry, as the time which this eclipse afforded.

The barometer rose $\frac{1}{100}$ th, and sunk $\frac{1}{100}$ th of an inch; and De Luc's whalebone hygrometer ranged from 51° to 57° during the eclipse.

Observatory, Gosport, Sept. 9, 1820.

Eclipse of the Moon.

The moon's southern limb immersed in the north side of the earth's shadow, at two minutes past five, a.m. mean time. From the beginning of the eclipse, the moon gradually grew dim, changing from a whitish light to a dull red colour; and the

edge of the earth's shadow was ill-defined on her disc, on account of the *penumbra*. At 22 minutes past five, when she was one-fourth eclipsed, she immersed behind a *cirrostratus* cloud near the horizon, through which she afterwards had but a faint appearance. Until she sunk beneath this cloud, the eclipsed part of her disc was visible to the naked eye through the shadow of the earth.

The moon set half eclipsed, but scarcely visible for clouds, at 45' 50" past five o'clock.

At the middle of the eclipse the digits eclipsed were more than 10° , which is nearly equal to that of the late solar eclipse.

This eclipse was rendered more interesting from its position being about midway between the planets Jupiter and Saturn.

Gosport Observatory, Sept. 22, 1820.

The Planet Venus.

This brilliant planet has this week excited much curiosity among the inhabitants of this town and neighbourhood, having been seen with the naked eye, when the clouds did not intervene, from eight, a. m. till late in the afternoon, by hundreds of persons of all ages assembled in the streets, &c. for that purpose.

Formerly it was considered an uncommon observation to discover Venus with the naked eye at noon: we have, however, for some years past, often seen her with the naked eye on both sides of the sun through a clear azure atmosphere at mid-day, while ranging in her orbit from 40° to 48° from that luminary; as she passes him from side to side to this distance, which is considered to be her greatest elongation.

On the 19th of last May she was at her greatest elongation, and afterwards seen here with the naked eye on many clear days till the 25th of June, when she was about 38° to the east of the sun, and appeared through a telescope only in the shape of a crescent.

On the 30th of July the planet came to her *inferior* conjunction; namely, between the earth and the sun. In passing from this point, she was on the 7th instant 41° nearly to the west of the sun, and is now Sept. 22, 45° to the W. of him. By the 8th of October she will have arrived at her greatest western elongation; till then, and for several weeks after, she may be viewed with the naked eye in the open day, when the sky is clear in that direction.

Since the 1st instant, up to the present time, we have seen this planet with the naked eye in fair weather at almost all hours of the day while she was above the horizon. Venus changes her phases like the moon, according to her position with respect

to the sun and the earth, which causes a considerable difference in her brilliancy.

She is now approaching to the shape of a *half-moon*, and would appear more brilliant to the naked eye, if she were shining with a full illuminated disc.

Observatory, Gosport, Sept. 22, 1820.

ARTICLE III.

Mathematical Problem. By Mr. James Adams.

(To Dr. Thomson.)

SIR,

Stonehouse, Sept. 3, 1820.

IF you will be pleased to insert the following problem, &c. in the *Annals of Philosophy*, when convenient, you will oblige

Your humble servant,

JAMES ADAMS.

Problem.—To find the value of $\frac{v^m - w^m}{v - w}$, when v is greater than w , and m any given number.

Put $v - w = r$, then will $v^m = (w + r)^m$ and $w^m = (v - r)^m$, we then have

$$\frac{v^m - w^m}{v - w} = \frac{(w + r)^m - w^m}{r} = \left(1 + \frac{m - 1}{2} \cdot \frac{r}{w} + \frac{(m - 1)(m - 2)}{2 \cdot 3} \cdot \left(\frac{r}{w}\right)^2 + \&c. \right) m w^{m-1} \dots\dots\dots (E.)$$

By substituting for w^m , we have

$$\frac{v^m - w^m}{v - w} = \frac{v^m - (v - r)^m}{r} = \left(1 - \frac{m - 1}{2} \cdot \frac{r}{v} + \frac{(m - 1)(m - 2)}{2 \cdot 3} \cdot \left(\frac{r}{v}\right)^2 + \&c. \right) m v^{m-1} \dots\dots\dots (F.)$$

If in the preceding equations $v - w = r$, be supposed *indefinitely* small, then may all the terms affected with $\frac{r}{w}$, $\left(\frac{r}{w}\right)^2$, $\left(\frac{r}{w}\right)^3$, $\frac{r}{v}$, $\left(\frac{r}{v}\right)^2$, $\left(\frac{r}{v}\right)^3$, &c. be omitted as being *indefinitely* small; on this supposition we should have $\frac{v^m - w^m}{v - w} = m w^{m-1}$, or $m v^{m-1}$, *indefinitely* near $\dots\dots\dots$

The expression given in the problem may be differently represented as follows, viz.

$$\frac{v^m - w^m}{v - w} = \frac{1 - \left(\frac{w}{v}\right)^m}{1 - \frac{w}{v}} \cdot v^{m-1} = \left(1 + \frac{w}{v} + \left(\frac{w}{v}\right)^2 + \left(\frac{w}{v}\right)^3 + \&c.\right) v^{m-1} = A \cdot v^{m-1} \dots \dots \dots (I.)$$

$$\frac{v^n - w^n}{v - w} = \frac{1 - \left(\frac{w}{v}\right)^n}{1 - \left(\frac{w}{v}\right)} \cdot v^{n-1} = \left(1 + \left(\frac{w}{v}\right) + \left(\frac{w}{v}\right)^2 + \left(\frac{w}{v}\right)^3 + \&c.\right) v^{n-1} = B \cdot v^{n-1} \dots \dots \dots (II.)$$

$$\frac{v^r - w^r}{v - w} = \frac{1 - \left(\frac{w}{v}\right)^r}{1 - \left(\frac{w}{v}\right)} \cdot v^{r-1} = \left(1 + \left(\frac{w}{v}\right) + \left(\frac{w}{v}\right)^2 + \left(\frac{w}{v}\right)^3 + \&c.\right) v^{r-1} = C \cdot v^{r-1} \dots \dots \dots (III.)$$

$$\frac{v^s - w^s}{v - w} = \frac{1 - \left(\frac{w}{v}\right)^s}{1 - \left(\frac{w}{v}\right)} \cdot v^{s-1} = \left(1 + \left(\frac{w}{v}\right) + \left(\frac{w}{v}\right)^2 + \left(\frac{w}{v}\right)^3 + \&c.\right) v^{s-1} = D \cdot v^{s-1} \dots \dots \dots (IV.)$$

&c. &c.

Hence we obtain

$$\frac{I}{II} = \frac{v^m - w^m}{v - w} = \frac{A}{B} \cdot v^{m-1} \dots \dots \dots (V.)$$

$$\frac{V}{III} = \frac{v^r - w^r}{v - w} = \frac{A}{B C} \cdot v^{r-1} \dots \dots \dots (VI.)$$

$$\frac{VI}{IV} = \frac{v^s - w^s}{v - w} = \frac{A}{B C D} \cdot v^{s-1} \dots \dots \dots (VII.)$$

&c. &c.

The equation marked V. when $n = \frac{m}{r}$, gives

$$\frac{v^{\frac{m}{r}} - w^{\frac{m}{r}}}{v - w} = \frac{1 + \frac{w}{v} + \left(\frac{w}{v}\right)^2 + \left(\frac{w}{v}\right)^3 + \text{to } m \text{ terms}}{1 + \left(\frac{w}{v}\right) + \left(\frac{w}{v}\right)^2 + \left(\frac{w}{v}\right)^3 + \text{to } r \text{ terms}} \times v^{\frac{m}{r}-1} \dots (VIII.)$$

The same as given by Mr. Landon, p. 6, Residual Analysis.

If in equation I. we write $\frac{w}{n}$ for w , we shall have

$$\frac{1 - \left(\frac{1}{n}\right)^m}{1 - \frac{1}{n}} \cdot v^{m-1} = \left(1 + \frac{1}{n} + \frac{1}{n^2} + \frac{1}{n^3} + \dots \dots \dots \frac{1}{n^{m-1}}\right) v^{m-1}.$$

The same in substance is given in the Edinburgh Philosophical Journal for July, 1820.

Now it is evident that each of the foregoing series was produced from their corresponding roots; and it is also evident that those

series which are always supposed to be of *some magnitude*, could not have been produced from *nothing*, that is, the expression, $\frac{v^m - w^m}{v - w}$, cannot, by any operation whatever, produce a *given magnitude* when v is equal to w ; but when $v - w$ is *indefinitely small*, as before remarked, we may then, without any *sensible error*, consider $\frac{v}{w}$ or $\frac{w}{v}$ equal to unity; on this supposition, the right hand side of Equation I. would become $m v^{m-1}$, and the right hand side of Equation VIII. would become $\frac{m}{r} v^{\frac{m}{r}-1}$. Hence it follows that when $v - w$ is *indefinitely small*, the expression $\frac{v^m - w^m}{v - w}$, will be indefinitely near to $m v^{m-1}$ or $m w^{m-1}$; but there never can be an *absolute equality* between these expressions.*— (See Analyst, Section 15.)

The following example is answered according to Equations marked (E) and VIII.

Example.—Given $v = 11$, $w = 10$, and $m = \frac{4}{3}$, to find the true and approximate values of $\frac{11^{\frac{4}{3}} - 10^{\frac{4}{3}}}{11 - 10}$.

First answer from Equation (E).

$$1 \dots\dots\dots = + 1.00000000$$

$$\frac{m-1}{2} \cdot \frac{r}{w} = \frac{\frac{4}{3}-1}{2} \cdot \frac{1}{10} = \frac{1}{60} \dots\dots\dots = + 0.01666666$$

$$\frac{m-2}{3} \cdot \frac{r}{w} \cdot \frac{1}{60} = \frac{\frac{4}{3}-2}{3} \cdot \frac{1}{10} \cdot \frac{1}{60} = -\frac{1}{2700} \dots\dots = - 0.00037023$$

$$\frac{m-3}{4} \cdot \frac{r}{w} \cdot -\frac{1}{2700} = \frac{\frac{4}{3}-3}{4} \cdot \frac{1}{10} \cdot -\frac{1}{2700} = \frac{1}{64800} = + 0.00001543$$

$$\text{The sum of four terms} \dots\dots\dots = + 1.01631186$$

$$m w^{m-1} = \frac{4}{3} 10^{\frac{4}{3}-1} = \frac{4}{3} 10^{\frac{1}{3}} = \frac{4}{3} \times 2.1544347 = 2.8725796$$

And $2.8725796 \times 1.01631186 = 2.919432$, which nearly agrees

$$\text{with } \frac{11^{\frac{4}{3}} - 10^{\frac{4}{3}}}{11 - 10} = 11^{\frac{4}{3}} - 10^{\frac{4}{3}} = (121.121)^{\frac{1}{3}} - (10000)^{\frac{1}{3}} \\ = 2.91942 = \text{true value.}$$

* From Equation 1, we have $v_m - w_m = (v - w) A v^{m-1} = r A v^{m-1}$, where $v - w = r$ may represent *any given quantity*; but if we suppose $r = 0$, or $v = w$, we shall then have $0 = 0 \times A v^{m-1}$ or $A v^{m-1} = \frac{0}{0} = 0$.

Second answer from Equation VIII.

| | |
|--|--|
| $1 \dots \dots \dots = 1.0000000$
$\frac{w}{v} = \frac{10}{11} \dots \dots = 0.9090909$
$\left(\frac{w}{v}\right)^2 = \frac{10^2}{11^2} \dots \dots = 0.8264463$
$\left(\frac{w}{v}\right)^3 = \frac{10^3}{11^3} \dots \dots = 0.7513148$ | $1 \dots \dots \dots = 1.00000$
$\left(\frac{w}{v}\right)^{\frac{m}{r}} = \left(\frac{10}{11}\right)^{\frac{4}{3}} \dots \dots = 0.88066$
$\left(\frac{w}{v}\right)^{\frac{2m}{r}} = \left(\frac{10}{11}\right)^{\frac{8}{3}} \dots \dots = 0.77557$ |
| Four terms of
the numerator = 3.4868520 | Three terms of the de-
nominator \dots \dots = 2.65623 |

$$11^{\frac{4}{3}-1} = 11^{\frac{1}{3}} = 2.2398, \text{ we then have } \frac{11^{\frac{4}{3}} - 10^{\frac{4}{3}}}{11 - 10}$$

$$= \frac{3.486852 \times 2.2398}{2.65623} = 2.91945 \text{ nearly.}$$

| | |
|--|------------------------|
| Therefore the true value of $\frac{11^{\frac{4}{3}} - 10^{\frac{4}{3}}}{11 - 10} \dots \dots \dots$ | = 2.91942 |
| Value by series (E) \dots \dots \dots | = 2.91943 |
| Value by series VIII. \dots \dots \dots | = 2.91945 |
| Approximate values $\left\{ \begin{array}{l} \frac{4}{3} \cdot 11^{\frac{1}{3}} \dots \dots \dots \\ \frac{4}{3} \cdot 10^{\frac{1}{3}} \dots \dots \dots \end{array} \right.$ | = 2.96531
= 2.87258 |

ARTICLE IV.

Reply to Mr. Boase on Rain-Gauges. By Mr. Meikle.

(To Dr. Thomson.)

SIR,

Berners'-street, Sept. 21, 1820.

IT is with no small reluctance that I again resume a subject which I had already more than once sufficiently discussed. Although I never adapt my writings to those who know nothing of science at all, yet I presume they are extremely plain and intelligible to all who are any way initiated in the first principles of the mathematical sciences.

Should M. Flaugergues have seen my very short note which you had the goodness to insert in the *Annals of Philosophy* for Oct. 1819, I do not in the least doubt that he would have been fully convinced of his mistake; and I should also hope that a perusal of your number for April last has likewise satisfied Mr. Holt of his having laboured under the same delusion. But what shall we say of Mr. Boase? who, notwithstanding these

warnings, comes forward with such an enchanted story in your present number, as if he had tried how absurdly a person really can write when so inclined. Indeed it is difficult to conceive how he could have been blessed with such a view of the subject even in a dream. His diagrams * are more like an attempt at caricaturing than any thing else. Is it possible that one who so writes has seen the first book of Euclid?

I shall not now encroach on your pages with a formal dissection and refutation of such nonsense; for I should still entertain hopes of Mr. Boase if he would only give my two former papers a careful perusal. This he seems never to have done, and cannot easily do, so long as prejudice tyrannizes over reason. I should also take it as a particular favour if any of your correspondents who are conversant with the subject would be so good as point out any error, if such there be, in these papers. But should they incline to treat the subject in the random way Mr. Boase has been pleased to do, I hope they will excuse me in allotting them a share of something which I may, perhaps, trouble you with inserting in a future number. As the point in dispute is purely geometrical, persons who are eminently endowed with dogmatism will probably find that a little knowledge of geometry would have been of infinitely more use.

It is something singular that a man of science like Flaugergues should have fallen into such a silly mistake; but is it not truly provoking that the same glaring absurdity should again be obtruded on the public after I had repeatedly refuted it in a way so simple that a child might understand it? Indeed I despair of being able to give a much simpler view of the case, unless I could make it so as Mr. Boase might feel it with his fingers.

At present I shall content myself with giving the following very familiar mode of proving that the *horizontal* distance of the lines in which the drops fall is not altered by a steady wind:—Suppose the wind to blow from London towards Penzance, and that 1000 rain-drops having all the same altitude are carried a furlong nearer Penzance by the time they reach the ground. Now it is manifest that the relative distances of these drops reckoned *horizontally* in the direction of Penzance is not altered in any part of their descent, because the wind acts *equally* on every one of them, however whimsical the curve through which it may descend, † or however different the velocity of the wind may be at different altitudes, provided only it act equally on every drop at the same instant, ultimately carrying each just one urlong nearer Penzance than it was before: for Mr. Boase has

* One of these he calls “Mr. Meikle’s own diagram.” This I am disposed to deny: the top indeed resembles my figure, but he has taken care to put his own comet-like tail to it. Had he used my diagram, he would have been in danger of stumbling on the truth.

† Mr. B. informs us that the drops describe “parabolic lines.” This I did not know before; nor do I see any reason to believe they are restricted to any particular sort of curve whatever.

taught us that equal causes produce equal effects. A rain-gauge, therefore, which would just receive the 1000 drops at the height we at first supposed them, would exactly receive them at the earth's surface, let their *general* inclination at any time be what it may.

Mr. Boase himself seems to be the very first person who would like to prove "that *equal* forces acting on equal drops produce *unequal* effects;" for he tells us that "the wind disperses the rain over a *greater* surface;" that is, *it carries one drop much further than it does another*—a pretty specimen of *equal effect!!!*

In order the more effectually to delude himself, Mr. Boase very prudently takes care not to trace all his drops from the *same* height, nor to make them all describe *equal curves similarly situated with respect to the horizon*.* Is it not distressing to see him go through so many numerical computations, while the principles on which they are founded have no existence but in his own imagination? In short, throughout the whole he seems to enjoy much of that perfect confidence and composure which usually characterize a well-established delusion.

After what I had formerly advanced on this subject, I should not have considered your correspondent's paper deserving of any reply whatever, were it not that I find there are many still labouring under the same hapless mistake.

With the view of guarding against any misconstruction, I beg to remark that it was never my object, though Mr. B. seems to think so in his *first* article, to disprove the well-known fact of a rain-gauge collecting less water in a more exposed situation. On the contrary, I have admitted that in my former papers; but my object was to prove, as I have so often done, that the *general* inclination of the rain, in which my learned opponents rest their explanation, does not in the least affect the phenomenon in question; and that, therefore, we must seek for an explanation in something else; probably, as I said before, in the obstruction which the gauge itself offers to the wind. I am, Sir,

Your most obedient servant,

HENRY MEIKLE.

* The following simple facts are obvious to every one who has the least acquaintance with curves; and are well worth the attention of your correspondent:

If two curves, which are every way equal, and similarly situated on parallel planes, have two parallel straight lines for their axes, then all straight lines joining the corresponding points of these curves are equal to one another.

Now in the curves described by the rain-drops, the *horizontal* distance is always the distance of two corresponding points.

It is not possible for two equal curves similarly situated in the same plane to be every where equidistant; or to have their perpendicular distance every where the same. Straight lines only have this property.

ARTICLE V.

On the Impermeability of Wire Gauze to Flame.

By Mr. Murray.

(To Dr. Thomson.)

SIR,

AN anonymous correspondent in your *Annals*, I believe, was the first to suggest that the impermeability of wire gauze to flame might be ascribed to its cooling influence, and it was adopted by that eminent chemist, Sir H. Davy, who very laudably endeavoured to prove its truth, or fallacy, by direct experiment. Now I must in candour submit, with every deference to that great authority, that though his conclusions may seem satisfactory to his own ingenious mind, that there are not a few who cannot say *Amen*, and of that number I am one. No persons, Sir, can have a more exalted opinion of that philosopher than myself, but no opinion shall pass current with me *merely* because it bears the signet and superscription of a great name. I have attentively considered his experiments, and repeated some of them.

I confess that I was inclined at first to think that a solution might be formed in the products of combustion. The aqueous vapour and carbonic acid which, together with the unaltered azote of the atmosphere surrounding the cylinder exteriorly might form, it occurred to me, a zone pervious to gaseous matter, and yet impervious to flame. The meshes of the safety lamp would, in like manner, be filled up with the same negative materials. This, however, was a mere opinion, though it seemed deducible from some experiments made by me, and recorded in the pages of the *Philosophical Journal*.

I would now find a solution of the question in the structure of flame itself. Flame is clearly a film: this, as I have already shown, may be demonstrated by pressing on the apex of a cone of flame by means of a plate of glass, or suffering alcohol or ether to be set on fire on the same plane. But press gently on the cone of flame, and it will be seen to recoil or curve downwards, whereas it should *become extinct* in contact with the wire gauze, if a cooling influence were exerted. In like manner when we incline the wire gauze laterally in the direction of any of the conic sections, the film will separate, and the mantle widen and elongate laterally.

It is hence clear from the very structure of flame considered as a film that it cannot pass through, even when the wire gauze is ignited, because if a film were forced through, it must necessarily become *solid*. We find an analogy in water placed on a plane of fine wire gauze, it spreads a filmy web over the meshes, and is effectually retained, but when the meshes are

sufficiently enlarged, the water passes through in the form of minute spheres. Hence perhaps the increased energy of the common blow-pipe, the film, or successive films, being directed to a focal point and becoming solid.

When the temperature of the tissue of the safety lamp is sufficiently exalted, the gaseous matters in contact with its external surface are kindled, and this high temperature is soon acquired by the continued inflammation of the explosive medium engaged. The iron or other metallic wire of the meshes radiates caloric indeed, but receives much more than it yields from the flaming cone; and by such repeated accessions it soon rises to ignition, which is accelerated when the explosive flame amounts to a maximum, and it plays in impotent fury against the bars of its prison, the walls of that enclosure which impose a limit to its sphere of action.

Thus, when wire gauze is acted upon by a continued jet of inflamed gas, the barrier is soon made sufficiently red-hot to kindle the gas which is propelled through unconsumed, and which, therefore, exhibits a prolongation of the inflamed jet on the other side. Thus gunpowder placed on a piece of fine wire gauze resting on flame will not explode, until the wire gauze is sufficiently ignited. But if the phenomenon of safety in the wire gauze is to be ascribed to cooling influence, then I presume to contend that alcohol, ether, or other inflammable matters should not flame or continue to burn in contact with cooling surfaces, yet alcohol, &c. may be kindled, and will continue to inflame on a plane of copper, &c. the best conductors of caloric among the metals.

In the occasional retrogression of the flame into the reservoir of the oxyhydrogen blow-pipe, as the probabilities are that this flame is a *solid*, an idea first suggested by Mr. George Oswald Sym, from the very nature of the materials employed, so it cannot be subject to the restriction presumed, as it is no longer a film. But the retreat of flame into the reservoir through so many centinels over its safety, seems to me, on the principle of the supposed cooling influence, a problem which admits of no solution. It has to encounter no less than four safety appendages, and finally brave the portal of a safety valve before it can penetrate the recipient; and yet it must enter this *meatus internus* in the character of flame to do its mischief. Now really one would think that it might be sufficiently cooled before it came this length.

Thus do we find a very simple expression of the facts, and I think a satisfactory one in the structure and constitution of flame itself; while, upon the other supposition, when we consider that wire gauze, even at a dull red heat, maintains its character of safety until it becomes of a temperature equal to that required to explode fire damp; and reflect also that meshes of paper, hair-cloth, &c. nonconductors of caloric, whose cooling influences are

problematical, interdict the exit of flame; we should, I think, not adopt, without some hesitation, an opinion which supposes so much: for myself, the more I consider it, the more gratuitous seems the assumption. I have the honour to be, Sir,

Your most humble and obedient servant,

J. MURRAY.

ARTICLE VI.

On the Origin of the Name of Calomel.

By S. F. Gray, Lecturer on the Materia Medica, &c.

In the *Annals of Philosophy* for October last, p. 309, a question is started respecting the origin of the name calomel, usually written in Latin calomelas, being given to Mercurius dulcis, at least when prepared by repeated sublimations. To this a correspondent has replied in your November number, and suggested that calomel or calomela is probably the Arabic name adopted by Spielman in 1766. It were to be wished that this correspondent had given his authority for the name calomel being used by the Arab writers before it was introduced in Europe, especially as the etymology given by him, namely, *burning rust*, is scarcely applicable to Mercurius dulcis.

In the *Cynosura Materiae Medicæ* of Paul Hermann, my copy of which is that published in 1726 by Boecler, but which states that the work had been published 16 years before by Henninger, and of course in 1710, I find (Part I. p. 687, and Part III. p. 9) a mixture of Mercurius dulcis with twice or thrice as much scammony, mentioned as the calomelas of Riverius, and said to be frequently ordered in his writings. This carries up the use of the word calomel more than half a century before Spielman; namely, to 1710, if not earlier, as Prof. Hermann died in 1695, and M. Riviere in 1656. The mixture of the white Mercurius dulcis with the dark coloured scammony gives some degree of probability to its alledged derivation from the Greek *καλος*, white, or fair; and *μελας*, black, or dark. I have not, however, been able to find the word calomel used in my edition of Riverii *Praxis Medica*, viz. the 7th, 1653, although in treating of the diseases of the liver, he recommends *Mercurius dulcis* to be used conjointly with scammony; but it may have been used in some later edition, or perhaps in the French ones.

As to the use of the word calomel, for Mercurius dulcis itself, Wiegleb, who was very well versed in the literary history of chemistry, designates calomel as a synonym of De la Brune's Mercurial Panacea. Of this author I have no recollection at present, but if he was the first introducer of these two appellations, he must have written upon this substance, or advertised it

by those names, before 1700, when Camerarius published a treatise, *De Panacea Mercuriali*, which was probably an inaugural thesis on that subject as a celebrated nostrum.

In respect to the etymology of the name, Gmelin, in his continuation of Murray's *Apparatus Medicaminum* inclines to the use of calomel, in Latin, rather than calomelas, for Mercurius dulcis, whence it should seem as if he considered it as a combination of *καλος* and *μελι*, the latter referring, as he supposes, to its honeylike sweetness; and agreeably to this idea, some authors call it *manna Mercurii*.

ARTICLE VII.

On Polyhalite, a new Species of Saline Mineral. By M. Fred. Stromeyer. (Read at the Anniversary Meeting of the Royal Society of Gottingen, Dec. 5, 1818.)

EXPERIENCE sufficiently proves to us that nothing certain can be established or affirmed respecting the real nature of unorganized bodies, or their specific differences, from the mere inspection and examination of their external and physical marks; that mineralogy must have recourse to the aid of chemistry in order to corroborate and assert those proofs which are deducible from the physical configuration of such bodies; and that the former science, unless assisted by the latter, is inadequate to the attainment of sufficient data. Yet notwithstanding that, from the intimate connexion observable between the chemical composition of mineral substances and their physical properties, it appears to be indubitably proved what great advantage the science of mineralogy has already derived from chemistry, and particularly, how greatly chemistry has of late assisted us in attaining a true knowledge of mineral bodies, there are certain naturalists who believe that mineralogy has no need of chemistry in order to arrange and characterize the different species of minerals, and that the indications afforded by the external appearances or internal structure of such bodies is quite sufficient in order to class them, and become acquainted with them. Yet the fallacy of this opinion, and its tendency to promote error, will, it is presumed, become apparent from a chemical analysis of the polyhalite, a new species of saline mineral hitherto classed as a variety of the anhydrites. This mineral is found at Ischel, a town of Upper Austria, not far from the borders of Salzburg, where it is met with among strata of rock salt.

It was at first supposed to be a variety of the *gypsum fibrosum*. Subsequently Werner, that illustrious mineralogist, whose death must be deplored by all who cultivate and admire the science,

was of opinion that it ought to be classed among the anhydrites, and assigned to it the specific name of fibrous anhydrite, it being principally distinguished from the other varieties of this fossil by its fibrous texture; which opinion was afterwards adopted by the celebrated Mohs, Karsten, and many other intelligent mineralogists.

Besides having received from him many other Austrian and Hungarian fossils, I am indebted for a specimen of this mineral to the kindness and politeness of the celebrated M. Schreibers, Director of the Imperial Cabinet of Natural History at Vienna, and one of the friends and correspondents of our own society. This specimen I have subjected to a chemical examination, no analysis having hitherto been made of the fibrous anhydrite.

My first experiments upon this mineral excited many doubts as to its identity with anhydrite, and led me to conjecture that its composition was entirely different from that usual to anhydrite, so much so that they ought by no means to be referred to one and the same species.

The principal respect in which our fossil differs from the anhydrite is, that applied to the tongue it produces a sense of a slight saltiness and bitterness, which can by no means be attributed to any admixture of rock salt, generally detected in anhydrite, because its solution, whether in water or nitric acid, is scarcely affected by the presence of nitrate of silver.

It is to be observed too, that anhydrite is much more easily acted on by water, and that it is dissolved with hardly any assistance from heat, depositing sulphate of lime, and producing a saline bitter solution, which, in addition to the sulphate of lime, exhibits also, after evaporation, many other crystals of some other sulphate prismatically shaped, and possessing the same flavour as the fossil itself. But the greatest difference between this and anhydrite is that, when exposed to the flame of spirits of wine, it immediately melts, and is very easily reduced to an opaque lump.

These circumstances have induced me to suspect that this mineral agrees with, or at least is very nearly related to, that which was many years since discovered at Villaruba, near Occana, in Spain, where it was also found in strata of rock salt, and named glauberite by Brongniart, (to whose experiments we are indebted for our knowledge of it), it being found to consist of anhydrous sulphate of lime and sulphate of soda.

And although I have myself never met with glauberite, nor have been able to judge of it, except from the description and chemical analysis of it given by Brongniart in the *Journal des Mines*,* I was nevertheless inclined to consider this opinion extremely probable, as well on account of the greater solubility of this by means of water and acids, and the saltiness and bitterness of its

* *Memoir sur une Nouvelle Espece de Mineral de la Classe des Sels nommée Glauberite, par Alex. Brongniart, vol. xxiii. p. 5—20.*

solution, as from the facility with which it is melted and reduced to an opaque mass, and likewise from the similarity of the specific gravity of both these minerals.

A more exact chemical analysis, however, has brought forward both facts and arguments that are in direct opposition to this hypothesis, and most satisfactorily prove that the fossil of which we are treating is completely different both from glauberite and anhydrite. For it is evident, as will appear from the experiments I am now about to give, that there was no sulphate of soda, but that it consisted of sulphate of lime both combined with water and in an anhydrous state, of anhydrous sulphate of magnesia and of sulphate of potash, in which latter respect, it differs from every fossil of this class.

This being the case, I had no doubt whatever as to this fossil constituting a new and unknown species of the class of salts, which I propose to distinguish by the term polyhalite, since it particularly differs from the rest of this class of fossils in the number of its constituent parts.

Mineralogical Description of Polyhalite.

Previously to giving a chemical analysis of this fossil, it will be proper to notice its physical qualities. As far as can be collected from the observations hitherto made, polyhalite is never found with its crystallization of a perfectly regular form, but is met with in shapeless masses generally of a compact or lamellated fibrous texture; yet its fibres, which are parallel and most commonly curved, are so united together that it is hardly possible to distinguish the separate courses of layers.

Its fracture is irregular and subfestucated (*subfestucaria*). The fragments are pointed, and generally edged. Rather indurated and brittle. It is somewhat harder than anhydrite, so as to scratch it slightly; while it scratches Iceland spar strongly. It is itself, however, easily scratched by fluor spar. In brittleness it resembles anhydrite, and may easily be pulverized.

In specific gravity it is nearly equal to anhydrite; for a fragment weighing 8.991 grm. in the air lost 2.586 grm. in alcohol, whose specific gravity was 0.7964, the mercury in the centigrade thermometer standing at 11.5°, and the barometer 0.748 m.; whence it follows that its specific gravity compared with distilled water is at the above-mentioned temperature and pressure = 2.7689.

Its colour is a brick-red, which doubtless is to be considered as not natural to this fossil, and to be attributed to the oxide of iron, thoroughly penetrating it, and only mechanically mixed.

The superficies of the fibrous variety has a pearly lustre, but the compact variety and the fracture of the fibrous possesses a waxen hue.

With respect to the pellucidity of this fossil, the lesser frag-

ments are perfectly transparent, the larger ones only at the edges; but if separated into very thin scales, and held up against the light, it is transparent, and nearly colourless.

When scratched, it exhibits a reddish white streak.

It has neither any smell of itself, nor emits any when either rubbed or breathed upon.

Fragments of it do not shine in the dark, when rubbed together, or scraped with the point of a knife, neither does the dust produce any phosphoric light when thrown on red-hot coals.

It is not possessed of any electric properties.

It is not at all affected by the magnet, neither does the latter attract particles of its finest dust.

It attracts a little moisture from the atmosphere.

Chemical Analysis of Polyhalite.

1. *Preliminary Analysis of Polyhalite.*—Having already made a few remarks respecting the physical characters of polyhalite, we now come to a chemical analysis of it, and to various introductory experiments made for the purpose of obtaining a knowledge of this mineral, and of the nature of its constituent parts.

(A.) *Examination of Polyhalite by Heat.*

a. A piece of polyhalite held in a platinum spoon over the flame of a spirit lamp soon loses its transparency, and gradually turning white assumes a reddish-white appearance. Its coherence likewise is diminished, and although its particles are rendered harder by calcination, they adhere less to each other. When the flame is increased so as to render the spoon red-hot, the fossil melts, and is converted into an opaque brown mass.

b. It undergoes a similar change whenever a piece is held in a pair of forceps in the flame of spirits of wine; except that it immediately becomes opaque at the first touch of the flame, and melts almost without becoming incandescent, changing to an opaque mass of a brownish colour.

c. The same is to be observed on applying a piece to the flame of a wax or tallow candle.

d. Exposed to the blow-pipe it melts instantaneously.

e. When mixed with borax and carbon, and exposed to the flame of the blow-pipe, it first of all effervesces very strongly like the sulphates, and is afterwards dissolved, exhibiting a clear mass slightly tinged with a yellowish brown. But if too much saturated, it becomes opaque and white.

f. In order to ascertain whether the changes which the fossil suffered during calcination as to colour, transparency, and coherence, were produced merely by the loss of the water of crystallization, or whether they are to be attributed to the dispersion of other of its component parts by heat, 10 grms. of polyhalite broken into fragments were put into a small glass retort, to which was

affixed a little receiver connected with a small mercurial apparatus.

On the application of heat, the polyhalite soon became white, and lost its former colour and transparency; at the same time little clouds appeared in the retort, which, being condensed like dew, distilled down, and passed into the bottle fixed to the retort; yet, excepting that from the air contained in the vessels being partly expelled by the fire, some elastic fluid escaped, no other substance was given off, although the heat was augmented to such a degree that the bottom of the retort began to be fused, and the fossil in many places was melted so as to form one mass with the glass. The liquid thus obtained from it afforded only a few drops, and when examined proved to be a pure distilled water.

Hence it appears that the changes which the fossil undergoes are to be attributed merely to the loss of the water of crystallization.

(B.) *Experiments on Polyhalite with Water.*

Water quickly affects polyhalite. On its being merely poured over it without heat, being at all employed, small fragments of the fossil are nearly taken up by this menstruum, as before observed; and on the application of a slight heat, the water dissolves more than two-thirds of it when in powder, leaving a tasteless powder of a whitish-red colour. When boiled in water, the fossil is almost completely dissolved, although not without a great deal of trouble, and employing a considerable quantity of the menstruum, nothing then remains, excepting a small portion of a brownish dust, which will not at all dissolve in water.

b. The solution of polyhalite obtained from cold water is limpid, and perfectly colourless. It is generally of a brackish flavour. It does not change the colour of turnsole, or violets, to red, neither does it restore a blue colour to paper stained with tincture of turnsole, or darken paper dyed yellow by the roots of turmeric. When concentrated by heat, the solution becomes turbid, and deposits pointed, tasteless crystals. Afterwards exposed to a spontaneous evaporation, it produces prismatic crystals, transparent, and possessing a brackish flavour similar to that of the solution itself: these exhibit partly four-sided prisms furnished with tetrahedral summits, partly hexagonal prisms, terminated at either extremity with a hexagonal pyramid; the former variety of these crystals cracks when exposed to the air.

The following are the observations I have made respecting the effects of alkalies, acids, and other agents upon this solution.

1. It instantly becomes turbid when mixed with a pure and perfectly caustic potash, and deposits a white precipitate, which is not dissolved by an addition of more potash, but is readily absorbed without any effervescence by nitric acid,

2. Similar effects are produced by caustic ammonia, yet the ammonia does not render the solution in the least turbid if it be previously acidulated by nitric acid.

3. Subcarbonate of potash produces a copious white precipitate, which afterwards subsides in the form of a white powder.

4. Similar effects attend subcarbonate of ammonia.

5. Mineral acids, when infused into this solution, produce no change.

6. Of the vegetable acids oxalic acid instantly causes a very copious precipitate. Tartaric acid likewise, if poured on in considerable quantity, renders the solution turbid, and produces a white crystalline powder, which on examination is found to possess all the properties of cream of tartar.

7. Oxalate of potash, when added, immediately forms a very plentiful precipitation of oxalate of lime; after the separation of which both pure and subcarbonate of potash, as well as caustic ammonia mixed with the remaining solution, produce a white precipitate. But carbonate of ammonia and neutral carbonate of potash produce no further turbidity.

8. It is instantly rendered turbid by muriate of platinum, and there is afterwards frequently deposited a yellowish sandy powder, which on examination is found to consist of muriate of platinum and potash.

9. If muriate of barytes be poured upon the solution, it immediately produces a copious separation of a white sediment incapable of being dissolved either by water or by acids.

10. Acetate of lead immediately causes a white precipitate not soluble in acetic acid.

11. Nitrate of silver produces only a slight milky appearance, and a little muriate of silver is deposited.

12. An infusion of galls neither changes the colour of the solution, nor occasions any other visible change.

13. Prussiate of potash and iron is attended with similar effects.

14. On adding hydrosulphuretted potash, the solution is not disturbed.

It is very evident from the preceding experiments that this solution of polyhalite, contains sulphate of lime, sulphate of magnesia, and also sulphate of potash, with a small portion of muriate of soda.

But in order to ascertain whether the fossil contained any sulphate of soda, I precipitated a portion of the solution by the acetate of barytes, and separating the sulphate of barytes, I evaporated it until completely dry. After which, I heated the mass thus dried, and the acetic acid being entirely expelled and decomposed, I extracted from the remaining powder, by means of boiling water, whatever was soluble; but this alkaline solution, when concentrated by heat, and rendered turbid by means of muriate of platinum, betrayed only some traces of muriate of soda,

which are doubtless to be attributed merely to the rock salt always inherent in the mineral. On being saturated with sulphuric acid, and afterwards subjected to a spontaneous evaporation, it was entirely converted into crystals of sulphate of potash.

c. That portion of the mineral which is not dissolved by cold water when treated with hot water produces a solution completely neutral, and perfectly colourless, like pure water. But this solution is without any taste, and when evaporated deposits only pointed crystals of sulphate of lime. The same effects are produced by reagents, and nothing but a mere solution of the salt is obtained; for it is neither precipitated by caustic alkalies, whether fixed or volatile, nor is it at all disturbed by tartaric acid, muriate of platinum, or by nitrate of silver; yet subcarbonated alkalies, oxalic acid, and oxalate of potash, likewise muriate of barytes and acetate of lead, produce copious precipitations.

d. The reddish-brown powder left by the water is scarcely acted upon by dilute nitric acid, without assistance from heat. If heated, it is then dissolved, and affords a reddish-brown solution. From this solution, caustic and carbonated alkalies precipitate a hydrate of oxide of iron. Upon an infusion of galls being mixed with it, it turns black; and if prussiate of potash and iron be dropped in, it deposits prussian blue; yet neither muriate of barytes nor nitrate of silver produces the least cloudiness; whence it is evident that this powder consists merely of red oxide of iron.

By attending to these experiments, we may be satisfied that polyhalite consists, as we have before observed, principally of sulphate of lime, and at the same time contains a small quantity of water with a little red oxide of iron, and muriate of soda. With respect to the two latter there is no doubt but that they are foreign to the original composition of this fossil, and only mechanically mixed with it. But the water doubtless forms gypsum with a portion of sulphate of lime, as is rendered probable from the efflorescence which the mineral undergoes during calcination.

2. *A more exact Chemical Analysis of Polyhalite.*—Having obtained the preceding information, I subsequently, in order to obtain a more complete knowledge of its nature and chemical properties, and to discover the proportion of its principal constituent parts, subjected it to the following more exact analysis:

(A.)

In the first place, I endeavoured to ascertain the quantity of water contained in polyhalite, the existence of which is proved by the diminution in weight which we perceive to take place during calcination. For this purpose I exposed to the fire in the glass tube of a barometer about 10 inches long, and closed at one end, by means of a lamp, a certain weight of the pure mineral divided into small pieces; then when the water was quite evaporated weighed the anhydrous residuum with great exact-

ness, and this experiment I repeated several times with equal care, in order to convince myself whether the proportion of water was uniformly similar, or at all varied.

That they may be more easily compared together, I have given the experiments and their results in the following table :

| Number of experiments. | Quantity of polyhalite calcined. | Weight of the residuum. | The loss of weight owing to calcination. | The quantity of water thus shown in 100 parts of polyhalite. | The average. |
|------------------------|----------------------------------|-------------------------|--|--|--------------|
| I | 3·149 grm. | 2·958 grm. | 0·191 grm. | 6·0654 grm. | } 6·13927 |
| II | 6·660 | 6·257 | 0·403 | 6·0515 | |
| III | 5·573 | 5·224 | 0·349 | 6·2623 | |
| IV | 5·651 | 5·303 | 0·348 | 6·1582 | |
| V | 5·000 | 4·695 | 0·305 | 6·1000 | |
| VI | 5·921 | 5·554 | 0·367 | 6·1982 | |
| VII* | 3·692 | 3·472 | 0·220 | 5·9588 | } 5·9335 |
| VIII* | 4·4515 | 4·1885 | 0·263 | 5·9081 | |

It is very obvious from these experiments that the quantity of water contained in polyhalite does not vary,† but is subjected to certain determinate proportions. Hence it is evident that the water is not fortuitous, but constitutes an essential part of its nature, and, as we may conclude from the appearance of the mineral during ignition, enters it along with a portion of sulphate of lime forming the gypsum.

But since 21 parts of water are contained in 100 of gypsum, it is sufficiently apparent from these experiments, reckoning the quantity of water which, with the sulphate of lime, produces the gypsum, according to the average of Experiments VII. and VIII. when, as we have observed, the mineral had contracted some moisture from the atmosphere, that in 100 parts of polyhalite there are 28·2548 of gypsum, or sulphate of lime united with water.

(B.)

a. 5·045 grm. of this fossil reduced to a very fine powder with twice that quantity of crystallized carbonate of soda dissolved in a proper quantity of water were boiled for half an hour; and, to prevent any of the powder from adhering to the sides of the vessel, were kept continually stirred during the whole operation. After which the liquor, while yet hot, was purified by filtration from any remaining powder, and the residuum well washed in boiling water.

* The moisture contracted from the atmosphere having been previously absorbed.

† The slight variations observable in my experiments were occasioned by the moisture which the mineral had contracted from the atmosphere.

b. The powder hence remaining, after being dried in a heat of the temperature of boiling water, was found to weigh 2.275 grm. First of all water, and afterwards nitric acid, being gradually poured on it, it was dissolved without any application of heat, creating a continual effervescence, and producing a solution limpid and colourless like water, nothing being left undissolved, except a very little red oxide of iron weighing only 0.014 grm.

c. In order to ascertain whether this nitric solution contained any portion of the fossil not yet decomposed, or whether any oxide of iron had been taken up by the nitric acid along with the lime and carbonate of magnesia, I evaporated it until perfectly dry. But during this operation, the solution deposited only some crystals of gypsum, scarcely amounting to 0.01 grm.; and when dried underwent no change of colour which might prove the presence of iron, but exhibited a completely white saline mass which was taken up by water without leaving any residuum. Upon carefully pouring some caustic ammonia with a few drops of nitric acid into the solution, the latter was hardly disturbed, and deposited only single red flakes of the hydrate of oxidated iron, the whole of which, when separated by filtration and well dried, weighed only 0.005 grm.; thus merely 0.004 grm. of red oxide of iron were indicated by the process.

d. The nitric solution being afterwards condensed by evaporation and put into a crucible of platina, was mixed with a sufficient quantity of sulphuric acid, and being continually stirred up during the whole time was very gradually dried by a moderate heat. The saline body, after being well exsiccated, was submitted to the fire until all the sulphuric acid was dispelled along with the nitric acid which had been left. Then the ignited mass, weighing 3.179 grm. being washed as usual, was separated into 2.241 grm. of anhydrous sulphate of lime, and 0.938 grm. of sulphate of magnesia equally freed from water.

e. The alkaline liquor obtained by experiment *a* was rendered rather thick in consequence of ebullition, and deposited a white powder, which, being separated by filtration,edulcorated, and well dried, weighed 0.05 grm. and in quality resembled carbonate of magnesia. According to Berzelius, these 0.05 grm. of carbonate of magnesia are equal to 0.064 grm. of ignited sulphate of magnesia.

f. From this liquor the sulphuric acid was afterwards precipitated by the assistance of muriate of barytes and the addition of muriatic acid sufficient to produce an acid reaction; from which were obtained 7.882 grm. of sulphate of barytes exsiccated in a temperature equalling that of boiling water, which were afterwards reduced by ignition to 7.650 grm.

But as sulphate of barytes contains in 100 parts 34 of sulphuric acid there are 2.601 grm. of this acid in 7.650 of sulphate of barytes; to which are to be added 0.0046 of sulphuric acid retained with the lime *b*; so that the entire quantity of sulphuric

acid obtained from 5.045 grm. of polyhalite is 2.6056 grm. If we subtract, therefore, the quantity of sulphuric acid mixed with the lime and magnesia weighing 1.3043 grm. + 0.6677 grm. = 1.972 grm. there remain 0.6336 grm. of this acid for potash, which, according to the experiments of Marcet, indicate 1.4118 grm. of sulphate of potash.

From this investigation, it appears that from an analysis of 5.045 grm. of polyhalite, there are obtained

| | Grammes. |
|--|----------|
| Anhydrous sulphate of lime (<i>d</i>) | 2.2410 |
| Anhydrous sulphate of magnesia (<i>d</i> and <i>e</i>) | 1.0020 |
| Sulphate of potash (<i>f</i>) | 1.4118 |
| Red oxide of iron (<i>b</i> and <i>c</i>) | 0.0180 |
| | 4.6728 |

Consequently 100 parts of polyhalite contain

| | |
|--|---------|
| Anhydrous sulphate of lime. | 44.4202 |
| Anhydrous sulphate of magnesia | 19.8612 |
| Sulphate of potash | 27.9841 |
| Red oxide of iron | 0.3568 |
| | 92.6223 |

(C.)

I afterwards submitted 5.573 grs. of this fossil to similar experiments, but so as first of all to separate the lime of the nitric solution from the magnesia by means of oxalate of potash, then to precipitate the magnesia with carbonate of soda, and afterwards unite each basis separately with sulphuric acid. By which method are obtained

| | Grammes. |
|--|----------|
| Anhydrous sulphate of lime | 2.5100 |
| Anhydrous sulphate of magnesia | 1.1390 |
| Sulphate of potash* | 1.5464 |
| Red oxide of iron | 0.0200 |
| | 5.2154 |

Consequently 100 parts of polyhalite contain

| | |
|---|---------|
| Anhydrous sulphate of lime. | 45.0386 |
| Anhydrous sulphate of magnesia. | 20.4378 |
| Sulphate of potash | 27.7480 |
| Red oxide of iron | 0.3588 |
| | 93.5832 |

* There were obtained to wit 8.568 grm. of sulphate of barytes, or 2.913 of sulphuric acid, of which 2.219 grm. of sulphuric acid are to be reckoned for lime and magnesia, and 0.694 grm. of sulphuric acid for potash.

(D.)

Lastly, in order that the real quantity of sulphuric acid contained in the fossil might be accurately ascertained from direct experiments, I made an analysis in the following manner :

a. 7·247 grms. of polyhalite being reduced into a very fine powder were boiled up with 200 grms. of pure water, which process was repeated with the same quantity of water until no more of the residuum would dissolve, or excite a brackish flavour.

b. The liquid obtained by the fossil, being thus boiled in water and afterwards precipitated by acetate of barytes, produced 6·638 grms. of sulphate of barytes, which, when ignited, were reduced to 6·539 grms.

c. After the separation of the sulphuric acid, I evaporated the acetic solution, exposed the concrete salt thus obtained to the fire, in order to destroy the acetic acid, and convert its bases into carbonates. After which I extracted all the carbonate of potash from the ignited mass by pouring warm water upon it, and saturated with sulphuric acid the alkaline liquor separated from the part undissolved by water. This being afterwards dried, and freed by ignition from the superfluous sulphuric acid, afforded 2·022 gm. of sulphate of potash, from which, during their solution in a very little water, 0·0285 gm. of ignited sulphate of lime were separated; so that the quantity of sulphate of potash amounted to only 1·9985 gm.

d. The powder left by the water *c* was dissolved in nitric acid, and as the solution contained some barytes produced by the decomposition of the acetate of barytes employed in too great quantity for the purpose of precipitating the sulphuric acid, it was mixed with a proper quantity of water, and sulphuric acid dropped into it, until the sulphate of barytes ceased to be precipitated. This having been removed by filtration, I evaporated the nitric solution to dryness, and adding more sulphuric acid, converted the lime and magnesia into sulphates by the usual method of evaporation and ignition; these, when afterwards separated by water, exhibited 0·580 gm. of anhydrous sulphate of lime, and 1·475 gm. of anhydrous sulphate of magnesia.

e. That portion of the mineral which was not soluble by boiling (*a*) weighing 2·695 grms. I boiled for half an hour with double that weight of carbonate of soda, in a proper quantity of water. I then strained the liquor while it was yet hot, and carefully washed the powder, left after filtration, in boiling water. This powder, when well dried, weighed 1·994 gm. By pouring upon it dilute nitric acid, it was almost entirely dissolved, with effervescence, and there remained only red oxide of iron, weighing not more than 0·020 gm.

f. The nitric solution of the powder on being mixed with caustic ammonia deposited 0·005 gm. more of hydrate of oxide of

iron, which are equal to 0.004 grm. of red oxide; therefore, the entire quantity of red oxide contained in the mineral which I analyzed amounted to 0.024 grm.

g. The solution, being afterwards heated until it boiled, and during this process precipitated by carbonate of soda, yielded 1.970 grm. of carbonate of lime; but 100 parts of carbonate of lime converted into sulphate by means of sulphuric acid yielded, according to our experiments, 134.69 parts of anhydrous sulphate of lime; therefore, the quantity of carbonate of lime arising from this solution is equal in weight to 2.6534 grms. of anhydrous sulphate of lime.

h. Lastly, when I perceived that the alkaline liquor was not at all deturbated by boiling, I precipitated it by muriate of barytes acidulated with muriatic acid. The sulphate of barytes thus deposited, being properly collected andedulcorated after ignition, was found to weigh 4.524 grms.

Therefore the entire quantity of sulphate of barytes obtained from this analysis of polyhalite was 6.497 grms. (b) + 4.524 grms. = 11.063 grms. which, according to the calculation before given, affords 3.7615 grms. of sulphuric acid. Therefore 7.247 grms. of polyhalite, when thus examined, presented

| | Grammes. |
|--|----------|
| Anhydrous sulphate of lime (c, d, and g).. | 3.2619 |
| Anhydrous sulphate of magnesia (d) | 1.4750 |
| Sulphate of potash | 1.9935* |
| Red oxide of iron (e and f) | 0.0240 |
| | 6.7544 |

Therefore, the proper calculations being made, 100 parts of polyhalite are found to contain

| | |
|-------------------------------------|---------|
| Anhydrous sulphate of lime. | 45.0103 |
| Anhydrous sulphate of magnesia. . . | 20.3532 |
| Sulphate of potash | 27.4817 |
| Red oxide of iron. | 0.3311 |
| | 93.1763 |

(E.)

This analysis being repeated with 5.921 grm. of polyhalite, there were found

| | Grammes. |
|---------------------------------------|----------|
| Anhydrous sulphate of lime. | 2.635 |
| Anhydrous sulphate of magnesia. . . . | 1.155 |
| Sulphate of potash | 1.634 |
| Red oxide of iron. | 0.018 |
| | 5.442 |

* From sulphuric acid computed 1.955 grm.

Therefore 100 parts of polyhalite consist of

| | |
|---|---------|
| Anhydrous sulphate of lime. | 44·5026 |
| Anhydrous sulphate of magnesia. | 19·5069 |
| Sulphate of potash. | 27·6010 |
| Red oxide of iron. | 0·3039 |
| | <hr/> |
| | 91·9144 |

(F.)

It now remained for me to determine the quantity of muriate of soda met with in polyhalite, having ascertained from the preceding experiments that this salt is always found mixed with the mineral, although only accidentally, and in such a small portion as frequently to be disregarded.

For this purpose, since the eye, however assisted, is not able to detect a particle of salt in the fossil, I dissolved in nitric acid 7·088 grms. of finely powdered polyhalite, and having poured a considerable quantity of water on the solution, kept dropping in some nitrate of silver until the precipitation of the muriate of silver had ceased.

I then set the deturbated liquor by for some days in a dark place, in order that the precipitate might subside, and afterwards carefully collected the muriate of silver, which, being well dried, weighed 0·033 gm.

But now 100 parts of muriate of soda, when precipitated by nitrate of silver, according to the experiments of Rose, afforded 243·5 parts of muriate of silver; whence 0·033 gm. of muriate of silver are equal to 0·01355 gm. of muriate of soda; so that 100 parts of polyhalite contain of muriate of soda or rock salt, 0·1910.

(G.)

But if we take the average of what we collect respecting the proportions of the various constituent parts of this mineral from each of these experiments and analyses, it follows that in 100 parts of polyhalite there are

| | |
|-------------------------------------|---------|
| Water. | 5·9335 |
| Anhydrous sulphate of lime. | 44·7429 |
| Anhydrous sulphate of magnesia .. | 20·0347 |
| Sulphate of potash | 27·7037 |
| Muriate of soda. | 0·1910 |
| Red oxide of iron | 0·3376 |
| | <hr/> |
| | 98·9434 |

On reckoning the portion of sulphate of lime as gypsum, or combined with water, and judging its quantity from that of the water found in the fossil, there are in 100 parts of polyhalite

| | |
|---|---------|
| Sulphate of lime combined with water .. | 28·2548 |
| Anhydrous sulphate of lime. | 22·4216 |
| Anhydrous sulphate of magnesia | 20·0347 |
| Sulphate of potash | 27·7037 |
| Muriate of soda | 0·1910 |
| Red oxide of iron. | 0·3376 |
| | 98·9434 |

From a comparison of the quantities of these sulphates contained in polyhalite, it is plain that they enter into the composition of this fossil in the proportion of their equivalent numbers.

This point is of considerable importance, since it clearly proves that they are not by any means mechanically or fortuitously mixed with each other, but chemically united so as to form a real combination. By which is proved beyond a doubt that polyhalite constitutes a peculiar mineral substance differing from all the other minerals of this class.

It is nevertheless uncertain what place in the mineral system should be assigned to polyhalite, especially as we are by no means acquainted with its real structure and conformation; and, therefore, cannot at present decide and determine upon which of its constituent salts, its physical constitution principally depends.

But as this fossil differs from the rest very much on account of the sulphate of potash which it contains, it appears to me that it would be better and most consistent to exhibit it in the interim as a peculiar species of the salts of potash.

ARTICLE VIII.

On the Theory of Franklin, according to which Electrical Phenomena are explained by a single Fluid. (A Memoir read at the Royal Institution of the Sciences at Amsterdam.) By Martin Van Marum, Knight of the Order of the Belgic Lion, Secretary of the Dutch Scientific Society, Director of the Teylerian Museum, &c. &c. 1819. (With a Plate). Translated from the French.

Is there any solid objection against the theory of Franklin which attributes electrical phenomena to a single fluid, or are they to be referred to two different fluids of an opposite nature?

When I caused the large electrical machine to be constructed for the Teylerian Museum in 1784 for the purpose of observing some phenomena on a larger scale by means of the electrical non-equilibrium, and thereby to procure the means necessary to

discover and investigate their nature, one of the first advantages that I derived from the undertaking was to observe that the spark of the first conductor, which had hitherto never been seen divided, shot several very visible and considerable branches, which were subdivided into several minor ones, and all of which directed themselves towards the opposite conductor to which they passed. This ramification of the sparks proceeding from the conductor of this large machine, was so much the more agreeable to me, as I believed that it furnished me with an incontestible argument in favour of the system for which I had long been searching for a decisive proof.

The system which I have in view is that of Franklin, according to which all electrical phenomena are explained by the non-equilibrium of a single fluid called the electric fluid. I had in several preceding memoirs published my opinion relative to this theory, and had demonstrated that all the phenomena of the interrupted equilibrium hitherto known were completely explained by this system, and that too in so easy and simple a manner that there was no occasion for having recourse to another theory formerly proposed by Du Fay, and afterwards more extended and combined by Symmer, according to which it is imagined that instead of a single electric fluid, there are two distinct fluids producing the two opposite electrical forces, viz. the positive and the negative; one of which, from its being excited by the friction of glass, was at the time of Du Fay, in the infancy of the science, called the vitreous electric matter, and the other was termed the resinous electric matter, from its being supposed to be principally produced by the friction of some resinous body.

In another memoir, on the Electrophorus, written by me in conjunction with M. Paets von Troostwijk, and presented by us in 1782 to the Batavian Society of Rotterdam, in reply to a question proposed by that Society relative to the then new and unexplained phenomena of this machine, we succeeded in explaining satisfactorily, both from the theory of Franklin and convincing experiments, all the phenomena of the electrophorus as produced by a single electric fluid: we, therefore, at that time thought ourselves justified in terminating this memoir * with the following conclusion; viz. that in consequence of the extremely satisfactory explanation which Franklin's system presents, respecting all the particular phenomena of the electrophorus, the theory of that philosopher was confirmed to such a degree that it might in future be admitted as a confirmed truth.

Yet it might, perhaps, be remarked with regard to the above inference that an hypothesis, however satisfactory, as far as concerns the explanation of all the phenomena for which it has been proposed, can nevertheless, for this very reason, not be

* Inserted in the *Recueil des Memoires de la Societ  Batave*, tom. vii.

considered as incontestably proved. Having, since the publication of that memoir in 1782, been employed in searching for some completely convincing proof of the unity of the electric fluid, I conceived that I had undoubtedly discovered it in the ramification of the sparks proceeding from the conductor of the large Teylerian machine, since the sparks were there so distinctly indicated. On looking at the sparks passing between the first conductor and the receiving conductor opposite, it might be continually seen that all the lateral branches of the sparks directed themselves towards the conductor opposite to the first conductor. One perceived them very plainly (what could never be observed so long as the common electrical machines had produced only such sparks as were undivided or not ramified) by the direction of the lateral sparks always turning themselves from the first conductor, and tending towards the opposite one, that the spark issued from the first and passed into the second conductor.

All those who were present at these experiments made with the large electrical machine, were struck with this manifest direction of the electric fluid from the first towards the opposite conductor; and each time that I have had the pleasure of finding at these experiments some philosophers conversant in electricity, and acquainted with the old theory of Du Fay and Symmer, according to which it was pretended that the electric fluid was produced not by the transition of a simple fluid from one conductor to the other, but by the concurrence of two different fluids meeting in contrary directions, I have had the satisfaction of hearing it generally averred that this visible direction of the fluid from one conductor to the other furnished the most evident proof against the theory of Du Fay and Symmer, and in favour of that of Franklin, according to which the electric phenomena are the effects of the transition of a single fluid.

I repeated at different times, for those who considered the importance of this phenomenon as a decisive proof in favour of Franklin's system, the experiment of the spark branching off to the other conductor, which is connected with the parts of the machine producing the friction, and named, conformably to Franklin's system, the negative conductor. Although the construction of the apparatus is such as to prevent the electricity of the negative conductor by any means equalling that of the positive one, nevertheless, when the weather was favourable, the spark formed ramifications which were constantly observed to take a direction opposite to that of the positive conductor; for instead of proceeding from the electrified conductor, as was the case with the positive conductor, one perceived, by the direction of the branches towards the conductor united to the cushions that the sparks proceeded from the next conductor, and passed to that which was in a state of electricity.

This phenomenon being so striking and interesting, I was



Fig. 1.

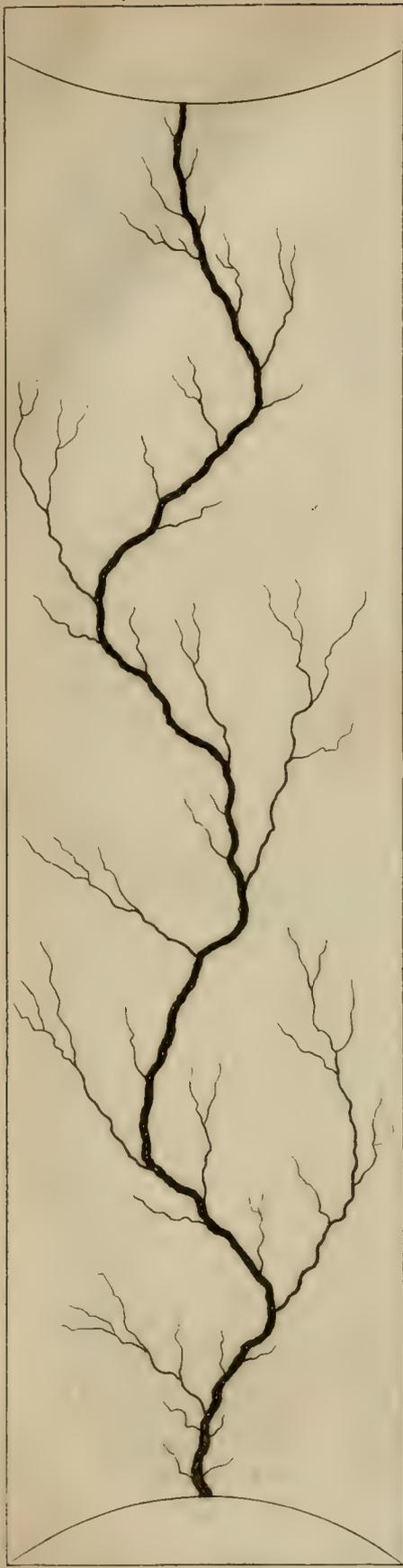


Fig. 2.

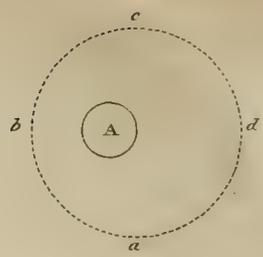


Fig. 3.

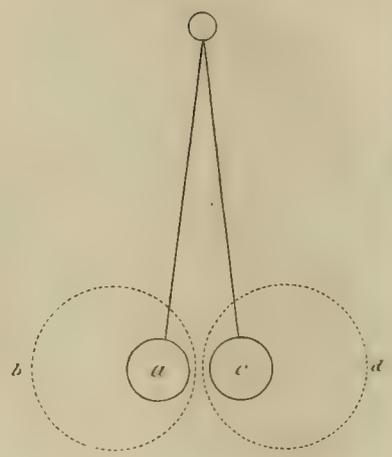
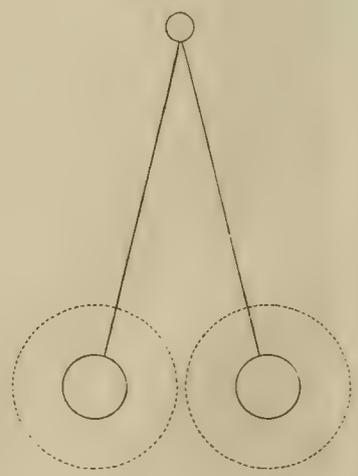


Fig. 4.



induced to give a plate of it representing the ray exactly as it was observed at the positive conductor. My colleague, M. Paets von Troostwijk, who, at that time, used frequently to favour me by attending at these experiments made with the large Teylerian machine, will be able to recollect that one evening, when the action of the machine was particularly strong, we endeavoured to make as correct a drawing as possible of this spark, and that we unanimously made choice of the sketch which we judged most faithful. Plate CX represents the spark one-third of its natural size.

Shortly after the appearance of the first volume of my electrical experiments made with the large Teylerian machine, I visited Paris in June, 1785, and distributed copies of the work to some members of the Royal Academy of Sciences, viz. to MM. Le Roy, Brissons, Lavoisier, Berthollet, Monge, and Van der Monde. After having seen the drawing of the spark with its ramifications, they all agreed with me that, considering the visible direction of the sparks produced by the large machine, it was incontestibly proved that, in conformity with Franklin's theory, a single electric fluid proceeds from the positive conductor, and passes into the next conductor, which receives it; while, on the contrary, when the machine is made to act negatively, the electric fluid proceeds in a contrary direction from the adjoining conductor, and passes into that which is negatively electrified.

The celebrated Franklin being at that time still at Passy, near Paris, but on the point of returning to America, many of the aforesaid members of the Academy eagerly encouraged me to present to that great genius, to whom this branch of physics was so greatly indebted, the first fruits of my electrical experiments with the large Teylerian machine. M. Le Roy accompanied me to Passy, where we were the last of those who were introduced to Franklin previous to his return to America.

On being first announced, we perceived that it was his intention, after having granted us a short interview, to excuse himself on account of his approaching departure. But perceiving that the experiments, of which I offered him the descriptions, were very important with regard to that branch of physics in which he himself had formerly made so many successful researches, this venerable old man, whose presence inspired me with a profound respect, made me sit down beside him, and begged me to communicate and point out to him whatever I judged most likely to throw a light upon this science. After attentively considering the figure of the spark, and the direction of its lateral branches, he inquired of me very seriously whether one might be satisfied that the drawing represented nothing but what had been actually observed? When I had given him assurances on this head that seemed to remove his doubts, he said, "This then proves my theory of a simple electric fluid, and it is now high time to reject the theory of two sorts of fluids."

After Volta, the celebrated Italian Professor, had conferred upon me the favour of passing a few days with me in 1782, and had since that time corresponded with me, I sent him in June, 1785, the first part of my electrical experiments as soon as the work made its appearance. The packet having been a long time on the road, I at length received an answer, dated March 8, 1786, in which he makes the following observation on the subject of the ramifying sparks of the large Teylerian machine :

“ Among the novel phenomena which you have obtained by means of such a great electric power, I am particularly struck by the ramifications thrown out by the spark, represented in Plate III. These branches clearly mark the direction of the electric fluid. As doubts had been raised with respect to the direction itself, which, it was said, might be supposed different; and as there has latterly appeared a sect who pretend to resuscitate the two opposite electric fluids which meet together, and catch fire from the shock, I had devised some experiments that might overturn these double electric torrents; among the rest was one which appeared to elucidate the matter completely; nevertheless it did not carry along with itself the evidence which I find in these ramifications, which, all making an acute angle towards the part where it is supposed the fluid is directed, show that the parts laterally escaped still retain somewhat of the movement common to the whole spark, and that consequently one is not mistaken in admitting but a single current of the electric fluid, and in assigning to it the direction supposed by Franklin’s theory. All orthodox electricians must be obliged to you for having thus overturned for ever the heresy of the dualists—the new partisans of the *Du Fays*, the *Nollets*, and the *Symmers*.”

Shortly after my treatise had appeared, many of the most intelligent philosophers, and the best acquainted with electricity, wrote at the same time to communicate to me similar observations. At this epoch, it appeared to be a generally received opinion, that after having so evidently seen in the ramifications of the electric spark its simple direction from the positive conductor towards the opposite one, or to that from the one opposite the negative conductor, it would not be possible ever to return again to the old theory of *Du Fay* and *Symmer*, who have fancied two electric fluids directed in opposite senses, and at the moment of their union forming the spark.

Nevertheless some later treatises on electricity have not answered the expectations of Franklin, of Volta, and of the generality of those philosophers, who are most conversant in whatever regards the science. People are again returned to the old opinion, of two different electric fluids, which, by their collision, cause the electric sparks and other phenomena. It was, as far as appears to me, the French philosophers who were the first to revive the old system.

Among these, M. Coulomb was the first who began to readmit the old hypothesis. He wrote two prolix memoirs respecting the manner in which the electric power distributes itself through several conductors, and on many subjects of an analogous nature there are to be found in the *Memoires de l'Academie Royale des Sciences*, 1786, 1789, and these inquiries appear to have led him to reasons for returning to the old system, in favour of which, however, he has neither alleged experiments, nor refuted the observations supporting the contrary system. Since this period many French philosophers have adopted the opinion of Coulomb, of which M. Libes gives the following account.* “Coulomb considers the electric fluid as composed of two particular fluids, which are neutralized by each other in the ordinary state of the bodies, and which separate themselves when the bodies are electrified. The first, produced by the friction of glass, is called vitreous fluid, or vitreous electricity. The other which is furnished by resin, silk, sulphur, wax, &c. is named resinous fluid, or resinous electricity. Thus vitreous electricity answers to Franklin's positive electricity, and the resinous to what he designated as negative electricity.”

In 1802, Abbé Haüy, who had then acquired much celebrity by his treatise on Mineralogy, published the preceding year, was charged by Buonaparte, at that time First Consul, to compose within a few months an elementary work on physics for the use of the masters in the Lyceums, for which a new plan of instruction was then forming. M. Haüy, being obliged to comply with this order in a very short space of time, although mineralogy rather than physics had for a long while been his favourite pursuit, admitted into his work the theory supported by Coulomb; that is to say, of two fluids, in order to explain electrical phenomena, without at all considering what has been alledged as evident proof in favour of the Franklinian system, or at least without making any reflections, much less any observations, tending to refute it. Since the publication of this work in 1802, and its being universally employed as a work of instruction in the Lyceums of France, the theory here laid down has been generally adopted in that country; that is to say, the theory of two entirely different fluids, one vitreous, the other resinous.

Having several times had occasion to converse with French philosophers, I am persuaded that the prejudice since that time so popular among the chemists and philosophers of that country, in favour of the *dualism*, or system of two electric fluids, ought, in a great measure, to be attributed either to their not recollecting, or to their not being acquainted with the observations which had been since made, and which tended to overthrow that system. I recollect among others the result of a conversation which I had on the subject with a French philosopher, known by his

* *Traité Elementaire de Physique*, Paris, 1801, tom. iii. p. 276.

work on electricity. On his return from Germany, where he had been in the army, he came to see me at my country house in 1810 on a festival, when it was not in my power to gratify his desire of seeing our large Teylerian electrical machine. He then eagerly entered into an ample detail concerning his theory of electrical or electrochemical phenomena, which was entirely founded on the old system of vitreous and resinous electricity, according to which he endeavoured to explain all electrical phenomena, at the same time availing himself of chemical affinities, which he made to act just according to his own imagination.

The prejudice of this learned man for his theory, which, he informed me, he was about to publish shortly, prevented me from then opposing his arguments. I, therefore, contented myself with inquiring whether he had read an account of the experiments which I had made with the Teylerian apparatus. He replied that he had once seen my publication, and that the journals had communicated to him my experiments.

Meeting him the following day at the Teylerian Museum, I showed him the drawing of the electrical spark of the large machine, and perceived that he either had not seen it before, or had forgotten it. He began by putting questions which plainly proved to me that he was doubtful whether all the branches were turned on the same side as they were represented in the figure. The temperature of the air happening to be then very favourable for electrical effects, I replied by offering to convince him of the truth of this direction of all the lateral branches of an electrical spark to the same side. I then conducted him to the new machine, invented and described by me in 1791, consisting of a disk 31 inches in diameter, and placed in a room in the Teylerian Museum, where it is always in readiness for use.

By means of this instrument I showed him that the rays proceeding from a button placed in the lower part of the positive conductor produce, in favourable weather, lateral sparks, which are all directed towards the globe conductor which receives the spark. I also showed him that when the same conductor which has emitted the electric sparks, having been positively electrified in its ordinary position, is made to touch the cushions (which is accomplished by the movement of its semicircle), and, when in this situation, deprived of its electric fluid, the same button, which, in the first instance, emitted ramified sparks, then produces no more sparks; but that, on the contrary, if a similar button of copper be brought within about six inches of the large globe of this conductor, one then perceives proceeding from the button sparks of a similar length, which, as well as those of the positive conductor, produce lateral ramifications, and these sparks differ singularly from those of the positive conductor, inasmuch as they have an opposite direction.

After the French philosopher had thus evidently seen by repeated experiments that all the lateral ramifications of the

electric sparks are, in every instance, directed towards the same side; and he could no longer doubt of the accuracy of the drawing which I had shown him of the ray produced by the Teylerian machine; after too he had seen, by means of this apparatus, that the ramifications proceeding from the positive conductor take a direction towards the opposite one, and evidently indicate that the spark is emitted by the opposite conductor; but, on the contrary, that similar ramifications of the spark at the negative conductor are directed towards the other, and thus indicate the transition of this spark from the opposite conductor towards the negative conductor, I asked him if the then so evident direction of the sparks did not satisfactorily prove to him that, in either instance, they ought to be attributed to the current of a *single* fluid, which, whenever there is a superabundance of electric fluid in the conductor, is emitted from it in order to pass to the neighbouring one, and that this current, on the contrary, when the conductor is deprived of the electric fluid, transmits itself from the conductor adjoining the negative one itself? To this question I added another; namely, whether, on the contrary, these different directions, so clearly indicated by the sparks, could be made to agree with his conjecture, that the electric spark is occasioned by two different fluids approaching each other in opposite directions, and uniting in the spark itself.

To these questions, he replied that what he had just seen might very well accord with his theory, adding, at the same time, that he was not able at once to give me his ideas on the subject, but judged it more proper to correspond with me upon it in writing; and that immediately on his return to Paris, he would inform me how what he had seen might be explained conformably to his theory. Yet although this occurred in August, 1810, I have never yet received the promised answer. Thus I am still unacquainted with his theory of electrical phenomena, a work, which, as he informed me, it was his intention to publish shortly after his return to Paris.

They still continue in France to adhere to the theory of two sorts of electric fluid, as I have at several different times had occasion to observe, among others by Thenard's *Traité Élémentaire de Chimie*. Paris, 1813. In this work, M. Thenard, speaking of Volta's electric column as a chemical instrument (tom. i. p. 91) explains the apparatus and its phenomena on the theory of vitreous and resinous electricity, although Volta himself derives the explanation of his invention entirely from the more simple and better supported theory of Franklin.

But, it may be asked, have either the French Academicians, or any other learned men, alleged more recently new reasons or experiments which have induced them to return to the old opinion, although that of Franklin was still received by them.

It is my intention to examine maturely with you whatever has

been brought forward in favour of the doctrine, and then to submit to your judgment whether the observations adduced against Franklin's system are so well founded, and of such a nature, that, notwithstanding what I have now said respecting it, greater probability is to be attached to the contrary system; and in this I have attempted to follow the course by which the old opinion has been endeavoured to be re-established in France.

In 1787, Haüy published the theory of Franklin as it had been developed by M. Cēpinus, Member of the Academy of St. Petersburg, in his *Tentamen Theoriæ Electricitatis et Magnetismi*. Peters. 1759; and this he drew up in such a manner as by excluding as much as possible all mathematical calculations and propositions, to render it intelligible to a great number of readers: this he entitled "*Exposition Raisonnée de la Theorie de l'Electricité et du Magnetisme d'après les Principes de M. Cēpinus*. In the preliminary discourse of this work, he observed, respecting Cēpinus's theory, "it must be allowed that among the principles upon which the theory of M. Cēpinus is founded, there is one so greatly at variance with sound physical principles that the author himself long hesitated whether he should admit it, nor was it until after mature examination that he determined upon doing so. The principle in question is that the particular atoms of bodies possess a mutual repulsive power, as do likewise the atoms of the electric or magnetic fluid. M. Cēpinus proves that the existence of this force is a necessary consequence of that of two forces, upon which is founded the theory of Franklin. In fact, it is not more inconsistent to admit of a repulsive force between the atoms of bodies than between those of fluids, whether it be electric or magnetic, because the term force expresses merely a fact of which we do not inquire into the cause. The more especial point of objection against similar forces is, that they cannot be reconciled with the principle of universal gravitation. But as the forces in question do not exert themselves, except in the particular instance of electric or magnetic phenomena; and since this action is, as it were, suspended by opposing forces when the bodies meet in their natural state, as will be shown in the course of this work, gravitation is not thereby rendered less a general force capable of proving only local and transient shocks occasioned by the phenomena of electricity and magnetism." Yet although in the preceding quotation M. Haüy has endeavoured, by the arguments he brings forward, to remove the obvious difficulty which arises, if we admit the principle of a repulsive force existing between the atoms of bodies, it appears that he subsequently considered his own reasonings as insufficient; for in his *Traité Elementaire de Physique*, Paris, 1806, tom. i. p. 398, we meet with the following passage: Cēpinus was led by his theory to the singular inference that taking into view electric phenomena, the atoms of bodies repel each other. In perusing his work one may

see that he rejects this inference with a certain indignation when it first presents itself, and that he is afterwards obliged to reconcile himself to it. It was in fact hard to avow that it depended merely upon the presence of the electric fluid whether the atoms of all solid bodies appeared to exercise upon each other an action directly contrary to universal gravitation. It was giving to the theory a very powerful and formidable opposition; and in order to avoid this inconvenience, the electric fluid was conceived to be formed by the union of two fluids, one of which performs the function attributed by *Æpinus* to the atoms of bodies.

In other French writers one also meets with similar reflections against the theory of a simple electric fluid, as proposed by *M. Æpinus*. "It is to be regretted (says *M. Libes*, *Traité Elementaire de Physique*, Paris, 1801, tom. iii. p. 276) that the way in which *M. Æpinus* has considered these two forces should have led him to an inference contrary to the laws of gravitation, and consequently inadmissible. We shall find (continues he) that the theory of *Coulomb* (of two electric fluids) obviates this disadvantage."

I have no other remark to make here, except that if these French philosophers had either known or duly considered the observations published* by our respectable colleague, *M. Von Swinden*, against the system of *Æpinus*, they would not have attached so much importance to the extension which *Æpinus* has given to *Franklin's* theory for the purpose of rejecting it, and this merely because this extension of *Æpinus*, and his analytical demonstrations, which are not sufficiently well grounded, have induced him to admit an inference incompatible with the law of universal gravitation.

This extension given by *Æpinus* to the theory of *Franklin*, which has led him to the aforesaid conclusion, has, as it appears, been judged indispensably necessary in order to explain electrical phenomena, or else it has been thought that several of these phenomena which might be explained by this untenable supposition could not be explained without it. What appeared particularly inexplicable at the time of *Æpinus* was the repulsion or separation of two negatively electrified bodies which could not be accounted for without admitting a repulsive force existing in the very matter of the bodies themselves. It seems that the French philosophers are still of the same opinion.

M. Haiiy says, "the repulsion of bodies considered to be negatively electrified has always been the stumbling block of all theories. It was necessary to attempt to explain how these bodies, each of which had lost a portion of its fluid, were determined to detach themselves from each other, while a superabundance of fluid produced precisely the same effect."

* *Recueil des Memoires sur l'Analogie de l'Electricité et du Magnétisme couronnés par l'Academie de Bavière*, la Haye, 1784, tom. ii. p. 217.

But it would never have been found so difficult to account for the repulsion of two bodies negatively electrified if attention had been given to what has been incontestably demonstrated by more recent observations; and has been adopted by many philosophers as a sound truth, and one well confirmed by experience, viz. *that no electric force can arise from the surface of a body unless this latter be in contact with bodies, either solid or fluid, which assume a contrary force, or be surrounded by such bodies.*

1. It necessarily follows from this universal law, already demonstrated by Baptiste Beccaria in the Phil. Trans. for the year 1770, vol. ix. p. 277, that in the air surrounding a body to which an electric force is communicated, there arises, for a certain distance, a contrary force. A body positively electrified and placed in the air is, therefore, conformably to this law, surrounded with an atmosphere of a contrary electric power; and a negatively electrified body is, on the other hand, surrounded by a positively electrified atmosphere.

2. It is evident that every electrified body must necessarily be placed in the midst of this atmosphere of an opposite electric force, the contrary being impossible for the following reasons. Bodies possessing opposite electric forces reciprocally attract each other; consequently there exists an attraction between an electrified body and its atmosphere; namely, the electrified air surrounding it, and possessing an opposite force. It follows hence that the attractive forces which the electrified air exercises on the bodies it surrounds must be equal on every side. Suppose, for example, that the body A, fig. 2, placed in its electrified atmosphere *a, b, c, d*, be retained by some cause towards the side *d*, so that the breadth of the atmosphere on side *b* double that on side *d*; it is then evident that the attractive force which the air exercises on side *b* upon the body *a* will be much greater than that on side *d*. This body, therefore, being drawn by unequal forces, and in opposite directions, must, as soon as it is in its power to yield to the attractive forces surrounding it, obey the stronger attraction, and remove itself until it be drawn on every side by attractions of equal power. Now this can happen only when it is in the centre of its electrified atmosphere.

3. When, therefore, an electric force is communicated to two light bodies, placed opposite to each other, and endued with great mobility, these two bodies will recede from each other until each of them comes within the centre of the atmospheres in which they have produced opposite forces. Suppose that the bodies *a, c*, be electrified, each will be then surrounded by a sphere of electrified air of contrary power. The moveable bodies *a*, and *c*, will then not be able to retain themselves within these atmospheres, one beside the other, as in figure 3, since in this case they will be attracted by unequal forces towards *b* and *d*; of course they must then separate until each of them

has reached the centre of its atmosphere, as is shown in figure 4.

It is clear that this explanation of two electrified bodies is as applicable to bodies deprived of the electric fluid, or negatively electrified, as to those in which there is an excess of electric fluid, or which are positively electrified.

The discoveries subsequent to the time of *Cepinus* having furnished a satisfactory explanation of the repulsion of negatively electrified bodies, there is no reason to suppose with *Hauy* that negative repulsion is the stumbling block of *Franklin's* theory, since *Cepinus* had given it an extension not sufficient for the explanation of this phenomenon. The repulsion of negatively electrified bodies being thus easily explained by *Franklin's* theory, which has been demonstrated by *Beccaria*, according to the aforesaid law, we may safely venture to set aside the extension which *Cepinus* has given it, and consequently cannot deduce from it any argument against the theory of *Franklin*.

Other phenomena have been observed during a discharge from a glass jar, which might lead superficial observers to suppose that the current of electric fluid which takes place whenever electrified bodies are discharged is produced by two fluids which unite from opposite sides. When, for example, one discharges a bottle so slightly charged with fluid that only a little spark is perceived, and the inner side of the bottle is touched with a finger of one hand, and its outside with a finger of the other hand, only a slight sensation is felt in the ends of the fingers. A little stronger charge occasions a sensation all along the fingers. On repeating this experiment with charges gradually stronger, one feels more and more the passage of the fluid in the hands and arms.

Let the discharge be passed through a long series of metallic globes, or other conductors, placed at very short but regular intervals from each other (which may be determined by placing a cord or other very thin substance between each, and afterwards removing it), then on a faint discharge being made, one may see the electric fluid pass only between the bodies nearest to the two sides of the jar. Should the charge be increased but ever so little, one sees the sparks pass between the bodies more remote from the jar; and the more the charge is increased, the easier is it to discern the passage of the sparks between the bodies still more remote from the latter.

It is from these and similar phenomena that it has been attempted to prove that during the discharge the electric fluid proceeds from two opposite sides; yet this cannot be inferred from the above-noticed phenomena, as these explain themselves very clearly, if, with regard to electrical effects, the least attention be paid to the three following well-confirmed observations:

1. That the denser the electric current be which takes place

when a discharge is made, the more powerful will the electrical effects prove.

2. This current does not display itself in form of sparks unless there be a certain degree of density.

3. The two contrary forces of a charged jar are very nearly equal.

As far as regards electricity, every philosopher will certainly confess that the above phenomena are easily to be accounted for on these principles; indeed they are too evident to require any explanation here. Cavallo has already demonstrated them in the third part of his treatise on Electricity, p. 200.

I am aware of no other phenomena or conclusions that have been drawn from them, and alleged, in order to combat Franklin's system, or confirm that of Symmer, at least I know of none on the subject that appear to me of any importance.

In order to explain the chemical decompositions occasioned by the electric current of Volta's battery, there have, it is true, been imagined certain connexions supposed to take place between what is termed the vitreous electric fluid together with a constituent part of the body which is decomposed, and the resinous fluid and its constituent part.

To chemists little acquainted with electricity, such a supposition made for the purpose of explaining these decompositions will appear to possess some probability, seeing that in the generality of decompositions new combinations take place; yet when we consider whether there are solid reasons alleged in favour of these pretended combinations of the electric fluid with some other substance, we shall perceive that they are assumed without any grounds.

It appears, therefore, from what I have alleged that there is no occasion for returning to the old system of Du Fay and Symmer, viz. that electrical phenomena are produced by two different electric fluids. Since the publication of the first part of my experiments made in 1785 with the large Teylerian machine, I have uniformly, along with Franklin, Volta, and many other philosophers of the first rank, held the conviction that the dualism of electricity was refuted too evidently by the direction of the ramification of the spark (as shown in the Plate) for this hypothesis to be ever rationally defended until it shall have been demonstrated that this ramification is by no means an evident proof in favour of the Franklinian theory of a simple electric fluid passing from one conductor to the other: that these ramifications, although all directed towards one side, are, on the contrary, equally explicable by the hypothesis of two antagonist fluids, proceeding from the two sides of the spark, and there uniting; but I defy the most zealous defenders of dualism to discover, in favour of this phenomenon, a supplementary hypothesis, at all plausible; at least all the partisans of this system to whom

I have pointed out these ramifications, and whom I have frequently called upon to explain how this direction of all the branches of a ray towards one side can happen, supposing, according to their hypothesis, the ray was produced by two antagonist fluids proceeding from opposite sides, have never hitherto attempted a reply.

The question proposed by the first class of the Institute of the Sciences at Amsterdam, at its last public sitting, on Oct. 8, was as follows :

Are there any experiments directly proving the system now maintained by a great number of philosophers, viz. that electrical phenomena are occasioned by two fluids? and, in that case, what are they? Or are there, on the contrary, decisive experiments in favour of Franklin's theory, which was generally adopted, until the commencement of the present century, by the first philosophers, viz. that electric phenomena are produced by a single fluid? It is desired that in the examination of different experiments particular attention may be paid to what has been seen with the grand Teylerian apparatus at Haerlem; that is, that the electric spark, carried to a certain degree of force, appears to furnish, by its ramifications, certain indications that it keeps one single direction throughout its whole course, whether the conductor be electrified positively or negatively;* and it is requested that it be examined whether this experiment furnishes a complete proof, that these electrical phenomena are produced by the current of a single fluid, or if this direction of the ray can be clearly explained by the theory of the electrical phenomena not being produced by a single fluid, but by the junction of two distinct fluids, of a different nature, and acting oppositely; and if there be no decisive experiments directly proving the system of two fluids, it is asked why conclusions deduced from formulæ founded on this system should appear to accord with experience, and where it is that the error lies.

ARTICLE IX.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

Nov. 9.—The Society resumed its meetings, when a letter from M. Ampère was read, on the subject of the Magnetic Effects produced by the Voltaic Electricity.

There was also a paper, by Sir E. Home, read, entitled "On the Black Rete Mucosum of the Negro, being a Defence against

* See M. Van Marum's work, entitled, "Experiences faites par la grande Machine Electrique de Teyler." Harlem, 1785.

the Scorching Effects of the Sun's Rays." The author began by stating some observations which had induced him to form the opinion that the scorching effects of the sun's rays are produced not merely in virtue of their heating power, but by the joint agency of their heat and light. To verify this opinion, he made several experiments which showed that the face and hands may be exposed to a temperature of 100°, or even 120°, without pain being produced, provided the light be excluded; but that if the same, or even an inferior degree of heat be produced by the direct rays of the sun, the parts are scorched, and blisters are produced. This effect he found to be completely prevented by covering the hand or face with black kerseymere, and the same purpose is attained by the black rete mucosum of the negro. In those cases when a black covering was superimposed, perspiration came on, and the same takes place on the skin of the negro when exposed to the direct rays of the sun.

The author observed also that the eyes of those animals exposed to the strong light of the sun are furnished with a black pigment apparently for the same purpose; while others, which are abroad by night, and consequently not needing such a protection, are not provided with it.

Nov. 16.—A paper, by Sir H. Davy, was read, entitled, "On the Magnetic Effects produced by Electricity."

On repeating Oersted's experiments, the author stated that with a voltaic battery of 100 four-inch plates, the south pole of a magnetic needle placed under the communicating wire of platinum, the positive end of the apparatus being on the right, was strongly attracted by the wire, which was shown to be itself magnetic by its power of attracting steel filings. The wire also was found to communicate permanent magnetism to steel bars attached to it transversely, while such bars placed parallel to the wire were only magnetic during their connexion with the apparatus.

Actual contact, however, of the steel wires with the platinum or other metal forming the conductor was found not to be necessary; for magnetism was imparted to a needle placed transversely to the connecting metal, but at some distance from it.

Sir H. next related some experiments, showing that the magnetic power is proportionate to the quantity of electricity passing through a given space without any relation to the transmitting metal, and that the finer the wires the stronger the magnetic effect.

The author found that an analogous effect was produced by the discharge of a Leyden phial through a wire; and by passing the discharge of a Leyden battery of 17 square feet through a silver wire with a steel bar transversely attached to it, of two inches in length, the latter became powerfully and permanently magnetic. The same effect was produced at a distance of five inches through air, water, and even through thick plates of glass.

When several wires parallel to each other formed part of the same circuit, each became similarly magnetic to the single wire, and the opposite ends of each wire were in different magnetic states, and consequently attracted each other. When two voltaic batteries were placed parallel to each other, the positive end of the one being opposite to the negative end of the other, and transmitting their electricity through two wires, such wires repelled each other, because the opposite ends were in similar electrical states.

The Copley medal has been voted to Professor Oersted for his important discovery of the connexion between electricity and magnetism.

LINNEAN SOCIETY.

Nov. 7.—The Society resumed its meetings, when there was read a paper on the Goblet Sponge, the *spongea patera*, by Gen. Hardwicke.

There was also read part of a paper, on the Cymothoadæ, a natural family of the crustacea, by Dr. Leach.

Nov. 21.—A paper was read containing some observations on the Economy of the Procellaria Pelagica, by R. Scarth, Esq.

ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

The Anniversary Meeting of this Society was held^d in the Museum, Penzance, on Tuesday, Oct. 3, and was, as usual, numerously attended by most of the gentlemen in the western part of Cornwall. Owing to the inconvenience of frequent sittings to members residing at great distances from each other, and from the apartments of the Society, the monthly and quarterly meetings of this Society have, in a great measure, given way to the general annual meeting in autumn, at which time all the principal memoirs have been presented to the Society.

On the present occasion, many valuable papers were read. The following is a brief notice of some of the more interesting of these: 1. The Secretary, Dr. Forbes, read a memoir on the Temperature of Mines, a subject which of late years has attracted much attention, but which had received little or no notice in Cornwall previously to the institution of this Society. In this paper, the author, in the first place, detailed the result of thermometrical observations, made by himself and others, in numerous mines, as well in Cornwall as in other countries; from all of which it resulted that the temperature of the air, water, and earth, in mines, as shown by the thermometer, progressively but irregularly increased from a few hundred feet beneath the surface to the greatest depths yet attained by the miner: the maximum temperature in the deepest mines of Cornwall (1300 to 1400 feet), being about 80 degrees of Fahrenheit, or 28 degrees above the mean of the climate. As the existence of so great a temperature as this so near the surface, and still more, the seemingly progressive and most rapid increase of it in descending, are

at first sight circumstances very startling to our preconceived notions, and still more so when traced to the conclusions to which they necessarily lead; the author of the memoir, previously to coming to any opinion as to the site or source of this high temperature, discussed the many hypothetical objections that can be advanced against the existence of an internal source of heat in the body of the earth. We have not space to notice all these, nor to notice any of them fully. The following, among others, were particularly adduced and insisted on: 1. The fact that the degree of elevation above the sea does not affect the temperature of mines; mountain mines, at equal depths below the surface being as warm as those at the sea level. 2. The difference of temperature in mines of the same depth under the surface. 3. If so high a temperature existed at so comparatively small depths, ought not the law of the equilibrium of caloric to render this perceptible at the *very surface* of the earth? Ought not the temperature of our deep wells and copious springs to be the mean of *this internal temperature, and the external or atmospheric temperature conjoined*, and not of the latter only, as is the fact? 4. Besides noticing the fact of the very low temperature of deep seas and lakes, as bearing on the same point, Dr. Forbes brought proofs that the temperature of several abandoned mines filled with water for years, to the depth, at least, of several hundred feet, is not greater than the mean temperature of Cornwall. These and many other considerations naturally led the author to inquire into the various possible sources of extraneous temperature that are found in mines, and to the examination of how far these will go in accounting for their high temperature; an inquiry, moreover, rendered more natural and necessary by the fact, fully proved by the author of the memoir, of the presence or absence of miners occasioning a difference often of 6, 8, or 10 degrees of temperature, in the same mine, or in different mines, similarly circumstanced in other respects. The various sources of extraneous temperature, noticed by Dr. Forbes, were: 1. Candles; 2. Gunpowder; 3. Friction and percussion; 4. The bodies of the miners; 5. The diminished capacity of air for caloric in deep mines, in consequence of the condensation caused by the increased height of the atmospheric column. In estimating the effect of the four first sources, the author entered into minute calculations, founded on the experiments of various philosophers, and illustrated the whole by application to the case of a single mine. The mine chosen for this purpose was the magnificent copper mine of Dolcoath, which employs (underground) 750 persons, consumes monthly 3000 lbs. of gunpowder, and 5000 lbs. of candles; is 1400 feet deep, and contains within it upwards of 7,000,000 of cubic feet of excavated space.

By Dr. F.'s calculations, it appeared probable that a quantity of *air* might be heated daily in Dolcoath by the various extraneous causes mentioned, from the temperature of 52° to 60° .

(which was considered the mean temperature of all the air contained in the mine) sufficient to fill it thrice, or about 21,000,000 of cubic feet. Applied to *water*, the same quantity of caloric will raise from the temperature of 52° to 77° (the mean temperature of the water in the mine) only 2,300 cubic feet per day; but the pumps of Dolcoath bring up daily upwards of 120,000 cubic feet of water of this temperature! From this, therefore, it is evident that the extraneous sources of caloric in mines, although very important and more considerable than has usually been allowed, entirely fail in accounting for the temperature found in them. An additional and hitherto unnoticed source of increased temperature in mines is that arising from the elongation of the atmospheric column, and consequent condensation of the air—a cause constantly operating in every mine where there is a circulation of the contained atmospheric fluid, as is indeed the case in all mines. But this, even in the deepest mines in Cornwall, will only cause an increase of four degrees; which, even when added to the other adventitious causes, entirely fails to meet the degree of the actual temperature. In the mines of Cornwall no decomposition of pyrites, or other mineral matter, seems to take place in any degree sufficient to cause any perceptible augmentation of caloric. Whence, then, we may ask with Dr. Forbes, is derived the high temperature of mines? Notwithstanding the strong arguments that can be adduced against it, must we admit the existence of a constant and natural temperature of from 70° to 80° in the body of the earth at the depth of little more than a thousand feet? Or are there other adventitious causes, not yet suspected, that can explain this very striking and singular phenomenon? Dr. Forbes considered the mean temperature of the whole atmosphere at the surface of the earth to be about 66° of Fahrenheit, and stated that *this* is the temperature which he would have expected, *a priori*, to be found on the earth at very great depths, that is, on the supposition that there is no internal source of heat. He concluded by promising some communication on the health of miners as affected by the *tropical* temperature of their subterranean climate.

2. Another paper on the same subject by Mr. R. W. Fox, of Falmouth, was also read, being the second on this interesting topic presented by this gentleman to the Society. Mr. Fox's paper was chiefly occupied in detailing observations on the temperature made in upwards of ten mines, and exhibited the results in tables. From these Mr. Fox drew the conclusion that the temperature of the earth in Cornwall progressively increases as we descend, nearly in the ratio of one degree of Fahrenheit for 60 or 70 feet. Mr. Fox has an idea that the ascent of vapour through the lodes, and its condensation in the mine, may be an important agent in the production of heat in these recesses. A very singular fact was detailed in this paper. An accident hav-

ing happened to a steam-engine in the United-Mines Mine, the water increased in the bottom of the mine at the depth of 200 fathoms, so as to fill the two lowest galleries, and continued two days. Immediately after this water had been pumped out, and before the men returned to work, the temperature of these galleries was $87\frac{1}{2}^{\circ}$ and 88° , and this rather diminished than increased for some days after the miners returned to their labour there. Will this fact afford any additional clue to the explanation of the temperature of mines?

3. Two papers, by Mr. John Hawkins, were then read: One was on the Alternation of primitive Strata in Cornwall; the other, on the Intersection of Lodes, and the Inferences to be deduced therefrom. Both these are unsusceptible of abridgment.

4. Two papers were read, by Mr. Joseph Carne, one on some singular Lead Veins lately discovered in Cornwall; the other, on Cornish Petroleum. The former paper referred principally to the very productive mine of Sir Christopher Hawkins in the parish of Newlyn, which is said to yield more than 1000*l.* monthly in silver alone. Naptha nor maltha has never been found in Cornwall. Petroleum has been found in the copper mine of Wheal Unity, at a considerable depth, contained in small cavities in quartz.

Short notices were also read, by Mr. Carne, on the Mode of Blasting Rocks, and on the Account of Tin and Copper produced in Great Britain and Ireland during last year.

5. A paper, by Mr. Boase, Treasurer of the Society, on the Cultivation of Geological Science in Cornwall, pointed out the difficulties thrown in the way of the cultivation of geology in the commencement of the study, and explaining away all the objections that have been made to its cultivation by well-designing but ignorant persons, the author proceeded to detail the vast importance of the science *generally*, and *especially to Cornwall*, which, while it can derive more benefit from the study than any other country, offers greater facilities to the student than any other spot of equal extent on the surface of the globe. The writer proceeded to demonstrate how very nearly the inhabitants of Cornwall, and especially the proprietors of land, are interested in the prosecution of the study of geology; and, on these grounds, he *demand*ed of them, as their own peculiar affair, to continue to this Society, and every other having similar objects, that patronage and protection which they have so liberally extended hitherto; and he appealed to those Cornishmen who were distinguished for their learning and science (and there are many such) to consecrate *some portion* of their immortal labours to the welfare and renown of their native county. The discourse thus concluded: "Stimulated by motives so powerful as the fair fame of our country, the diffusion of useful knowledge, and the increasing prosperity of the community at large, much may be expected, and we should not forget that much is

expected, from the matured labours of this Institution. Let it not then be forgotten that success is the prize of exertion—not of the few, but by a general concurrence in the spirit of our comprehensive Cornish motto “One and All.” Then will the memorial of these our days go down to posterity, adorned with the imperishable trophies of science. But if we supinely neglect the auspicious opportunity, instead of living in the grateful recollection of future times, posterity will mourn over our apathy, over the unimproved talents thus buried,

“And heap the pile with each inglorious name,
On the fall'n altar of their country's fame!”

At this meeting, being the first general one since the accession of his present Majesty to the throne, an address of congratulation was voted to the King as Patron of the Society.

HARLEM PHILOSOPHICAL SOCIETY, 1820.

The Society held its Sixty-seventh Anniversary on May 27 and 28, when the President, M. J. P. Van Wickevoort Crommelin, called upon the Secretary to report the papers which had been sent in since their last sitting in 1819.

Physics.

1. The author of the German memoir, in reply to the question, “What are the general rules according to which it may be judged without actual experience, what exotic plants may be most usefully cultivated in Holland? is Johann Karl Leuchs, of Nuremburg.

2. “What is the cause of the tarnishing (*het weer*) with which glass is affected, after being exposed for some time to the air and sun? and what are the most effectual means of preventing it?” The paper on this question to which the medal has been adjudged is by G. W. Muncke, Professor of Physics at Heidelberg. It was resolved to print also another answer (in German) having for its motto, *La Nature parle par les Experiences*, and to present the author, whenever he chooses to avow himself, with the silver medal.

3. “What uncultivated lands are there in the southern districts of the kingdom that, judging from experiments on similar soils, appear capable of being tilled with success, and of repaying the expense of cultivation: and what would be the most judicious method of undertaking such enclosures with hope of their answering; the whole to be grounded on sure experiments?” Three papers were sent in on this subject, but being unsatisfactory, it was resolved to let it continue for answer till Jan. 1821.

4. Two memoirs on the Draining of the Great Lake at Harlem had been received, but both of them were deemed unsatisfactory.

5. Two also, and equally unsatisfactory ones, respecting the Draining of the Wyker-meer. It was, therefore, determined, that the time assigned for the first of these questions should be extended to Jan. 1, 1822, and that of the second to Jan. 1, 1821.

6. "By what artificial methods can the arms of the sea at the Texel be improved, either entirely or more particularly at the *Schulpen-gat*, and how can they be rendered deeper?" The plan proposed in the memoir sent in was found to be so expensive as to be judged altogether impracticable: it was, therefore, determined to repeat the question, which is to be answered by Jan. 1, 1822.

7. "What knowledge do we possess of those insects which are destructive to objects deposited in cabinets of natural history, and likewise to furs and skins of animals; and what are the most efficacious means of securing them against, or protecting them from, the attacks of such insects?"

Two papers received on the subject, neither of which obtained the prize; one was rejected with indignation as a despicable plagiarism, being little more than a transcript from the second volume of "*Olivier sur les Coleopteres.*" The question is, therefore, repeated, and the answer to be sent in by Jan. 1, 1822.

8. "What is known concerning the circulation of the sap in certain trees and shrubs during the spring; for instance, in the vine, poplar, ash, &c.; what is likely to be learnt in this respect from further experiments; what deductions are to be derived from them as to the cause of the saps rising in plants; and what practical information can be expected to be obtained on this head which may conduce to an improved cultivation of useful trees?" Two answers, one in French, and the other in Dutch, had been received, but neither of them satisfactory. It was, therefore, agreed, that the question should be again proposed; the answer to be delivered by Jan. 1, 1822.

9. "It having been proved by practical agriculture that, during the first vegetation of wheat and other grain, the earth does not diminish in fertility, but that after fructification, and during maturation, the same soil is considerably exhausted, and deprived of its fecundity; it is inquired, what is the cause of this phenomenon, and how far is it likely that the solution of this problem could supply certain rules for ameliorating the cultivation of lands?" A French paper had been received on this subject, but as it contained nothing important or satisfactory, it was determined that the question should be repeated without assigning any limited time for a reply.

10. "What are the principal causes of the degeneration of plants to which we are indebted for varieties of them; and what information can be obtained on this head that may tend to improve the culture of useful plants?" The memoir sent in was deemed unsatisfactory, and it was, therefore, agreed to extend

the time for answer to Jan. 1, 1822. It is requested that the assigned causes be supported by actual experiment and observation.

11. "What kinds of potatoes are generally cultivated in the different provinces of this kingdom; what is the difference with respect to kind and quality? How do they differ particularly with regard to the constituent principles, and the use which is made of them? Is it proved by experience that any one kind is more decidedly nutritious or more salutary than the rest? And what improvement is such knowledge likely to produce in the cultivation of potatoes within this kingdom?" A paper written in French had been sent in as a reply to this question, but was proved by one of the members of the Society to be merely a translation from a German work, published at Weimar, 1819, entitled, "*Versuch einer Monographie der Kartoffeln, &c.*" The question was, therefore, re-proposed for answer before Jan. 1, 1823.

12. The new method of distillery, first practised at Montpellier, and afterwards improved in the South of France (by which the spirituous liquors are not immediately exposed to the fire, but heated by steam), being not only more economical than the common method, but the spirits thus obtained having a more pure and agreeable flavour; and it being, therefore, desirable that this method should be introduced into Holland, the Society had proposed the following question: "What is the best apparatus for thus obtaining, and in the most profitable manner, the purest spirituous liquors from grain, as they are distilled in France from wine?" In reply to this, a paper had been sent in, but what novel information it contained, being unsupported by experiment, it did not obtain the prize, and the question remains open till Jan. 1, 1822.

No answers having been received to the six following questions, they were re-proposed, and the limited time extends to Jan. 1, 1822.

1. "How far is it actually demonstrated that fumigation with chlorine gas has prevented the propagation of contagious diseases? What are the contagious diseases in which it ought to be tried, and what ought to be principally observed in such experiments? Is there any reason to expect more salutary effects from any other method hitherto employed or proposed for this purpose?"

It is requested that a succinct enumeration be given of the cases in which such fumigation has proved effectual in preventing various contagious diseases.

2. "How far does the physiology of the human body afford just grounds for supposing, or how far has experience satisfactorily proved, that oxygen gas is one of the most efficacious remedies for recovering persons who are drowned, suffocated, or

in a syncope? and what are the most prompt and certain methods to be employed for this effect?"

The Society request that the methods successively proposed for the recovery of drowned persons be succinctly explained and examined, as far as our actual knowledge in this respect will admit; and that it be endeavoured to throw as much light as possible, either by new experiments or observations, on whatever circumstances are more or less doubtful.

3. "What is to be considered as justly proved with regard to the gastric juice of the human body, and its influence on the digestion of food? Is its existence sufficiently proved by the experiments of Spallanzani and Senebier, or is it rendered doubtful by those of Montegre? What has been demonstrated in this respect by comparative anatomy, and particularly by opening the stomachs of animals which have been killed, either fasting, or shortly after taking food? And supposing the existence of gastric juice in the human body to be well proved, what ought to be avoided in order that its effect on the digestion may not be impeded?"

4. "How far are we acquainted, from the chemical experiments of Vauquelin, with the various species of *quinquina*; likewise from the experiments and observations of others: 1. What is the different nature and the quantity of their constituent parts? 2. To what particular principle ought we to ascribe the febrifuge power of *quinquina*? 3. What criteria can we deduce from it so as to distinguish the best species (or those possessing greater febrifuge virtues than the rest), and the various barks used as substitutions? 4. Are any rules to be obtained for preserving the principle, in which consists its febrifuge power, entire in the various preparations of *quinquina*?"

5. Dry yeast having been substituted for moist in brewing, the Society demands "a comparison, founded upon chemical analyses, of the nature of yeast both in the moist and dry forms, and a statement of their relative qualities? 2. That a method be pointed out by which liquid yeast may be freed from the bitter and disagreeable flavour occasioned by the hop used in brewing? 3. That some means be shown by which liquid yeast might be preserved for at least some time, so as not to lose the power of fermenting dough?"

6. "It having been observed in many places, and it being still observed, that a variety of plants whose growth is rapid produce a kind of peat, the Society wishes to have a succinct and exact statement and comparison of whatever has been described, or may be observed, on this subject; likewise to have it discussed what methods ought to be observed in order to promote the growth of some species of peat."

The Society then proposed the nine following questions to be answered before Jan. 1, 1822.

1. "Is it true, as has been asserted, that our indigenous iron is inferior to that of Sweden, or any other country, and that it would be impossible to employ it for many purposes for which foreign iron is now used? If this be really the case, to what is this defect to be attributed? Are we to look for it in the metal itself, or in the method of preparing it? Supposing the latter to be the case, might not this iron be carried to such a degree of perfection as to rival foreign iron, and what method is likely to be most effectual for this purpose?"

2. Although a general introduction of vaccination has almost every where put a stop to the epidemic small-pox, yet within these few years past, that disease has reappeared both here and elsewhere, and as a species of variol pustules (termed by the English *modified small-pox*) have recently shown themselves in those who had been vaccinated, it is inquired, "1. Of what description are these pustules? In what do they differ from the real small-pox? Is it the latter that is produced in those individuals who have been previously vaccinated? Does it arise from constitution, from indisposition, from the matter employed in vaccination, or from other circumstances, and what is the method of preventing it? 2. What can be safely asserted with regard to the duration of the preservative virtue of vaccination? Would it prove of any service to re-vaccinate on the re-appearance of the disease? Are the methods employed by us for the encouragement of vaccination sufficient, and do they tend to cause the entire disappearance of the small-pox. In case they are not, what more efficacious ones could be adopted?"

3. "What is the cause, owing to which oysters are occasionally, as was particularly the case last year, so prejudicial to health? Is it in consequence of a small worm that is found in them? In this case, of what species is it, and whereabouts is it more easily detected? Are oysters subject to it only at certain times of the year? Has the venom of oysters any analogy with that which from time to time renders muscles poisonous and unwholesome? What are the disorders occasioned by such oysters and muscles, and what are the most efficacious remedies either for arresting the evil, or for removing it?"

4. "To what is it owing that shrimps are sometimes pernicious? How are such shrimps to be distinguished? What kind of disorders do they occasion, and what are the remedies to which, in such cases, recourse ought to be had?"

5. "As extensive hot-houses are now heated by steam in England, might not this method be adopted among us for small hot-houses, and what would be the most proper apparatus for such a purpose?"

6. "What information has been obtained respecting the nature, habits, and production, of those little insects which are so injurious to plants cultivated in hot-houses, and what method

would such information suggest for preventing the propagation of such insects, or for extirpating them?"

7. "Has experience clearly proved that there are certain trees and plants, particularly of the most useful species, which cannot vegetate when close to each other? And in this case what experiments can be produced as proofs? Can this antipathy between some species be any ways accounted for by what we know of the nature of plants, and what useful information does it supply us with for the cultivation of trees and useful plants?"

8. "What are the insects most hurtful to trees and shrubs in forests? In what consist the injuries they produce? What are the remedies proper to prevent such injuries, or to remove them?"

9. "How far are we acquainted with the economy of moles, and what means does it suggest as most efficacious for ridding lands of them where they prove destructive? Are there, on the contrary, any observations tending to prove that moles are ever useful by destroying other vermin, and how it may be known when moles ought to be tolerated?"

The Society have in preceding years proposed the following 14 questions to be answered before Jan. 1, 1821.

1. "What advantages have resulted to medicine from the extension and reformation of chemistry, since the time of Lavoisier and his successors, by making us better acquainted with the action of the usual medicines, and how can we acquire a certain knowledge of the chemical action, hitherto unknown, of certain medicines?"

2. "How far can it be proved by accurate observation that the diseases prevalent in the Netherlands have, since a certain lapse of time, changed their nature: what are the physical causes of this change, especially as dependant upon the manner of living which is different from what it used formerly to be?"

3. "What is the state in this country of the prisons in general? What are the defects which a scientific examination might point out in them? and what methods should be employed to improve the health of the prisoners?"

4. "What are the most easy and proper methods to which seamen should have recourse to preserve themselves as long as possible in case of shipwreck, and thereby to increase the possibility of their being saved? Is there any method more efficacious than the scaphandre described by M. de la Chapelle? What measures too ought to be taken to enforce the adoption, in every case, of the very best means for retarding the submersion of shipwrecked persons?"

5. "What are the certain signs of the real epidemic (*epizootie*), which for more than 30 years has made such ravages in many southern countries, and likewise our own? Are there sufficient reasons to determine that the said malady never takes place with-

out contagion? In this case, are the precautions used in neighbouring countries to prevent such contagion sufficient to produce perfect security? or if there still remain any apprehension, what should be still done to prevent as much as possible the contagion from spreading?"

6. "How far are we acquainted on chemical principles with the processes employed in brewing different kinds of beer; and what information does our actual knowledge on this point supply us with that may tend either to improve such liquors, or render them more profitable?"

7. "What does experience teach us with regard to what ought to be observed in the multiplication and culture of new varieties of fruit trees by means of seeds in order to prevent the diminution of the good qualities of the varieties that have been obtained, and their entire annihilation?"

8. "In what consists the difference in the constitution of the atmosphere in those parts of the Netherlands which vary most in point of situation, and what are the diseases which are affected by this different disposition?"

9. "Is there any plan by the adoption of which gas light might be advantageously employed in private families?"

10. "What sure and practicable methods can be derived from the present state of physical and chemical knowledge to prevent or arrest endemic maladies, occasioned by the draining of extensive lakes or by exhalations arising from the bottoms of lakes that have been drained?"

11. "What salutary or pernicious alterations does food sustain from the action of fire; and what rules can hence be derived for producing such modifications as may render aliment most nutritious and wholesome?"

12. "How far are we acquainted with the properties of that species of fungi, which grows beneath planks of wood, causing them to decay? Are there any means to be hence suggested either for its prevention or extirpation, or at least a diminution of its pernicious effects?"

13. "What is the peculiar nature of the green matter appearing on the surface of stagnant water in calm and warm weather, particularly during July and August, and known under the name of *Byssus Flos aquæ*? Is there reason to regard it, according to the general opinion, as a cryptogamous plant, or is it of an animal nature? Is it an inorganic production originating in a chemical union of certain principles when the degree of temperature and other circumstances are favourable? What will chemical analysis be able to prove? Is there any method of preventing the production of this matter in water, or of causing it to disappear? Is the water covered with this matter, and producing a disagreeable effluvia injurious to health?"

14. "It is demanded that a complete and succinct system be

given of the rules, according to which fruit trees ought to be trained and pruned in the Netherlands, in order to increase and improve their produce; and what are the physical principles on which these rules are founded?"

ARTICLE X.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS
CONNECTED WITH SCIENCE.

I. *Gelatine from Bones.* By Mr. John Murray.

(To Dr. Thomson.)

SIR,

You are aware that the French have laid claim to the first extraction of gelatine from bones by the aid of diluted muriatic acid, and its employment as a nutritious aliment in hospitals, &c. Signor Carlo di Gimbernati, Counsellor of Legation to the King of Bavaria, however, assured me that the first suggestion was made by him, and carried into effect during the siege of Strasburgh. The besieged were hence enabled to extend their resistance beyond that period to which they had seemed reduced by the horrors of famine. It is but justice to award the merit where it is due.

II. *Naphtha in Coal.* By the Same.

In your very interesting researches on coal and its varieties, you state it as your opinion that naphtha is a proximate produce of coal, and exists in coal gas. Mr. Intow, of Intow Hall, put into my hands a specimen of quartz sandstone, which had been obtained from a considerable depth in one of the Whitehaven collieries, and on which the coal was incumbent. This, when broken, or abraded, exhaled the peculiar smell of naphtha, and yields that substance on distillation.

III. *Primitive Conglomerate.* By the Same.

I have not witnessed for a long time any geological phenomena that have interested me more than the primitive conglomerate near the "General's Hut," on the road from Inverness to Fort Augustus. I call it *primitive*, for the cement is well-defined granite, and the enclosed pebbles are variously sized masses of granite, mica slate, quartz, &c. The imbedded nodules must have undergone attrition by water, and it seems difficult, if not impossible, to conceive that the materials of the paste could have been consolidated otherwise than by the agency of water. It appears to me powerfully conclusive as to the aqueous forma-

tion of granite, while at the same time the contemporaneous existence of nodules of mica slate and granite in the granitic envelope forms an additional proof to the many others that granite is not entitled to take precedence in all formations.

IV. *Insular Climate.* By the Same.

I found a fine corroboration of the opinion I gave on insular climate, and which you were so good as to insert in your last September number, in the state of the thermometers kept at the *Bell Rock*, and in the signal station communicating with it at *Arbroath*. In the former, the range is uniform; and in the latter, the changes are abrupt and irregular.

V. *Galvanism.* By the Same.

Any facts that can be communicated on the production and operation of this singular and powerful agent seem deserving of record, and you will, I dare say, forgive me for adverting to some phenomena which have presented themselves in the course of my experiments, and these may not be altogether unacceptable to the practical electrician. I constantly use the galvanic triads, for which we are indebted to the ingenuity of that enlightened and distinguished philosopher *Dr. Wollaston*, and a comparison between this arrangement and the common form of the trough is altogether out of the question, at least in its powers of igniting platinum, and deflagrating the metals, &c.

I use only three instruments in my experiments, each containing 10 porcelain cells, the triads being four inches square. About 4 lbs. of nitrous acid are expended, and I add merely double the quantity of water, which occupies about one-third of the cells, and seems to act much better than when the cells are full to the top. This always affords me a series of most imposing phenomena, and by this arrangement I can ignite 8 to 10 inches of platinum wire of 100th inch diameter to whiteness. When I deflagrate gold, silver, &c. foils, I plunge the extremity of one of the conducting rods into a flat glass dish containing mercury, say a foot diameter, and brush the surface with the foil suspended from the other arm. The deflagration is then of the most brilliant description, because the sweep is extensive.

It is material to have the conducting wires of considerable diameters; I never use them less than one-eighth of an inch.

I have elsewhere pointed out the fact that when the plates are heated in a sand-bath, or the acid medium into which the plates are plunged, raised in temperature, there is a decided increase of action, and that the energy is at least renewed by raising the plates, and suspending them for a short period in the atmosphere.

When nitric acid is used, the zinc alone is corroded, but in a mixture of sulphuric and nitric acids, then both copper and zinc are dissolved, and much acid vapour is developed, exceedingly annoying to the operator.

VI. *Arctic Expedition.*

About the beginning of November intelligence was received that this long looked-for expedition was on its way towards this country, where it has since arrived in safety. The expedition consisted of the Hecla and Griper, Messrs. Parry and Liddon, Commanders; and the following is a summary of the most important particulars, which have yet transpired, respecting its operations and discoveries:

In the beginning of Aug. 1819, the expedition reached Lancaster's Sound, and having entered this, proceeded directly to the westward between the parallels of 74° and 75° N. lat. and at length penetrated in that direction as far as long. $112\frac{1}{2}^{\circ}$ W. from Greenwich. In this space 12 islands were discovered, which were named the islands of New Georgia. The winter setting in about the middle of September, they repaired to a harbour on the south side of the largest of the above-mentioned islands (named Melville Island) situated in lat. $74^{\circ} 47'$ N. and long. $110^{\circ} 47'$ W. where they passed the winter. When the ice broke up at the beginning of August of the present year, they left the above harbour, and proceeded to the westward, the ships being at this time in perfect condition, the officers and men in excellent health, and with every prospect of the final accomplishment of their purpose. At the south-west end, however, of Melville Island, the quantity and magnitude of the ice increased to such a degree that after 16 days (being one-third of the whole time of summer in these regions), it was found impracticable to get beyond long. $113^{\circ} 47'$ W. They then ran along the edge of the icy barrier to the southward, but could find no opening. Being thus foiled in their attempts to proceed further, and their provisions being nearly exhausted, they found themselves under the necessity of returning.

The greatest degree of cold experienced was in January, when it reached 52° , or, according to other accounts, $55\frac{1}{2}^{\circ}$ below the zero of Fahrenheit's scale. The mean temperature for 12 months was found to be about $1\frac{1}{2}^{\circ}$ or 2° below zero. The darkness, which lasted from the beginning of November till the beginning of February, is said to have been so great in the middle of winter that at noon the largest type was not legible.

The scientific gentlemen connected with the expedition have made a great number of interesting and important observations on magnetism. They have also collected a few objects in most of the departments of natural history, particularly ornithology. It is to be regretted, however, that the *red snow* which attracted so much notice on the former occasion could not be now found (at least in the state in which it was said to have then existed), and consequently that the question respecting its origin and nature could not be set at rest.

ARTICLE XI.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

Dr. Prout's Work, entitled "An Inquiry into the Nature and Treatment of Gravel, Calculus, and other Diseases, connected with a deranged Operation of the Urinary Organs," is very nearly ready for publication.

A Dictionary of Chemistry, in which the Principles of the Science will be investigated, and its Application to the Phenomena of Nature, Medicine, Mineralogy, Agriculture, and Manufactures detailed, by Dr. Andrew Ure, in 1 Vol. 8vo. is nearly ready.

Mr. Price, Surgeon and Electrician, has in the press an Essay on the Medical Application of Electricity and Galvanism.

Dr. Macculloch has nearly completed an Elementary Work on Geology, and is now preparing a Description of Shetland.

JUST PUBLISHED.

Illustrations of Phrenology. By Sir G. Mackenzie. With Engravings. 8vo. 15s. boards.

A Treatise on Dyspepsia, or Indigestion. By J. Woodforde, M.D. 8vo. 2s. 6d.

Outlines of Midwifery. By J. T. Conquest, M.D. With 12 Engravings. 12mo. 7s. 6d. boards.

A Synopsis of the Diseases of the Eye, and their Treatment; to which are prefixed a short Anatomical Description, and a Sketch of the Physiology of that Organ. By Benjamin Travers, F.R.S.

The Climate of London; deduced from Meteorological Observations made at different Places in the Neighbourhood of the Metropolis. By Luke Howard. 2 vols. 8vo. 1l. 5s. boards.

Lectures on the Philosophy of the Human Mind. By the late Thomas Brown, M.D. Prof. of Moral Philosophy in the University of Edinburgh. 4 vols. 8vo. 2l. 12s. 6d. boards.

A Treatise on the Art of Brewing; exhibiting the London Practice of Brewing Porter, Brown Stout, Ale, Table-Beer, &c. By Frederick Accum. With Plates. 12mo. 9s. boards.

A Treatise on the Art of making Wine from native Fruits. By the same Author. 12mo. 3s. boards.

Description of Instruments, designed for extending and improving Meteorological Observations. By John Leslie, Esq. 8vo. 2s.

ARTICLE XII.

NEW PATENTS.

Robert Frith, of Salford, Lancashire, for improvements in dyeing and printing various colours, so as to make the same permanent or fast on cottons, lincens, silks, mohair, worsted, and woollens, straw, chip, and Leghorn. Oct. 9, 1820.

William Harvey, of Belper, Derbyshire, for improvements in ropes and belts by machinery, and also improvements in the said machinery. Oct. 12.

Richard Witty, of Sculcoates, Yorkshire, for improvements in pumps, of various constructions, for raising and conveying water and other liquids; and methods of applying a certain principle, or principles, to ship pumps, and for other useful purposes. Oct. 16.

ARTICLE XIII.

Astronomical, Magnetical, and Meteorological Observations.

By Col. Beaufoy, F.R.S.

Bushey Heath, near Stanmore.

Latitude 51° 37' 44·27" North. Longitude West in time 1' 20·93".

Astronomical Observations.

| | | | | |
|---------|--|---|------------------------|-------------------------|
| Oct. 2. | Emersion of Jupiter's third satellite | { | 9 ^h 45' 26" | Mean Time at Bushey. |
| | | } | 9 46 47 | Mean Time at Greenwich. |
| 6. | Emersion of Jupiter's second satellite | { | 10 45 32 | Mean Time at Bushey. |
| | | } | 10 46 53 | Mean Time at Greenwich. |

Magnetical Observations, 1820. — Variation West.

| Month. | Morning Observ. | | | Noon Observ. | | | Evening Observ. | | |
|-----------------|--------------------|------------|-----|--------------|--------------------|------------|-----------------|-------|--|
| | Hour. | Variation. | | | Hour. | Variation. | | Hour. | Variation. |
| Oct. 1 | 8 ^h 45' | 24° | 29' | 58" | 1 ^h 45' | 24° | 39' | 31" | Owing to the shortness of the days, evening observations discontinued. |
| 2 | 8 35 | 24 | 29 | 48 | 1 35 | 24 | 39 | 53 | |
| 3 | 8 35 | 24 | 29 | 52 | 1 20 | 24 | 39 | 42 | |
| 4 | 8 35 | 24 | 31 | 21 | 1 20 | 24 | 39 | 08 | |
| 5 | 8 40 | 24 | 30 | 56 | 1 30 | 24 | 38 | 36 | |
| 6 | 8 35 | 24 | 31 | 05 | 1 20 | 24 | 39 | 25 | |
| 7 | 8 40 | 24 | 30 | 07 | 1 15 | 24 | 40 | 29 | |
| 8 | 8 40 | 24 | 30 | 10 | 1 20 | 24 | 39 | 44 | |
| 9 | 8 35 | 24 | 30 | 26 | 1 20 | 24 | 40 | 40 | |
| 10 | 8 40 | 24 | 32 | 20 | 1 20 | 24 | 39 | 36 | |
| 11 | 8 40 | 24 | 31 | 15 | 1 20 | 24 | 39 | 19 | |
| 12 | 8 35 | 24 | 30 | 48 | 1 25 | 24 | 40 | 03 | |
| 13 | 8 35 | 24 | 30 | 32 | 1 25 | 24 | 37 | 56 | |
| 14 | 8 35 | 24 | 30 | 50 | 1 30 | 24 | 39 | 57 | |
| 15 | 8 40 | 24 | 31 | 06 | 1 40 | 24 | 39 | 18 | |
| 16 | 8 35 | 24 | 30 | 47 | 1 20 | 24 | 39 | 42 | |
| 17 | 8 35 | 24 | 30 | 07 | 1 25 | 24 | 39 | 06 | |
| 18 | 8 35 | 24 | 30 | 52 | 1 20 | 24 | 41 | 00 | |
| 19 | 8 30 | 24 | 30 | 58 | 1 20 | 24 | 40 | 51 | |
| 20 | 8 30 | 24 | 31 | 56 | 1 15 | 24 | 39 | 52 | |
| 21 | 8 25 | 24 | 33 | 15 | — | — | — | — | |
| 22 | — | — | — | — | 1 15 | 24 | 39 | 32 | |
| 23 | 8 35 | 24 | 30 | 06 | 1 20 | 24 | 39 | 36 | |
| 24 | 8 35 | 24 | 32 | 45 | 1 50 | 24 | 38 | 24 | |
| 25 | 8 45 | 24 | 31 | 49 | 1 15 | 24 | 39 | 32 | |
| 26 | 8 40 | 24 | 32 | 15 | 1 20 | 24 | 38 | 54 | |
| 27 | 8 35 | 24 | 31 | 26 | 1 20 | 24 | 38 | 41 | |
| 28 | 8 45 | 24 | 33 | 36 | 1 25 | 24 | 39 | 35 | |
| 29 | 8 35 | 24 | 31 | 54 | — | — | — | — | |
| 30 | 8 15 | 24 | 30 | 14 | — | — | — | — | |
| 31 | 8 40 | 24 | 31 | 32 | 1 25 | 24 | 39 | 21 | |
| Mean for Month. | 8 36 | 24 | 31 | 00 | 1 24 | 24 | 39 | 33 | |

Meteorological Observations.

| Month. | Time. | Barom. | Ther. | Hyg. | Wind. | Velocity. | Weather. | Six's. | |
|----------|----------|----------|--------|------|---------|-----------|-----------|-----------|-----|
| Oct. | | Inches. | | | | Feet. | | | |
| | 1 | Morn.... | 29.682 | 50° | 79° | NW | | Very fine | 48½ |
| | | Noon.... | 29.748 | 56 | 55 | WNW | | Very fine | 57½ |
| | | Even.... | — | — | — | — | | — | — |
| | 2 | Morn.... | 29.943 | 49 | 68 | NW by W | | Very fine | 43½ |
| | | Noon.... | 29.987 | 55 | 54 | NW by W | | Fine | 56 |
| | | Even.... | — | — | — | — | | — | — |
| | 3 | Morn.... | 30.088 | 50 | 68 | NE by N | | Very fine | 46 |
| | | Noon.... | 30.102 | 54 | 51 | NE | | Very fine | 56 |
| | | Even.... | — | — | — | — | | — | — |
| | 4 | Morn.... | 30.102 | 44 | 68 | ENE | | Fine | 39 |
| | | Noon.... | 30.031 | 55 | 54 | ENE | | Fine | 56½ |
| | | Even.... | — | — | — | — | | — | — |
| | 5 | Morn.... | 29.920 | 52 | 71 | ENE | | Cloudy | 47⅔ |
| | | Noon.... | 29.859 | 57 | 53 | ENE | | Very fine | 57¼ |
| | | Even.... | — | — | — | — | | — | — |
| | 6 | Morn.... | 29.792 | 50 | 68 | ENE | | Very fine | 45½ |
| | | Noon.... | 29.781 | 58 | 58 | ENE | | Very fine | 58 |
| Even.... | | — | — | — | — | | — | — | |
| 7 | Morn.... | 29.728 | 50 | 77 | ENE | | Cloudy | 45 | |
| | Noon.... | 29.713 | 59 | 58 | ENE | | Very fine | 59½ | |
| | Even.... | — | — | — | — | | — | — | |
| 8 | Morn.... | 29.726 | 49 | 67 | NE by E | | Fine | 44½ | |
| | Noon.... | 29.734 | 56 | 57 | NE by E | | Cloudy | 57⅔ | |
| | Even.... | — | — | — | — | | — | — | |
| 9 | Morn.... | 29.783 | 48 | 68 | NE | | Cloudy | 45 | |
| | Noon.... | 29.772 | 50 | 61 | E by N | | Cloudy | 50 | |
| | Even.... | — | — | — | — | | — | — | |
| 10 | Morn.... | 29.700 | 46 | 73 | NE | | Cloudy | 44 | |
| | Noon.... | 29.654 | 50 | 65 | NE by E | | Cloudy | 51 | |
| | Even.... | — | — | — | — | | — | — | |
| 11 | Morn.... | 29.552 | 45 | 73 | NE | | Fine | 48 | |
| | Noon.... | 29.530 | 49 | 52 | NNE | | Cloudy | 51 | |
| | Even.... | — | — | — | — | | — | — | |
| 12 | Morn.... | 29.593 | 43 | 66 | N | | Very fine | 39 | |
| | Noon.... | 29.590 | 51 | 52 | NE by E | | Very fine | 51 | |
| | Even.... | — | — | — | — | | — | — | |
| 13 | Morn.... | 29.538 | 42 | 69 | WNW | | Showery | 38 | |
| | Noon.... | 29.520 | 50 | 59 | WNW | | Cloudy | 50⅔ | |
| | Even.... | — | — | — | — | | — | — | |
| 14 | Morn.... | 29.278 | 46 | 64 | SE | | Cloudy | 44 | |
| | Noon.... | 29.087 | 50 | 56 | SSE | | Fine | 51 | |
| | Even.... | — | — | — | — | | — | — | |
| 15 | Morn.... | 28.560 | 57 | 78 | SSW | | Showery | 46 | |
| | Noon.... | 28.513 | 60 | 74 | SW by W | | Showery | 60 | |
| | Even.... | — | — | — | — | | — | — | |
| 16 | Morn.... | 28.723 | 46 | 70 | SSW | | Showery | 44 | |
| | Noon.... | 28.700 | 50 | 58 | SW | | Showery | 51 | |
| | Even.... | — | — | — | — | | — | — | |
| 17 | Morn.... | 28.389 | 49 | 71 | SW | | Showery | 44½ | |
| | Noon.... | 28.353 | 52 | 64 | W by S | | Showery | 52½ | |
| | Even.... | — | — | — | — | | — | — | |
| 18 | Morn.... | 28.519 | 42 | 73 | W by S | | Fine | 40 | |
| | Noon.... | 28.534 | 51 | 59 | W by N | | Fine | 53 | |
| | Even.... | — | — | — | — | | — | — | |

| Month. | Time. | Barom. | Ther. | Hyg. | Wind. | Velocity. | Weather. | Six's. |
|--------|----------|---------|-------|------|---------|-----------|-----------|--------|
| | | Inches. | | | | Feet. | | |
| Oct. | Morn.... | 28·854 | 45° | 69° | WNW | | Fine | 42½ |
| | Noon.... | 28·858 | 48 | 60 | W | | Fine | 49 |
| | Even.... | — | — | — | — | | — | — |
| 19 | Morn.... | 28·459 | 42 | 73 | WSW | | Very fine | 40½ |
| | Noon.... | 28·468 | 45 | 65 | W | | Showery | 48 |
| | Even.... | — | — | — | — | | — | — |
| 20 | Morn.... | 28·911 | 40 | 70 | W by N | | Very fine | 37½ |
| | Noon.... | — | — | — | — | | Very fine | 51 |
| | Even.... | — | — | — | — | | — | — |
| 21 | Morn.... | 28·489 | — | 81 | SSW | | Rain | 35½ |
| | Noon.... | 28·359 | 49 | 79 | SW | | Showery | 50 |
| | Even.... | — | — | — | — | | — | — |
| 22 | Morn.... | 28·583 | 46 | 72 | W by N | | Sm. rain | 40 |
| | Noon.... | 28·659 | 51 | 63 | W by N. | | Cloudy | 52½ |
| | Even.... | — | — | — | — | | — | — |
| 23 | Morn.... | 28·434 | 44 | 82 | SSW | | Cloudy | 42 |
| | Noon.... | 28·264 | 44 | 77 | WNW | | Rain | 49 |
| | Even.... | — | — | — | — | | — | — |
| 24 | Morn.... | 28·547 | 43 | 74 | W by N | | Showery | 40 |
| | Noon.... | 28·655 | 50 | 63 | WNW | | Fine | 50⅔ |
| | Even.... | — | — | — | — | | — | — |
| 25 | Morn.... | 28·842 | 40 | 71 | SSE | | Rain | 37 |
| | Noon.... | 28·538 | 46 | 83 | SSW | | Fine | 50⅓ |
| | Even.... | — | — | — | — | | — | — |
| 26 | Morn.... | 28·533 | 47 | 75 | WSW | | Fine | 45 |
| | Noon.... | 28·595 | 47 | 73 | W | | Showery | 51 |
| | Even.... | — | — | — | — | | — | — |
| 27 | Morn.... | 29·089 | 43 | 71 | W by N | | Clear | 39 |
| | Noon.... | 29·167 | 50 | 60 | WNW | | Very fine | 51 |
| | Even.... | — | — | — | — | | — | — |
| 28 | Morn.... | 29·039 | 41 | 68 | SSE | | Rain | 37 |
| | Noon.... | 28·729 | — | 70 | SE | | Hardrain | 45½ |
| | Even.... | — | — | — | — | | — | — |
| 29 | Morn.... | 29·073 | 38 | 79 | SW by W | | Clear | 37⅔ |
| | Noon.... | — | — | — | — | | — | 50 |
| | Even.... | — | — | — | — | | — | — |
| 30 | Morn.... | 29·023 | 42 | 79 | SSE | | Foggy | 39¼ |
| | Noon.... | 28·886 | 46 | 69 | ESE | | Cloudy | 46½ |
| | Even.... | — | — | — | — | | — | — |
| 31 | Morn.... | — | — | — | — | | — | — |
| | Noon.... | — | — | — | — | | — | — |
| | Even.... | — | — | — | — | | — | — |

Rain, by the pluviometer, between noon the 1st of Oct. and noon the 1st of Nov. 2·538 inches. Evaporation, during the same period, 2·500 inches.

ARTICLE XIV.

METEOROLOGICAL TABLE.

| 1820. | Wind. | BAROMETER. | | THERMOMETER. | | Evap. | Rain. | Hygr. at
9 a. m. |
|----------|-------|------------|-------|--------------|------|-------|-------|---------------------|
| | | Max. | Min. | Max. | Min. | | | |
| 10th Mo. | | | | | | | | |
| Oct. 1 | N W | 30.46 | 30.19 | 58 | 41 | — | | 61 |
| 2 | N W | 30.58 | 30.46 | 58 | 46 | — | | 65 |
| 3 | N E | 30.60 | 30.58 | 58 | 34 | — | | 62 |
| 4 | N E | 30.60 | 30.43 | 58 | 44 | — | 05 | 53 |
| 5 | E | 30.43 | 30.33 | 60 | 45 | — | | 62 |
| 6 | E | 30.33 | 30.27 | 61 | 40 | 56 | | 61 |
| 7 | N E | 30.27 | 30.25 | 62 | 40 | — | | 72 |
| 8 | N E | 30.30 | 30.25 | 60 | 40 | — | | 57 |
| 9 | N E | 30.30 | 30.23 | 53 | 43 | — | | 58 |
| 10 | N | 30.23 | 30.08 | 52 | 44 | — | 04 | 69 |
| 11 | N | 30.14 | 30.08 | 53 | 37 | — | | 64 |
| 12 | N | 30.14 | 30.12 | 54 | 25 | — | | 58 |
| 13 | S W | 30.12 | 29.83 | 52 | 38 | 40 | 02 | 78 |
| 14 | S E | 29.83 | 29.02 | 61 | 48 | — | 11 | 61 |
| 15 | S | 29.14 | 29.02 | 62 | 44 | — | 15 | 63 |
| 16 | S W | 29.02 | 28.87 | 52 | 44 | — | 21 | 64 |
| 17 | S W | 28.97 | 28.87 | 56 | 41 | — | | 63 |
| 18 | W | 29.28 | 28.97 | 54 | 41 | — | 03 | 68 |
| 19 | N W | 29.31 | 28.92 | 51 | 41 | — | 20 | 63 |
| 20 | W | 29.50 | 28.92 | 53 | 36 | — | 13 | 61 |
| 21 | N W | 29.50 | 28.50 | 54 | 34 | 55 | 02 | 70 |
| 22 | S W | 29.10 | 28.50 | 52 | 43 | — | 48 | 67 |
| 23 | N W | 29.10 | 28.53 | 54 | 43 | — | 09 | 66 |
| 24 | S W | 29.20 | 28.53 | 52 | 40 | — | 35 | 68 |
| 25 | N W | 29.37 | 28.88 | 52 | 37 | — | | 72 |
| 26 | S | 29.10 | 28.85 | 53 | 42 | — | 22 | 69 |
| 27 | S W | 29.70 | 29.10 | 54 | 39 | — | 05 | 68 |
| 28 | N W | 29.70 | 29.10 | 50 | 37 | 50 | | 66 |
| 29 | S | 29.60 | 29.10 | 48 | 37 | — | 15 | 60 |
| 30 | S W | 29.60 | 29.20 | 53 | 32 | — | | 73 |
| 31 | S E | 29.53 | 29.15 | 49 | 42 | 11 | | 78 |
| | | 30.60 | 28.50 | 62 | 25 | 2.12 | 2.30 | 78—53 |

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Tenth Month.—1—8. Fine. 9, 10. Cloudy. 11, 12. Fine. 13. Hoar frost: cloudy. 14—16. Showery: a little hail: very tempestuous night, with frequent showers. 19. Cloudy. 20. Cloudy: a shower of hail at two, p. m. 21. Foggy morning: cloudy day: stormy, wet night. 22. Stormy, with rain and sleet. 23—25. Cloudy. 27. Showers: cloudy. 28. Cloudy. 29. Rainy. 30. Fine. 31. Cloudy.

RESULTS.

Winds: N, 3; NE, 5; E, 2; SE, 2; S, 3; SW, 7; W, 2; NW, 7.

Barometer: Mean height

| | |
|---|----------------|
| For the month. | 29·615 inches. |
| For the lunar period, ending the 27th | 29·686 |
| For 14 days, ending the 5th (moon north) | 30·182 |
| For 14 days, ending the 19th (moon south) | 26·720 |

Thermometer: Mean height

| | |
|---|---------|
| For the month. | 47·379° |
| For the lunar period, ending the 27th | 48·224 |
| For 30 days, the sun in Libra. | 49·416 |

Evaporation 2·12 inch.

Rain 2·30

Mean of hygrometer 65°

At Tottenham, mean temperature for the month (the 4th, 5th, and 27th days wanting), 47·50°; rain, 1·90 in. On the 17th, a brilliant meteor, about seven, p. m. descending to the NW, followed by a lunar halo. 19. Solar halo, at three, p. m. 20. Several fluely-coloured *Nimbi* in the S. about sun-set. The swallows continued about till near the close of the month in considerable numbers. The movements of the barometer, after the great depression on the 14th, were singularly desultory, the curve changing its direction almost every 24 hours.

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