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On the 1st of May next will be Published by Mr. Murray, in Albemarle-Street, in 3 vols., 8vo.,

A MANUAL OF CHEMISTRY,

Containing the principal Facts of the Science, arranged as they are discussed and illustrated in the Lectures at the Royal Institution of Great Britain, by

WILLIAM THOMAS BRANDE,

Sec. R. S., Professor of Chemistry Royal Institution, &c.

The subjects are treated of in these volumes in the following order:

Vol. I.—A Prefatory History of the Progress of Chemical Science, from early times to the End of the last Century.

General View of the Principles of Chemistry, and of the Phænomena of Attraction, Heat, and Electricity.

History of Radiant or Imponderable Matter; of the Simple Supporters of Combustion; of the Acidifiable Bodies; and of their Mutual Combination.

Vol. II.—General and Individual History of the Metals, and of their Combinations.

Of the Analysis of Metalliferous Compounds, and of Mineral Waters.

Vol. III.—History of the Organic Products of the Vegetable and Animal Kingdoms.

Outline of Geological Theories, and of the Structure of the Earth.

A very Copious Index of Reference is subjoined to this Volume.

This work is illustrated by several Copper-plate Engravings of the Laboratory of the Royal Institution, &c.; and by more than one Hundred Engravings on Wood, illustrative of the different Apparatus employed in Chemical Researches; and of the Subject of Geology: it also contains a variety of useful Tables and Diagrams.

TO CORRESPONDENTS.

We are much obliged by the Lucubrations of ELÆOGABALUS; but the subject is too serious to be so treated.

The information contained in E.E's Letter will be acted upon when he favours us with his address.

The communication from Liverpool, adverted to in the Letter signed Edward H——, has not been received.

CARTHUSIANUS is wrong, as the following quotations show:

Vos estis sal terræ; *quod si sal infatuatum* fuerit, quo saletur?
(*Vide* Novum Testamentum, *ex Sebast. Castellionis interpretatione.*)

SAL: *neutraliter* condimentum; *masculinum* pro sapientia.
(*Vide* GESNERI *Thesaurus*.)

The best answer we can give to a Correspondent, whose signature is mislaid, will be found at page 222 of this Number.

The Anatomical facts contained in a Letter, with which we have been favoured, dated East Bergholt, Suffolk, March 10, are scarcely sufficiently explanatory; we have, therefore, not noticed them, in the hope that our Correspondent will be able to furnish us with more precise information.

We have received several valuable documents respecting the Combustion of Smoke, Consumption of Fuel, &c., in various furnaces and fire-places, but shall not enter upon the subject till further experimental evidence is before us.

Our OCCASIONAL READER will probably find something to his purpose in our next Number, when we hope to take up the subject of warming and ventilating houses and public buildings.

At present we must decline all interference respecting the subject of two Letters which we have received, the one dated "from the Tombs of Westminster;" the other purporting to be the advice of "an eminent sculptor." The latter is very wrong in his conclusions; the former, more amusing than just.

The queries respecting the reflectors of Telescopes, must, for the present, remain unanswered.

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THE

QUARTERLY JOURNAL,

April, 1821.

Art. I. *On the Forms of Mineralogical Hammers.* By J. Mac Culloch, M.D., F.R.S.

[Communicated by the author.]

THOSE who have not been very conversant in countries consisting of primary and trap rocks, will not easily believe how difficult it is to procure specimens from many of these substances by means of any of the hammers in common use. This is more particularly the case with some of the members of the trap family, which are often characterized by an uncommon degree of toughness or tenacity; and it is not uncommon in those granites which are of a fine grain, and which contain a conspicuous proportion of hornblende. In those varieties of gneiss in which compact feldspar predominates, in some of the members of the primary sandstone series, and in the varieties of hornblende schist in which the lammar structure is obscure or wanting, the same difficulty frequently occurs, and to such a degree as absolutely to defy the utmost efforts of the heaviest hammers in common use, whether by stone-masons or mineralogists. Serpentine also very often, and diallage rock almost always, present such a resistance as to deprive the collector of the power of obtaining satisfactory or sufficient specimens; but it is unnecessary to enumerate all the rocks which those who are in the least conversant with this department of mineralogy must occasionally have abandoned in despair.

Independently, however, of the wish to obtain a mere specimen, such as can first be detached, it is often desirable to obtain a deep access to the rock under examination, on account of the changes which the superficial parts undergo from the loss of

their water, or from the ordinary effects of exposure. In such cases it is necessary to procure specimens in succession from the same point; an attempt, in which ordinary means will often fail, from the gradual loss of the protuberances or angles on which alone an impression can be made by a moderate or ordinary force. It is also often desirable to obtain a cross fracture of some of the schistose rocks, as in micaceous schist, for the purpose of displaying the contortions. This, from the greater facility with which the laminæ yield to a moderate force according to their direction, can rarely be effected by any ordinary hammer; requiring a greater and more concentrated impulse, and often, indeed, demanding the very sudden effort communicated by gunpowder.

The weight of a hammer required to produce such effects, if of the ordinary construction, is a serious inconvenience to a geologist; who must, in many cases, necessarily examine the ground which he is investigating, on foot, and who is also not unfrequently incumbered with specimens. Nor will mere weight answer the purpose, as a very slight consideration of the laws which regulate the communication of motion will show. Having at first suffered much inconvenience from the use of hammers of the common construction, the geological readers of the *Quarterly Journal* will not be sorry to know the expedients which I adopted to diminish it; and, to render the form which I have used for some years past more intelligible, I have accompanied this communication with explanatory sketches.

That I may not incumber a plain practical question with mathematical considerations, I shall only here remark, that although the momentum of a body is compounded of the weight and velocity, and that the same absolute quantity of motion may be communicated under varying relative proportions of these two elements, the disintegration of bodies is regulated by other rules, and a diminution of velocity cannot be compensated in this case by an addition of weight. It is by an increase of the impulse that the cohesion of bodies is overcome: a great weight causes the body to move in one mass; a great velocity strikes off a fragment, or breaks the whole to atoms. Illus-

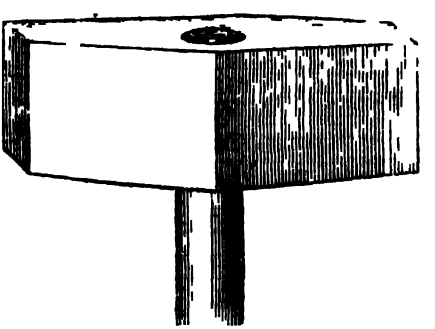
trations of this law must be familiar to every one; the penetration of a musket ball through an open door is well known; and the same is no less true in the case of elastic fluids. Thus the action of fulminating mercury will break, in the gun, that shot which common gunpowder will project; and thus also, in splitting rocks, the greatest effect is produced by the worst gunpowder. An attention to this simple law would have prevented the useless attempts so often made to substitute the stronger detonating compounds in the practice of artillery but I must not enter, further than is necessary, into this subject, though it will immediately be seen how this doctrine bears on the use of mineralogical hammers

In striking a fragment from a mass of rock, and equally, indeed, in detaching the smaller superfluous parts from specimens, it is necessary that a vibration should be excited in one place, or lamina, and, by the communication of a limited motion among the particles of this lamina, the parts at rest on each side are separated from it In the harder and tougher stones the nature of this process is distinctly to be seen, as it is also in the more brittle and compact as well as in glass In the former it will be found that the point immediately subject to the impulse is bruised, and that the area of vibration extends, in a somewhat concentric manner, along some lamina which is generally determined by the texture of the rock in common flint, and in glass the conchoidal form of the fracture is easily seen to respect the point of impulse.

It is therefore necessary, in breaking a rock, or in detaching a large fragment from a solid mass, not only that the impulse should be considerable and proportioned to the tenacity of the substance, but that it should be directed on one point, or on one line, or at least on a small surface The smallness of the surface of contact between the hammer and the stone, is, however, not only useful in this way, by causing the vibration of the lamina which is to separate the adjacent parts, but it produces the further effect of concentrating the whole weight, or rather momentum, of the former on one place, instead of suffering it to be wasted by being directed on many points at once. In the form

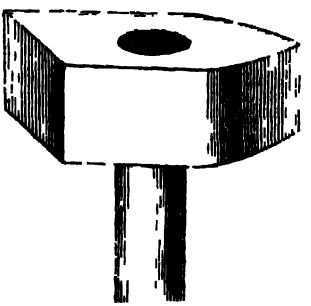
here adopted, a very small weight will thus be found adequate to produce an effect which would be in vain expected from the same acting on a larger surface, or exposing a broad face of contact. It is true, that, in the ordinary practice of masons and quarrymen, a flat-faced hammer will detach a fragment, by communicating motion to the whole of it, while the mass is comparatively at rest; but it will at the same time be recollected, with how little effort blocks of granite and marble are split into two parts by the comparatively slight blows given on the feather wedges, and how hopeless an attempt it would be to separate such masses by any practicable momentum applied to one half of them. The object of the improvement here suggested is, as in other cases of mechanics, to œconomize power; and though the arm of a practised quarryman may render such expedients of comparatively little value to him, that of a mineralogist is seldom in a condition to despise them.

The ordinary mason's hammer, used for breaking rough stones for rubble work, is formed of two frusta of pyramids on a common parallelogramic base, (to describe it mathematically,) and the blade is of considerable length. The faces, it is true, are thus somewhat narrow in proportion to their length, but yet they present far too large a surface. This hammer is attended

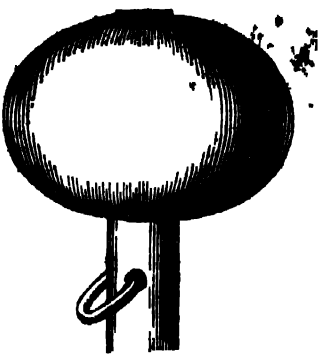


with another serious inconvenience, in consequence of the length of the blade. If the blow is not given in such a manner that the line joining the centre of gravity (or percussion) with the point of impulse, is vertical to the surface of the stone struck, the blow fails, or, at least, a portion of the momentum is lost.

Nor is the missing of a blow attended with impunity; as the length of the lever afforded by the blade, twists the handle in the hand, and injures the wrist if too strongly grasped, as it always will be by an inexpert practitioner. The mineralogical hammer generally in use, is not, it is true, so long or narrow as that used by masons; but, having a broad flat face, it is a feeble instrument, and produces a small effect in proportion to its weight; while it cannot be increased in size so as to compensate this defect, as it becomes inconvenient to carry, and requires too much strength to give it the requisite velocity.



The construction by which these defects are remedied, and the greatest effect produced with the least possible weight and strength of arm, is that where the face of the hammer is round, or spheroidal. Theoretically, an obtuse wedge would, perhaps, be generally preferable; but it is scarcely possible to give the blow in such a manner that the centre of percussion should fall in the true line; and, in such a form, the slightest deviation causes the blow to be wasted. A hammer in the form of a sphere, would, indeed, ensure the effect of every blow, but it is very difficult to steel such a figure all round, and it cannot be made all of steel, since it will not stand without a centre of iron. Besides, with a weight of three and a half or four pounds, the surface of the sphere becomes somewhat less curved than is convenient for making the impulse on one point. I have, therefore, preferred the form of an ellipsoid, and the particular figure will be better understood from the accompanying drawing, than from any description.



It is plain that, with a solid of this form, any deviation from the most favourable line of impulse which is likely to happen, will have but little effect in diminishing the force of the blow; and that the whole momentum will be concentrated on one point. From the shortness of the blade, or the small distance of the face from the axis of the handle, the missing of a blow by the sliding of the hammer on an oblique surface, and its consequent attempt to turn round, communicates no strain to the hand.

The weight of such a hammer need not be very great, nor is any advantage, indeed, to be gained by increasing it beyond a certain point, proportioned to the strength of the arm which is to use it; as, in such cases, the power of the impulse is diminished. A few trials will convince any one that, with this construction, a given weight will produce as great an effect as the double, or even much more, would do in a flat or broad faced hammer. It is not convenient, however, that it should be less than two pounds, and it need not exceed four. With hammers of these weights so constructed, almost every object of the mineralogist can be obtained. A weight of three pounds forms a convenient general size for most purposes; and, to facilitate the construction, I may add that, allowing the usual general size for the eye, or hole, the relative diameters of $3\frac{1}{4}$ inches by 2, of $3\frac{3}{4}$ by $2\frac{1}{4}$, and of 4 by $2\frac{3}{4}$, will, in the form here represented, give pretty nearly the weights of two, three, and four pounds, respectively.

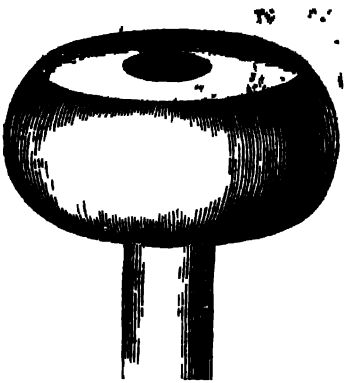
The artist intrusted with the making of these hammers must

be directed to pay attention to the particular form of the ellipsoid represented in the plate. If the longer diameter is made much greater than the short one, for the purpose of securing the necessary weight, the blade becomes too long, and it will have the fault of the mason's hammer. If the face, again, is made of a surface with too long a radius of curvature, it will strike on too large a portion of the rock at once, and part of the blow will be wasted.

There are some minor conveniences arising out of the form of this ellipsoid, which are worthy of notice. The steel cannot easily be struck off the face, as happens at the sides of flat-faced hammers when too much hardened; nor does it yield and turn over as when, in the same construction, they are too soft. The directness of all the blows prevents over-hardened steel from splintering; and, if too soft, a second blow replaces the vacuity which the first may have made. Thus also it retains a degree of smoothness which those will know how to appreciate who have suffered in their hands, their pockets, or their clothes, from the ragged edges of a worn hammer. The durability of such a hammer in practice, is in itself no small convenience; as a mineralogist is not always in a situation to get one replaced or repaired, and of this superior durability, my own experience has afforded ample proof. It is unnecessary for a breaking hammer to be provided with a cutting edge, as the great weight prevents any effectual use being made of it. That which is required is best done by a lighter trimming hammer; and thus also, the breaking hammer, having two faces, has double the durability.

I need scarcely add, that all handles should be made of ash, or vine, if it can be procured, and somewhat of a conoidal form, larger towards the hand, to prevent slipping; and, that to render hammers portable, it is convenient to have a loop of wire near the head, through which a strap may be inserted. This is represented in the sketch.

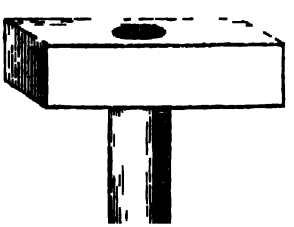
I have added another figure which I have, in practice, found very convenient, where a great weight is required. It is an oblate spheroid, with the polar surfaces cut away, as it is not found easy by the makers to apply the steel to a whole spheroid.



It is unnecessary to dilate further on the advantages of this form; as it is, for all purposes of use, a sphere. From the extent of face, it is almost eternal; and it is not difficult to construct, by welding a ring of bar steel on a nucleus of iron.

The drawings which accompany this communication, represent also the particular form of those hammers for trimming or shaping specimens, which I have found, in practice, to exceed all others.

The ordinary trimming hammer has two cutting edges only, one at each end, and placed in a reverse direction, or like an axe and an adze. Doubtless, this answers its purpose, but not equally well with the construction here represented.



In that there are only two edges, and they soon wear, as these hammers must be made of hard steel. In this there are, at first, four edges; and, as the handle may then be turned, there are thus acquired four more; so that each hammer of this construction is at least as durable as four of the former.

In mineralogical journeys this is particularly convenient, as a trimming hammer [illegible] and the collector must then carry unnecessary weight, or perhaps fail entirely in procuring convenient and well-shaped specimens. In a collection of rocks, where the number and weight, and the room occupied, form so serious an inconvenience, the regular shape of a specimen is an object of no slight moment.

There is an additional advantage in this form of the trimming hammer, arising from the rectangular shapes of the edges, which renders them more durable than those of the axe-shaped hammer. I need scarcely say that they must be regular prisms, that the eye may see the edge which strikes, and that they must be entirely made of steel.

With respect to the weights of trimming-hammers, they must be proportioned chiefly to the weight of the specimen to be broken or shaped, or to the size of the fragments which it will be requisite to detach. They must also bear some relation to the fragility of the specimen; the most brittle requiring the lightest hammers. It is not possible to give any exact rules on this subject, but the general principle has already been stated sufficiently to show that it is only by the velocity of a small weight, or by the impulse, that fragments can be detached from any desired place without disturbing other parts of the specimen. The mineralogist should be provided with different weights, from a drachm to two ounces, and upwards; and his own experience will very shortly direct him to that which will produce the desired effect on any specimen or substance under trial. To facilitate the labours of the artist, I have thought it better to insert a scale of dimensions than of weights, for a set of such hammers, and they are as follows:—

Length of prism—Inches	Side of base—Inches
1½	½
1¾	½
1¾	⅝
2½	¾
2½	1

I may add, lastly, that when worn by use, the hammers of this construction are more easily repaired than the common ones; as, by grinding one face to a small distance downwards, four edges are at once replaced.

In concluding this paper, it will not be improper to suggest that the same principle might be advantageously extended to the ordinary hammers of quarrymen and masons, and more particularly to those of road-makers. The forms of these latter are almost in every instance very faulty, and the consequences are important, as they add double or treble the expense to that which is, in many places, one of the most costly parts of road-making. It is not only indeed in the shape, but in the use of the hammer, that the system of breaking stones for roads is defective. Independently of the fatigue of standing, the same velocity, or impulse, cannot be communicated by two hands as by one, from the crossing, or obliquity of the arms; and, with a single-handed hammer, sitting, one person can easily do the work of two standing, possibly more. This practice has indeed been partially introduced of late, but the prejudices against it are still very general. To render it perfect, the forms of the hammers should also be improved according to the principles already laid down; and the labourer should further be provided with a set of these, of different weights, using them in succession as the size of the materials diminishes under his hands.

Art. II. *Geological Description of Barbadoes, with a Map of the Island.* By James D. Maycock, M. D.

[Communicated by Dr. H. Holland, F.R.S.]

The interest which geology at present excites, and the very respectable station it holds among liberal pursuits, having resulted no less from the industry which has been exercised in the accumulation of facts, than from the genius which has been displayed in the arrangement of them, I am encouraged to attempt a description of the geological features of Barbadoes.

It would be impossible sufficiently to illustrate by any similitude the irregular figure of this island, (Plate I.) It is twenty-one miles in length, and fourteen miles in its greatest breadth

That portion of the coast, the aspect of which is to the west and to the south, is *generally* shelving to the sea with a flat shallow beach; the south-eastern and northern coasts are, on the contrary, perpendicularly precipitous from thirty to sixty feet, and the water immediately becomes deep, except in some of the small creeks, where steep sandy beaches occur under the rocky cliffs: the windward or north-eastern coast to the extent of thirteen or fourteen miles, exhibits a mixed character; the low land sinking very gradually under the sea, and the rugged conical hills terminating not in mural precipices, but sloping abruptly to a flat extended beach. The island is nearly encircled with rocks, many of which are immense masses, separated and rolled a considerable distance from their original situation; but the greater part of this rocky belt consists of the substance of the island extended under the surface of the water in tables, and rising in reefs, or insulated rocks, at no considerable distance from the shore.

The low flat land occupies the northern, southern, and western parts of the island; and rises by precipitous broken acclivities, running *generally* parallel to the coast, in terraces of flat open country to the highest land, situate something to the northward of the centre of the island. This progressive rise is, indeed, sometimes interrupted by the occurrence of valleys; only one of which, termed The Valley, is deserving particular notice. This tract of low land passes from the windward coast of the *thickets* between two elevated ridges, denominated the *Ridge* and the *Cliff*, through the parishes of Saint Philip, and Saint George, to Bridge Town, forming the only general interruption to the regular terraced rise from the sea to the highest land. If the sea were fifty or sixty feet above its present level, Barbadoes would be divided into two islets of an equal size by a narrow strait occupying the site of what is now the valley.

Mount Hallaby is the highest land of the island, its altitude being upwards of nine hundred feet. From this point the high land branches off, in steep precipitous ridges, in two directions, northerly and easterly, and southerly and easterly towards the sea on the windward coast, suddenly diminishing in height as

they approach it. These two ridges of high land include a country, the appearance of which is altogether different from the flat open scenery of that which has just been described. This portion of the island is distinguished by the appellations of SCOTLAND, and BELOW THE CLIFF. The hills in this district are numerous; they are lofty, conical, and steep; and they project irregularly in chains from the ridges of high land, or rise in small groups from the plain, which is little above the level of the sea. The deep valleys intersecting the hills are covered with the most luxuriant vegetation, the hills themselves appearing naked and barren, or richly clothed with timber. The scenery is every where wild, irregular, and picturesque; and displays in miniature all the beauties of a mountainous country.

Such is the striking dissimilarity in the general appearance of the two districts, the hilly and the flat, into which this little island may very properly be divided. Attentive observation points out an essential difference in the immediate substratum of the soil; that of the flat country being entirely calcareous, the soil of the hilly country resting almost exclusively on mineral substances belonging to the clay genus. I shall, however, enter into a more particular description of each district, beginning, with the calcareous or flat country.

Upon examining the structure of the calcareous formation we find it to consist of the spoils of zoophites, of which several species of madreporæ, milleporæ, corallinæ, and alcyoniæ, are strikingly evident. These are cemented together by carbonate of lime, containing an abundance and great variety of the lithophytæ and molluscæ. The cement may be said to vary from marl, more or less indurated, to a hard compact limestone, with conchoidal fracture and translucency on the edges. In some places the organic remains constitute the principal, in all a very considerable, portion of this formation, and although these remains are intimately blended in the common structure, they appear to be arranged in some degree in families; in some situations, the alcyoniæ, in others the madreporæ being most conspicuous. Upon this coralline mass there frequently occur detached beds of white shelly sandstone, the cement and the

grains of which are calcareous. It is quarried for the purposes of building, and, being sufficiently porous, is employed for the filtration of water. It sometimes appears disposed to assume the slaty structure, and when the beds are of considerable thickness, they are stratified. Calcareous spar also, and calc sinter occur abundantly; and I have seen small specimens of white granular limestone. They are found imbedded in the common calcareous rock, and, like the spar, they have been deposited in accidental cavities at a recent period.

The whole of the calcareous portion of the island presents numerous rents and fissures; the smaller are filled with crystallized and other modifications of carbonate of lime; the larger remain open, and are the deep precipitous ravines or gullies, which are so very numerous in the higher parts of this district, and which become during the rainy season, the conducting channels of temporary torrents. Like most other calcareous formations of recent date, it is extremely cavernous; and dislocation and sinking of the surface occasionally takes place at the present time; and, from general appearances, we must conclude that they happened very frequently, and to considerable extent, at former periods. It is to this cause that the island is plentifully supplied with those fissures, denominated sucks, through which the water, frequently lodged on the surface, is drawn off and conducted to the ocean by means of subterranean channels; and to this cause, together with the breaking away of the face of hills, is owing the precipitous mural cliffs so common on the coast, and in the interior of the calcareous district.

The hilly district, or that part of the island which has been denominated *Scotland* and *below the Cliff*, is principally composed of mineral substances belonging to the clay genus; particularly loam, potters'-clay, slate-clay, and clay-stone. There is also found here a fine grained friable sand-stone, which is for the most part micaceous; a ferruginous conglomerate, or pudding stone; quartzy sand-stone, flint, and iron-flint in balls and fragments; gypsum, yellow earth, fullers' earth; and a variety of the ores of iron, such as clay iron-stone, compact black iron-stone, compact and ochrey brown iron-stone; bituminous shale,

mineral oil, and asphaltum; and probably many other minerals; which I have not myself met with. In addition to the above, however, I have found in a hill of Scotland, which, from the white appearance of its broken clayey cliffs, has been improperly termed Chalky Mount, a bed of porphyritic slate, or clinkstone porphyry. It is about eighteen inches in thickness, lies between beds of very loosely cohering sandstone, and dips to N. E. at an angle of 30°. The occurrence of clinkstone porphyry in this situation is deserving attention; for, as the great mass of the hilly district is composed of minerals which I suppose to be most properly associated with the independent coal formation, this fact will stand as an additional proof of the existence of a trap formation of anterior date, as first noticed by Professor Jameson, to that which has been denominated by Werner, the newest flötz trap formation, to which the clinkstone porphyry has been supposed to belong. This part of the island evinces very perfect stratification, the strata being generally much inclined, and not unfrequently distorted.

The petroleum, or mineral oil, the green tar, as it is here termed, occurs in abundance in some situations, exuding from crevices in the clay-hills. It is collected on the surface of water in holes dug for the occasion, and is employed for various economical purposes. The gypsum occurs in fragments, in crystals, and distinct concretions, from a very small magnitude to such as weigh several ounces, disseminated through clay-beds. The fullers' earth is found in Chalky Mount, where I have seen it in a bed a few inches thick, composed of alternate laminæ of fullers' earth and yellow earth. The clay, which is abundantly distributed through the hilly district, is not very pure, being generally charged with iron, petroleum, or calcareous matter; but in most places it answers sufficiently well for the manufacture of coarse ware and bricks; and accordingly there are several pot-kilns in the parishes of St. John, St. Joseph, and St. Andrew. Furnaces are frequently constructed in this island of unburnt bricks; the cement used on such occasions being a paste of the same kind of clay as that of which the bricks are made. Upon the application of heat, the whole be-

comes consolidated into one mass, and furnaces of this description will last many years, although subjected to very strong fires, such as are employed in the manufacture of sugar.

Masses of the calcareous formation, some of considerable magnitude, are to be seen in Scotland; they are either projections from the high ridges which have never been covered by the clay formation, which every where appears superimposed on the calcareous, or they are rolled fragments, of which there is an endless number, and many at a great distance from their original situation, to which, however, they can often be traced.

I cannot omit taking notice in this place of an extinguished *pseudo-volcanic* hill, situate on the windward coast, in one of the estates belonging to the Society for the Propagation of Christian Knowledge. It is to this day very properly denominated the Burnt Hill, and is mentioned by Hughes as having been accidentally set on fire by a slave, and as having continued to burn for the space of five years. It consists entirely of highly burnt clay and earth slag, and the neighbourhood abounds in bituminous shale and mineral oil.

The natural springs of Barbadoes are not very numerous. The inhabitants of the flat country are supplied with water principally from wells, which are frequently of considerable depth; but running streams are abundant in the hilly district, in which occur several saline and one chalybeate spring. There is also a spring in Scotland, called the Burning Spring, which generally attracts the notice of the traveller. This little streamlet rises in a deep sequestered ravine at the foot of a hill, richly clothed with timber; and on its first appearance forms for itself a little basin, in which the water is in a continued state of ebullition, from the passage of inflammable gas through it, which, readily inflaming on the application of a lighted taper, gives to the spring its characteristic appellation. The gas does not indeed rise in great quantity, but the scenery in the approach to the spot is beautiful and imposing; and one can hardly view it without fancying what might have been its celebrity and importance had it been known to a people, prone to attach superstitious veneration to

unusual phenomena. Associations of this kind bestow a mystic interest on the place.

The saline springs make their appearance at an inconsiderable height above the level of the sea, through the sides and very near the base of clay-hills, abounding in gypsum; and it is quite evident that the saline matter over which they flow and from which they derive their impregnation, is subjacent to those minerals which appear as the external crust of Scotland. The water of these springs has not been carefully analyzed; in taste and other qualities they resemble the waters of Cheltenham, and they are occasionally employed to answer the same medicinal purposes.

The several mineral substances enumerated, as forming the hilly district, are composed principally of argil, or of argil and silex, frequently blended with ferruginous or bituminous matter, and they very certainly and obviously rest on the coralline mass, which constitutes the exterior crust of the other and more extensive portion of the island.

Having in the preceding pages given a faithful account of the peculiarities which characterize the two districts, into which nature has divided this little island; we will now turn to the consideration of those causes, secondary to the will of the Creator, which have contributed to the production of the island; and of the distinctive features of each district. In conducting this inquiry, it will be of great importance to keep our minds constantly fixed on the two following statements of facts:—First, the argillaceous minerals are constantly found superimposed on the calcareous:—secondly, the argillaceous minerals appear only on the north-eastern portion of the island, and principally in a deep hollow, protected to the west, north-west, and south-west, by high ridges of coralline structure; and they are every where to be found on the north-eastern coast, extending in a greater or less degree into the body of the island, according to local circumstances. To illustrate the latter part of this statement, I will instance only a single example to be found on the coast, forming Skeet's Bay, on the north-east of the thickets,

where, notwithstanding the land is low, the minerals of Scotland, clay and gypsum, are thrown up against the calcareous cliffs; but immediately round the point, on the south-eastern coast, these minerals are wanting, and the precipitous calcareous cliffs appear bare and undermined by the waters of the ocean.

It cannot be doubted by any naturalist that the calcareous formation, of which the body of this island consists, has originated in the submarine operations of insects belonging to the order of Zoophytes and that the various modifications of carbonate of lime by which the coralline are cemented, have been derived from these substances acted on by water. The island, however, which was once so undeniably under the surface of the ocean now rises considerably above it. To what cause are we to attribute this difference of relative height? Has the land been elevated or have the waters subsided?

It is not my intention to enter into the general discussion, to which this question naturally leads. It will be sufficient to observe with reference to the subject under consideration that although the calcareous formation of this island exhibit various rents and dislocations, some of these have taken place in the memory of man, and they can all be fully accounted for by the effects of earthquakes, inundations, and such like phenomena of nature, without having recourse to so violent a convulsion as the elevation of the island from the bosom of the ocean; and as far as we may be guided by the appearances of the district there is no reason to suppose that such a catastrophe ever took place.

In respect to the unstratified minerals forming the hilly district, their uniformly regular occurrence on the north-eastern side of the island, and their obtaining heights proportioned to the calcareous cliffs situate to the westward, and to which they are opposed, very clearly demonstrate that they have been deposited under the influence of a current setting from a north-east point which, while the deposition was taking place in the protected hollow, and similar situations, would wash freely down the inclined surface of the other parts of the island those loose materials which may accidentally have been accumulated

thereon. That this was not a ground current, I infer; first, because I can perceive in the coralline aggregate no indications of the island having been elevated from the bottom of the sea; and secondly, from the relative position of the calcareous and argillaceous minerals, which satisfies my mind that the latter were deposited in the situation they at present occupy; for it seems to me quite inconsistent to suppose that this island could have been formed at the bottom of the sea, and then elevated to its present altitude, without a complete destruction of all the regularity of relative position of the two formations which is now so strikingly evident. It is also more reasonable to attribute a phœnomenon to a known sufficient cause, than to one which is merely presumed to exist, from its sufficiency to explain the phœnomenon. Now, we have no evidence of the existence of a ground current setting from the north-east, and we have the fullest of a superficial current setting from that point. I would therefore attribute the deposition of the clay and other minerals of the hilly district, in their particular situation, to the influence of a superficial current,—to that superficial current dependent on the north-easterly trade wind, which must have been coëval with the present direction of our terrestrial poles, and which, stopped in its progress by the Isthmus of Darien, is reflected through the gulf of Mexico, and passing between the shores of Florida, the Bahama Bank constitutes the gulf-stream, so powerfully affecting the navigation of the Atlantic; and which would be equally efficient at any altitude of the ocean.

That the earth has at some period been overwhelmed by an universal deluge, during which the waters rose considerably above the highest mountains, is a fact established on the authority of the Mosaic history, and supported by the traditions of the rudest nations, and the observations of enlightened geologists. The mind naturally turns to the period of this stupendous catastrophe, as that at which the mountainous district of Barbadoes was formed; and feels something like certainty on this point, from the geognostic situation of the salt-springs.

The beds of saline matter over which these waters flow, and from which they derive their impregnation, have in all proba-

bility been formed by repeated inundations by the ocean, of what is now the plain of Scotland. Each inundation effected by the concurrent influence of strong trade winds, and spring tides, would form a saline lagoon; and the repeated formation and evaporation of such lagoons would occasion the deposition of salt, or of minerals charged with salt, as well as of gypsum. Now it is evident, that such inundations and evaporations could only take place at a time when the sea stood nearly at its present level, and when the constant occupancy of the ocean was prevented by some natural dam, or barrier; and as the salt minerals appear to have been deposited under the argillaceous, which form the exterior crust of Scotland, it would seem to follow necessarily, that the argillaceous minerals, which reach an altitude of at least eight hundred feet, must have been deposited during a rising of the waters subsequent to the formation of the saline minerals, and of such a rising we have no example, except during the great and universal deluge.

I conclude, therefore, that the coralline structure which constitutes the body of this island, was produced during the subsidence of the primeval waters of the ocean, antecedently to the deluge, and that it rests on primitive or secondary rocks of ancient date; that during the period which intervened between the formation of the coralline mass and the deluge, frequent eruptions of the ocean over its bounds formed saline lagoons, which gave origin to those minerals which impregnate the saline springs; and lastly, that the argillaceous minerals, which form the hilly district, were deposited from the troubled waters of the ocean, when they had risen high above the whole island; that is to say, during the universal deluge.

In opposition to the opinion expressed in the preceding pages, is the hypothesis, which considers Barbadoes and the chain of neighbouring islands to be of volcanic origin.

That islands are occasionally thrown up from the bosom of the ocean, by the action of submarine volcanoes, is most certain; but these islands are so obviously of volcanic formation, and consist so entirely of volcanic materials, that I cannot suppose it possible that their origin, and the origin of Barbadoes, should be con-

sidered as analogous; but it is a theory, I believe, rather prevalent, that these islands have been raised from the bottom of the sea, by a central expansive force of volcanic origin. My opinion on this subject, and the considerations on which it is grounded, in reference to Barbadoes, I have already endeavoured fully to explain; what may be the arguments for or against my way of thinking, presented by the other islands, I am not competent to determine; but I cannot refrain from observing, that a popular argument in favour of the elevation of these islands by volcanic force, drawn from the occurrence of volcanoes in many of them, is entirely without weight; it being certainly one thing for an island, or tract of country, to contain the materials capable of producing a volcano, and another, very different, to have been itself elevated to its present station in the globe by the force of volcanic fire.

It would have been impossible to render the preceding observations intelligible without the assistance of a geological map, which I have therefore endeavoured to furnish. It is intended to illustrate the rise by successive terraces, from the southern, western, and northern coast, to the highest land, which runs in an irregular semicircular direction, marked by the letters *a a a H a a a*, H pointing out the situation of Mount Hillaby, the highest land of the island. It is also intended to shew the deep protected hollow, occupied by *Scotland*, and *below the Cliff*. The object has been to give a general idea of the relative situation and difference of character of the two districts, and much pains have been taken to render the Map, in this respect, minutely correct.

Barbadoes, August 26, 1820.

ART. III. *Account of the Remains of a Mammoth found near Rochester, with some general Observations, connected with the Subject. By* CAPTAIN VETCH, *of the Royal Engineers*, M.G.S.

[Communicated by the author.]

THE remains of the Mammoth, or fossil elephant, being but

of comparatively rare occurrence* in this country, an account of the circumstances under which they occur, must I conceive in every instance be worthy of record; as we can only expect by very extensive observations and comparisons to derive any satisfactory helps, for ascertaining the order of the Geological Epochs connected with the state of existence and extinction of that animal.

When such remains are found in the alluvium of a river within reach of its present floods, a great uncertainty must attach to the time and mode of deposition. The animal may have lived and died on the spot where the remains are found, it may have been brought from a considerable distance by floods in a recent state, or the remains may have been washed out from older strata by the same floods, and carried down in a fossil state to their present site.

When such remains are found considerably under the surface, in a bed of gravel, and situated above the reach of any present tides, we may conclude the animal did not live or die in that spot, but was transported either in a recent or fossil state, by the same action that accumulated the gravel, and when the entire skeleton is found in one spot, we may presume that the animal either died or was conveyed there in a recent state; while, on the contrary, if the fragments are much dispersed, it is to be inferred they are not in their original repository.

With respect to the remains, the more immediate subject of this notice, the first view of the question would appear to countenance the most recent period and mode of deposition, *viz.*, that the animal inhabited this island under the same natural condition as at present, or in fact might have lived and died near the spot where the remains are found so late as 2,000 years ago, and subsequent to the last deluge or debacle, for these remains were found above the influence of any present floods, and only four feet under the surface covered with a

* As the remains of a huge animal of this sort scattered over a considerable extent of ground, and found at various times, may often lead to the belief that they are abundant, when a single individual only occurs, it is necessary to be guarded against such a source of deception.

sandy loam, a substance and depth they might easily have penetrated in that lapse of time by their own gravity, aided by the action of rains and ordinary surface water.

But as the general arguments of the case are against this view of the question, it becomes necessary to investigate the matter more closely. By supposing the race of Mammoths to have existed subsequently to the last debacle, we at once deprive ourselves of the most reasonable mode of accounting for their extinction. If, however, they did so exist in Great Britain, we might have expected they would have maintained their race in spite of the hostility of savages, at least down to the Roman invasion; we might have expected their remains would prove plentiful in alluvial grounds, as in Siberia; but we would not have expected to have found their remains so situated, as to shew that they also existed previous to the last debacle, for in that case we must suppose they were first rooted out of the island by a natural event; that they again colonized the island, and were a second time rooted out by human means.

Neither does there appear to be any strong ground to suppose this island has been subjected to the action of any debacle, since that which accompanied its original formation or emergence from the ocean.

It is therefore necessary to look more minutely into the circumstances, attending the state in which the remains were found, before we adopt the first view of the question.

These remains were found on the west bank of the Medway about two miles and a half south from Rochester Bridge; at a place where a lateral valley meets that, in which the Medway flows at an acute angle pointing down the stream. The point of land separating the two valleys is fundamentally chalk, covered with gravel, sand and loam. On the side of the point of land, towards the lateral valley two well-marked shelves or ledges are seen, indicating the different heights at which the water formerly rested. The perfect level of the surface of these ledges and the regularity and steepness of their talus, combined with their situation and extent, are quite decisive of the mode of their formation. On the lower of these two shelves, and

about sixty feet above high-water mark were found the remains in question, consisting of one upper grinder nearly entire; its fellow in fragments and considerable portions of the bone so extremely decayed, as only to admit of lifting in very small portions; the largest portion I uncovered appeared from its breadth and flatness to belong to the cranium, or lower jaw, the portions of bone were all found together, and as no other remains could be discovered by digging in different places near the spot, there is reason to conclude that a portion of the bones of the head and two teeth were all that were deposited in this place; had bones of other parts of the animal been there, the more definite shape of the fragments would have pointed them out. The teeth were decomposed into laminæ, the osseous part being entirely gone and the enamel only remaining.

A few inches immediately below the remains, was a layer of flints but little water-worn, the teeth were more immediately enveloped in a layer (a few inches thick,) of clean hard sand, such as is generally found in the beds of rivers; over the remains was a bed of two feet of sandy loam; and, lastly, a foot and a half of mould. Among the loam, near the remains I found a shark's tooth of the same colour and appearance as those found in the blue clay of Sheppey.

Among the layer of flints already mentioned, might also be observed some fragments, from the green sand; and strongly adhering to the largest portion of the bone which I uncovered, was a fragment of an indurated clay stratum containing numerous bivalves. From a consideration of all which circumstances, it seems more reasonable to infer that the site where the remains were found, was not their original repository, but that they were washed out from a stratum above the chalk, and that the cranium and teeth were deposited on the ledge at the time of its formation, along with the other travelled matter; indeed the fragment of indurated clay, containing shells, would seem to point out the particular stratum from whence they were derived—the circumstance of the remains being originally deposited in a bed containing shells, offers no difficulty as some

of the strata above the chalk, from containing a most extensive mixture of land and sea remains, notoriously point out that they were formed in the sea at the mouth of some immense river, of which the mud or clay of the Isle of Sheppey may be given as an example; indeed, were the mouths of the Mississippi or Ganges to be laid dry, we might expect to see similar formations. That the chalk hills immediately above the site of the remains, as well as that of the highest portion of the North Downs, were actually denuded of superior strata, the following fact may be taken as a proof:

At one of the highest parts of the ridge of the North Downs where the old road from Rochester to Maidstone crosses it, I observed a fissure in the chalk of various widths from ten to thirty feet, which I traced half a mile in length running in an east and west direction parallel and close to the edge of the chalk; the most remarkable of the contents of the fissure were huge tabular masses of the siliceous sandstone, known by the name of grey withers. These are very numerous and disposed in the most irregular manner possible; the intervals between the blocks are filled with flints not water-worn, and the intervals of the flints are filled chiefly with clay. Now as this occurs on the highest part of the ridge, it is obvious that the materials filling the fissure could be derived only from strata that were superior to, and incumbent on, the present surface of the chalk; some of these tabular masses of sandstone are ten feet in length, others of the same sort but much smaller in dimensions, and partially rounded, are found sparingly scattered about the surface of the chalk, and among the gravel alluvium. The great quantity of this stratum preserved in the fissure, compared with the small quantity found on the surface, and the hard quartzose nature of the rock, demonstrate the force and duration of the current which swept the surface of hills, now 600 feet above high-water mark, and removed extensive strata of great hardness. If such has been the fate of a bed of such seeming durability, the traces of strata of sand or clay cannot be expected to exist, though from the strata we see incumbent on the chalk, in other places it is most probable the same were

continuous over the summit of the North Downs; and though the traces of the strata themselves may be lost, we may nevertheless expect to find occasionally amongst alluvia, the more durable of their organic contents; and in this manner the circumstance of the remains of the Mammoth under notice, being found in their late repository, admits of a solution without opposing the conclusions induced, by more general views of the subject.

Accompanying these observations is a representation of one of the teeth referred to, engraved from a very accurate drawing by Mr. Outram, of the Honourable East India Company's engineers. The tooth consists of twenty-one laminæ, but has evidently lost the most anterior one. The dimensions in inches are as follow:

Laminæ, length of the largest	*8.25
——— total number	21
——— in use	†9 or 10
Length of tooth	17
Length in use	7.5 or 8.25
Depth	7.57
Breadth	3.5

Twenty-four or twenty-five laminæ seem to be the number belonging to a tooth at its maximum size; it is therefore probable the Rochester tooth was past its maximum, and at the defunction of the animal was so far protuded and abrazed, as to have lost three of the laminæ. But as these dimensions are exclusive of any osseous covering to the enamel, it may safely be pronounced to have belonged to one of the largest Mammoths of which remains have yet been found.

No appearance of any portion of the bone of the tooth is to be seen, but its place is supplied by a very fine white earthy substance, chiefly carbonate of lime which is possibly derived

* The length of this lamina is greater than the depth of the tooth from the diagonal direction of its position.

† From the mutilation of the tenth lamina, it is uncertain whether it has been in use or not, and therefore, whether the dimensions in the fifth column should be 7.5 or 8.25.

from the decomposition of the bone; the enamel appears fresh and little altered, is hard and not easily frangible.

As many are unacquainted with the appearance of a Mammoth's tooth in a state of decomposition, the representation of the tooth may be useful in preserving future discoveries; and as it will probably be admitted that few facts are so likely to throw light on geological history, as those connected with the extinction of this animal, it would be desirable that every occurrence of this kind should be well noted and recorded.

Art. IV.—*Observations on the Solar Eclipse, September 7th, 1820, by* J. L. Memes, Esq.

The phænomena of eclipses, as the subject of scientific investigation, may be contemplated under two relations, or as connected with two distinct and separate departments of inquiry,—the motions; and the physical analogies of the planetary bodies. On the occurrence of such an event, therefore, the same diversity necessarily obtains in the observations on its economy, according as their tendency is more exclusively directed to the improvement of astronomy, and its cognate sciences; or to the illustration of those affinities of their organic frame, which from analogy are inferred to pervade the different parts of the system. In the following observations on the late solar eclipse, appearances are attempted to be described, illustrative chiefly of the latter department, as it regards the inequalities of the lunar surface, and the existence of an atmospheric medium.

By calculation, these observations were made in lat. 51° 8′ N. lon. 0° 5′ W., the telescope employed being a small but very excellent reflector, with a power at first of 60, and afterwards of 135. Cassini's account of the transit of Mercury, in 1736, rendered it extremely probable that the supposed atmosphere of the moon might be visible from refraction, if the body of the planet could be discovered whilst moving in space at a distance from the solar disc. A series of experiments, therefore, partly suggested by an incidental remark of D'Isle on this passage in Cassini, was previously undertaken in order to ascertain the

best mode of observation. In consequence of their results, measures were adopted which, in viewing the eclipse, placed the observer in total darkness, the only light admitted from without, passing through the telescope; although by other means it could occasionally be introduced, and also the general appearance of external objects observed. Circumstances, however, did not permit of proving the efficacy of this arrangement, with regard to the primary object in which it originated. The atmosphere, which during the early part of the 7th had been clear and serene, as the morning advanced became gradually overcast by numerous aggregations of loose floating clouds. These continuing to move slowly towards the N.N.W., had nearly disappeared by mid-day, at which time the thermometer stood at 68° in the shade, and soon after twelve a few dense clouds only remained near the zenith towards the S.E. One of these dark masses extending directly over the sun, a few minutes before the expected commencement of the eclipse, completely obstructed the view for the space of 15′. The appulse took place during this interval, and on emerging from the cloud, a dark crescent of the moon's orb was distinctly visible on the sun's disc. This was the last cloud that passed, and throughout the whole duration of the eclipse, the luminaries were not again obscured even for a moment, but a serene and unclouded sky constantly prevailed.

On first viewing the body of the moon, its irregular and in many places deeply serrated circumference was very plainly discernible. By applying a higher power to the telescope, the unequal magnitudes and varied forms of these inequalities were distinctly to be marked, although there was merely a very attenuated circular segment visible. As the dark orb continued to advance, these irregularities became every moment more conspicuous, exhibiting the appearance of alternating eminences and depressions, in every respect similar to the mountains and valleys on the surface of the earth; while from the strong contrast of their shaded outline, their limits and extent could be traced with the greatest precision. Owing to the progressive motion in that direction, these projections on the south-eastern

portion of the lunar circumference produced a very singular and pleasing effect, their summits frequently appearing to impinge, as it were upon the sun, shewing like angular incisions in the margin of his lower limb, before the adjacent and less elevated parts of the moon's periphery had as yet made any impression on his disc. Small and imperfectly angular interstices also were thus formed between the points of appulse in the two luminaries, through which coruscations of the solar rays were seen to dart at intervals, sometimes with a faint purplish light, at others with a dark red or dusky splendour, but without any regularity in the alternations. This distinction, however, was invaribly observed, that the intensity of the colour increased in the inverse ratio of the angle formed by the ray, and the plane of the moon's darkened hemisphere; that is, the more acute the angle at which the ray fell into the penumbra, the greater was the brilliancy of its light. To these appearances reference hereafter will be made; at present, it is sufficient to remark their agreement with what M'Laurin mentions respecting "the light breaking into irregular spots near the point of contact during the formation of the annulus" in the eclipse of 1737; also with Dr. Halley's statement "of flashes of light darting from behind the moon" in that of 1715. And in general with similar appearances described by later observers, although the present observations, as will afterwards be shewn, seem to warrant a different explanation from that which is commonly received.

Of these lunar mountains some appeared to be disposed in connected chains of great extent, which at one extremity generally terminated abruptly, inclining from the perpendicular by a very acute angle; but in the opposite direction their altitude diminished by degrees, gradually subsiding to the common level of the moon's circumference. Another division consisted of mountains apparently solitary and detached, and which seemed to rise suddenly from the surface of extensive plains, with a form almost regularly conical, as far at least as could be judged from the gradual inclination of the sides exposed to view. In these isolated mountains likewise every gradation

of altitude was to be observed, some appearing considerably the most elevated ground in the planet, while others scarcely extended beyond its dark sphericity. Throughout the whole extent of the circumference, and during every period of the eclipse, these appearances were nearly uniform; the mountainous inequalities exhibiting less diversity both in figure and position, than *à priori* would have been inferred.

This irregularity of surface, as it constitutes one of the most striking analogies in the conformation of the earth and its attendant planet, so it has naturally been the subject of considerable discussion, and the existence of lunar mountains is not less generally admitted than their magnitudes have been variously estimated. While they were thus conspicuous therefore, it seemed probable that an attempt to ascertain their elevation, might be made with success. A different method of investigation, however, from any of those hitherto employed, was obviously necessary, and their progress over the margin of the sun's western limb at length suggested, that from the relation between the motion of a planet in its orbit, and the space passed over in a given time, the requisite *data* might be derived. Since the earth and moon revolve about the same centre, and with a common velocity, both, as regards this motion, in the present instance, may be considered as at rest. The former thus becomes a fixed station, whence the observer may view the progress of the latter over the surface of the sun, as she is carried along by the horary motion in her own orbit. Hence it appeared, that if during the eclipse any remarkable point in the body of the sun were assumed, and the number of seconds accurately determined, which elapsed from the apex of a mountain on the moon's circumference coming in contact with this point, to the arrival of the base at the same point; the orbicular velocity of the moon, as compared with the observed time, would give the distance described, that is, the perpendicular height of the mountain. It is plain, that this method could be proceeded on in the case of those mountains only, the whole height of which extended beyond the moon's circumference; and that two points in the solar body might be employed for this purpose,

either the extreme boundary of the periphery, or the line of demarcation of any spot then visible upon the disc; also that the observation might commence from the appulse, either of the summit or of the base of a mountain, according as its situation was on the moon's eastern or western limb.

It soon appeared, however, that only one of these methods could be adopted in the present instance, and that several causes concurred, during the early part of the eclipse, to impede the application of the general principle in question. The observer not being aware of its approach till the mountain had actually appeared, part of its height was passed over before he could commence his observation; from a similar cause, not knowing in what particular point to expect the appulse, the most favourable opportunities were altogether lost; the intensity of the solar light also being very little diminished, necessarily obstructed investigations so minute. For these and other reasons, it seemed most advisable to defer any farther attempt till the eastern limbs of the two luminaries should come in contact. Soon after the central conjunction, two eminences on the moon's eastern circumference were accordingly selected on account of their superior height and convenient situation, one being detached, the other a precipitous termination of a ridge, of which an exact outline is given in Fig. 1, Plate II. As had been anticipated, the former obstructions were now in great measure removed; the observer could trace any particular projection through every part of its course, and note the instant of appulse with the utmost accuracy. Of the two, forming the principal subject of observation, that of which the figure is given arrived at the edge of the solar disc some minutes before the other, and from the contact of the summit, till the base attained the same point exactly 2′ elapsed. In like manner, the second was found to take nearly 2$\frac{1}{2}$″ in passing; 2″ 30‴ were assumed at the time, as a very near approximation to the truth. Before their egress, the appearance of these mountains was extremely beautiful, especially of the higher; on both sides of which, arcs of the sun's surface were visible, becoming gradually narrower, and at length appearing like

two lines of intensely brilliant light, separated by a broad dark space. A short time before the appulse of the lower, bright flashes of light were seen to dart from an opening towards the north; which, by enlightening the adjacent parts, presented an appearance, as if the extremity of the mountainous ridge had been separated from the general mass. In order to convey a more distinct idea of this effect, these luminous streaks are represented in the figure, although they had ceased for some time before the mountain had reached the position, at which it is there supposed to have arrived.

I. In order to ascertain the respective elevations of these mountains, by the method now proposed, let M (Fig. 2, Plate II.) represent the body of the moon, when the apex of the mountain at A has just attained the edge of the solar disc (S), and let its base join the circumference at *m*, hence A*m* is its perpendicular altitude; also let the curve AB equal the moon's horary motion at the time of observation; then the angle ACB will express the time, that is 60′, in which the *radius vector* describes the whole area BAC. It is evident, that when the base *m* has moved to A, the summit will be at *x*, hence during this interval the moon has travelled in her orbit over a space *x*A, equal to A*m* and the angle AC*x*, that is the angle AC*m* is the expression for this time, which, if known, A*m* is found. Thus from the laws of planetary motion,

As $\angle$ ACB : $\angle$AC*m* : : $\triangle$ABC : $\triangle$A*m*C;

But on account of the extreme shortness of the time, the eccentricity of the moon's orbit may be disregarded; and, for the same reason, its curvilinear direction may be projected into a straight line, the areas then become triangles of equal altitudes, hence

As $\triangle$ABC : $\triangle$A*m*C : : AB : A*m*

consequently As $\angle$ACB : $\angle$AC*m* : : AB : A*m*

From this the altitude of the mountain is found in ″ and ‴.

II. To find A*m* in miles, let AM represent the semidiameter of the moon in miles 1090; then the angle ACM expresses the moon's apparent semidiameter, as found for the time of observation and as before, the angle AC*m* is the angle subtended by

A *m* but since the moon's semidiameter, and the height of the mountain, are viewed under the same angular distance, a similar relation subsists between the real and apparent lengths of both; hence

$$\angle AGM : \angle AC\,m :: AM : A\,m$$

Let H = the moon's horary motion, as found for the time of observation, T = 60′ and t = the time observed during the passage of the mountain; also D = the moon's apparent semidiameter for the given day, and d = her semidiameter in miles, and, lastly, y = the elevation required. By substituting the value of y, as found in the former case, there results this general formula $y = \frac{H \times dt}{DT}$ expressing the height of the mountain in feet.

The moon's horary motion on 7th of September, as calculated in the usual way, is found to have been 27′ 1″ 4‴, and her semidiameter 14′ 41′ 2″; hence, by substituting their values, the elevation of the higher and isolated mountain is shewn to have been 7353 feet nearly. In like manner, the height of the other appears to have been 5783 feet.

The elementary reasoning employed in the preceding investigation, has been preferred, from the extreme simplicity of the result thus obtained. This motive may seem to have induced the adopting of principles apparently liable to objection, in as far as no portion of the moon's orbit can, strictly speaking, be considered as rectilinear, nor the motion in that orbit uniform. The effects arising from these causes are, however, so inconsiderable as to be safely omitted; and are accordingly neglected in several cases, embracing both a greater extent of orbicular space, and longer duration of time, than are comprehended in the present observations. To produce a similar instance: Dr. Halley, in describing a method of determining the limits of the penumbra in a solar eclipse, by means of certain properties of the sphere and cone, proposes to consider as straight lines, not only the *axes* of the eclipse formed by the common intersection of the conical shade with the earth's circumference, and extending several hundred geographical miles

over the surface of the latter; but also states, with regard to the moon's orbit, "that so small a portion of the curve as passes over *England* may be regarded as a straight line." "The like," he adds, "may be said of the velocity, which for so *short* a time, (the whole duration of the eclipse), may be considered as equal, without any *sensible* error." The difference arising from curvature, therefore, or from inequality of motion, must be reduced almost to nothing in the extent of a few thousand feet, and during an observation of less than three seconds. For similar reasons, the diurnal rotation of the earth has been likewise disregarded, since, from the nature of the problem, and in a latitude so high as that of London, its effects on the general result would scarce be perceptible. In truth, to have corrected discrepancies so minute, would have rendered the solution more complicated, and merely added an appearance of precision, which the subject, perhaps, does not admit. And in cases of this nature, where an approximation to the truth is all that can reasonably be expected, that method seems most eligible, which, with the due degree of accuracy, combines the greatest plainness of inference, and facility in application.

As there exists considerable diversity of opinion among astronomers, respecting the altitudes of the lunar mountains, it may be requisite to compare those now given with former measurements. Previous to the discoveries of the present age, elevations were assigned to these mountains altogether inconsistent with the conclusions from analogy. Hence appear to have arisen the first doubts of the accuracy of the deductions. These suspicions were well grounded; and the principles employed in the early calculations on this subject, have proved erroneous*. Disregarding former measurements,

* Sir William Herschel has shewn that a quantity on which the whole of the operation, in a great measure, depended, was, by some astronomers, assumed as equal to $\frac{1}{7}$ of the moon's diameter, and by others to $\frac{1}{10}$, by which a difference of 2¼ miles was produced in the measurement of the same mountain. It appears also, that the same method would, at different ages of the moon, give different results.

therefore, if a review be taken of those which have been given in the course of the last forty years, their results will be found to range from $\frac{1}{2}$ to 5 miles, giving a medium elevation of upwards of 2 miles. The altitudes, however, of the two mountains in question, as already stated, fall far short even of the mean, since the higher appears to have extended 1 $\frac{4}{5}$, and the other, 1 $\frac{1}{10}$ miles only in perpendicular elevation. There is this important distinction also, that the two on which these observations were made, appeared to be equal in height to any others visible on the moon's circumference; hence there is no reason to conclude, that any much higher exist on the planet. But instead of taking the mean as the standard elevation, a comparison is rather to be instituted from those which may justly be considered as the most accurate calculations on this subject. During the above period, few observers, if any, have given the same attention to the subject, and certainly no one has brought to its examination more of science or practical skill than Sir William Herschel; who, after a series of observations continued for years, comes to the conclusion, that "when we have excepted a few, the generality of the lunar mountains, does not exceed half a mile in perpendicular elevation." The altitudes of some which were formerly estimated from 8 to 3 miles, in the account of these observations, are stated to extend from $\frac{3}{4}$ to 1$\frac{3}{4}$ miles. The latter is the elevation of *mons sacer*, and is considered by Sir William as much overrated, owing to circumstances which impeded the observations*. With these measurements, the present results nearly agree; and it may, perhaps, be regarded as a presumption in favour of their accuracy, that they thus coincide with statements of such high authority, although deduced from principles so totally different.

When these calculations, likewise, are compared with the results which analogy furnishes, their agreement is almost equally satisfactory. At the surface of the moon, gravity being diminished nearly one-third, its mountains are probably somewhat more elevated in proportion than those of the earth.

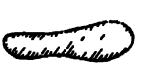
* Phil. Trans.

And in the present case, the difference is not greater than this effect alone would seem to warrant. Since the most elevated mountains on the earth scarce extend above 20,000 feet, and the proportional diameters of the two bodies being 11 and 3 nearly, the highest of the lunar mountains should not exceed the dimensions now given, allowing for the effects of gravity and other *known* causes.

In measuring lunar elevations, by the method now proposed, notwithstanding its simplicity, certain precautions will be found requisite. Great attention is obviously necessary in selecting for observation, such projections only as rise immediately from the moon's circumference, and whose whole height, consequently, is exposed on the solar disc. This may be ascertained from the manner in which the base appears to unite with the periphery, for if it fall on either side, the mountain will present a truncated form, with defined angles at the points of apparent junction; on the contrary, where the whole elevation is projected from the circumference, the base seems gradually to expand, and to blend imperceptibly with the general mass. Such, at least, were the appearances which usually accompanied certain degrees of elevation in the instance before us, except in parts where the moon's edge seemed so marked with slight undulations only. In the terminal elevations of ridges, from their being, for the most part, abrupt, and precipitous, these characteristics were by no means so conspicuous. This circumstance, however, was pretty generally observed, that where a ridge of any considerable altitude terminated, that portion of the circumference, which lay contiguous to its base, presented, for a considerable extent, the appearance of a smooth, unbroken surface, seeming to indicate that such a ridge was surrounded by a plain, and that its entire height was visible.

With regard to the position of the mountain, that is the most convenient, where the direction of gravity at its summit approaches nearest to parallelism, with the line described by the path of the moon's centre. Attention, therefore, was principally directed in the present case to such inequalities as were situate near the diameter lying in this path. In proportion as

the angular distance of the mountain and this diameter is increased, the former will occupy a longer time in passing over any particular point, and a proportionally greater quantity than the real altitude, consequently, obtained. The angular position may be ascertained with sufficient accuracy by the eye, especially if assisted by cross wires in the focus of the telescope, and the proper corrections accordingly made. Thus, if CD, (Fig. 3.), the diameter, represent also the direction of the moon's motion, and A the mountain to be measured; AB being the direction of gravity, A *b* is the altitude sought. But instead of moving parallel to A *b*, the point of contact in the sun's margin appears to move along AE parallel to CD. The moon, therefore, will in reality have travelled in her orbit a space equal to A e, greater than A *b*, the true altitude as radius: cos. of the mountain's angular position. Hence, when the direction of gravity is perpendicular to that of motion, the altitude of the mountain cannot be ascertained; for its summit may then move along the margin of the sun's disc, so as not to cross it for many seconds. This consideration obviates an objection, which might have been raised against the accuracy of the above conclusions, from Dr. Halley's observations on the total eclipse of 1715, where he states, that, "For the space of a *quarter of a minute*, a small piece of the southern horn of the eclipse seemed to be cut off from the rest by a good interval, and appeared like an oblong star, rounded at both ends, in this form,



which appearance could proceed from no other cause but the inequalities of the moon's surface, there being some elevated parts thereof near the moon's southern pole, by whose interposition that exceedingly fine filament of light was intercepted*." Provided, however, the direction of the perpendicular forms a

* Phil. Trans. vol. xxix, p. 248.

sufficient angle with that of motion, there is no cause to prevent the application of the present method to the measurement of mountains in any position. Indeed, from some observations made towards the end of the eclipse, but which a derangement of the pendulum rendered too inaccurate to be detailed, it appeared, that even superior advantage would be obtained from observing such mountains as lay considerably removed from the line of motions, as will be seen from inspecting the figure. For, whilst the edge of the solar disc has seemed to move along either of the sides of the mountain, the moon has passed over the respective distances A e and *g c*. By noting the time, then, in which *g c* as the largest, is described, and drawing *d c* B in the direction of the perpendicular, the true altitude may easily be found, while the observer enjoys the same advantage as if the mountain subtended an angle of nearly thrice its real elevation.

Hitherto the point of contact has been supposed to occur in the circumference of the solar disc, it is plain, however, that when there are spots on the surface, sufficiently conspicuous, a point in their extreme boundary may be employed with greater ease and success, both as respects the obstructions arising from the circular form of the marginal limit, and those occasioned by the difficulty of ascertaining the exact moment of appulse. For during the time of observation, it was found that the dark summit of the mountain mingling with the almost equally dark atmosphere, immediately on egress became invisible; hence the utmost care and circumspection were frequently inadequate to mark with sufficient precision, the instant of contact, before any part had yet crossed. Had circumstances, on the contrary, permitted observations to be made from the line bounding a solar spot, the matter would have been comparatively easy. The effect of contrast being scarcely impaired, the outline of the mountain would be as apparent when moving over the slighter shade of the spot, as when opposed to the superior brilliancy of the unclouded surface, and hence its progress would be easily traced. This method presents likewise the important advantage of enabling the observer to obtain two measurements

of the same mountain: the elevation of which, may thus be determined to a high degree of accuracy.

The uniformity of appearance, exhibited by the lunar mountains, has been already noticed. This seems to have arisen from the operations of two causes—similarity in the grouping, and diversity in the angular position of this "mountain scenery." Generally speaking, it would appear that few detached or solitary elevations of any magnitude exist on the moon's surface; but on the contrary, that the mountains are arranged in extensive chains, running in different directions, and separated from each other by large tracts of comparatively low and level ground. On this supposition the appearances already described can easily be explained as optical effects arising from variety of position with regard to the eye of the observer. The appearance of ridges, precipitous at one extremity and sloping gradually towards the other, would be exhibited by those chains which had a direction nearly parallel to the circumference, and which rising immediately from it, at one end, exposed to view the whole, or nearly the whole, elevation of the terminating mountains. By a small inclination from this direction, they would recede towards the interior of the disc either by crossing the periphery, or advancing forwards on the side presented to the spectator; and in both cases, the elevation would appear gradually to subside, from the spherical surface rising above their summits. In like manner those chains which crossed the circumference nearly at right angles, and placed therefore in the line of vision, would present the appearance of one lofty eminence, towering from a base situate directly on the extreme margin of the disc. This seems to account for the superior altitude of these apparently isolated mountains; a circumstance which otherwise is difficult to explain. About 3″ after the central conjunction, that is, about $1^h\ 51'\ 40''$ nearly, an appearance was observed which seems further to confirm this hypothesis. Fig. 4. From the point where the western limbs of the sun and moon appeared to cross, a broad stream of pale reddish light, was seen to dart from the moon's circumference, and extending along that part which was not upon the solar disc, illumined with a mild steady lustre, a considerable

portion of the surrounding surface. It was not, however, one uniform diffusion of light, but divided into several streams of unequal extent and brightness; separated apparently by inequalities, running in nearly continuous ridges. Towards the inner extremities of the enlightened portion, the dark summits of these ridges rising above the surrounding splendour, presented to the eye, irregular, or rather undulating, lines of shade. Along the circumference, on the contrary, where the light was more powerful, the tops of these mountainous inequalities appeared like dotted lines of very brilliant spots, but no where did they shew as insulated or scattered points. This appearance continued for upwards of half a minute, and seemed gradually to decrease, apparently at one time shooting nearly at right angles from the body of the moon towards the west, as if deflected from the spherical surface. This, however, not being well ascertained, is not represented in the annexed sketch.

A very singular circumstance connected with these mountains, is, that during the former part of the eclipse, they were most conspicuous on the lower or western portion of the lunar circumference; but from a few seconds after the central conjunction they continued to be most remarkable on the eastern or upper circumference, to the end of the eclipse. At the time of greatest obscuration they were rather indistinct, the outline appearing to melt into the surrounding light; but this was of so short duration as to be scarcely more than just perceptible.

At the conclusion of this phenomenon, when the moon was just on the point of receding from the sun, no elongation of their margins, as mentioned in other instances, was observed, but the circumference of the moon was less distinctly marked, and for a few moments the motion seemed to be suspended, the moon appearing as it were to adhere to the surface of the sun; one small triangular portion at length only remained, which disappeared, not gradually, but at once, the light bursting from between as if forcing the luminaries apart.

ART. V. *Account of a Coloured Circle surrounding the Zenith. By* Mr. THOMAS TAYLOR, Jun. *In a Letter to* JOHN POND, Esq., *Astronomer Royal.*

SIR, *Greenwich, February* 28, 1821.

I trust you will pardon the freedom I take of informing you, that I was favoured this morning with the sight of a beautiful phenomenon; and as it appears to be of an extraordinary kind, I beg leave to give you a short statement thereof.

About twenty minutes before nine o'clock (the sky being rather overcast), a circular ring appeared, extending itself nearly three quarters around the zenith as a centre, at the distance of 30°, which exhibited the colours of a rainbow, but far more brilliant than I had ever witnessed in that phenomenon; and I plainly saw they followed the order of the exterior bow. The sun at that time (I found by the globe) was about 16° high, and that part of the arc appeared most brilliant that was nearest to, and immediately above, the sun. I called my father, but before he came the colours grew very faint, and it was diminished to nearly a semicircle. The time of its duration after I first saw it till it disappeared, was about five minutes.

I remain, Sir, with due respect,

Your obedient humble servant,

T. G. TAYLOR, Jun.

ART. VI. *Some additional Observations relating to the Secreting Power of Animals, in a Letter addressed to the Editor of the Journal of the Royal Institution, by* A. P. W. PHILIP, M.D., F.R.S.E., &c.

SIR, *London, January* 18, 1821.

As the question respecting the nature of secretion is intimately connected with the healing art, it must be considered an important one. This I hope will appear to you a sufficient excuse for my troubling you with some additional observations on this subject. My present observations will occupy but a small space.

A paper appeared in the last number of the *Journal of the Royal Institution*, in which Dr. Alison professes to reply to some observations of mine which you did me the honour to publish in the eighteenth number of that Journal.

With respect to the preliminary matter of Dr. Alison's paper, if he will take the trouble to recur to my observations, he will find, that I did not object to the opinion, which he there restates, on account of its novelty; but because, while he allows that the nervous influence in increasing muscular contraction only stimulates the muscles of voluntary motion, he supposes it to increase the contractile power of the muscles of involuntary motion, without pointing out any sufficient grounds for thus referring similar phenomena to different causes. To this objection Dr. Alison makes no reply.

I am a good deal surprised at Dr. Alison's objection to the term nervous influence; for it cannot surely be denied, that the brain and spinal marrow possess a certain influence over other parts. I have always avoided the use of the term nervous fluid, that my words might convey nothing more than a simple expression of the fact. It has never appeared to me, that we have proof of either the nervous influence or galvanism being a substance of any kind. What idea Dr. Alison attaches to the term nervous influence I do not know; I never attached to it, nor does it appear to me possible to attach to it, any other than that here stated. The following sentence of Dr. Alison, is to me wholly unintelligible: "First, let it be made clear that there is such an existence in nature as this nervous influence, and then I will admit the obligation." Does Dr. Alison here mean that there is no proof of the brain and spinal marrow influencing other parts of the animal body? I cannot suppose that this is his meaning, for he speaks of the brain stimulating some muscles and giving power to others. If this be not his meaning, what other can his words convey?

Dr. Alison confesses (p. 277) that he had not sufficiently adverted to some of my statements. He again, in his present paper, misconceives me to a degree, which I am sure, if he refer to my Treatise (p. 157, 2d. Ed. and other passages) will sur-

prise himself. What he, with great justice, calls "rather a strained hypothesis" (p. 278), namely, that the fluid secreted in the stomach after the division of the eighth pair of nerves in the neck, is the effect of the nervous influence which remains in the lower portion of the divided nerves, and which he ascribes to me, is wholly his own, and altogether incompatible with the facts I adduce. I may add, with respect to what he says of my opinion of the action of sedatives, that he will find it observed in the 82d. page of my Treatise, that "we always, for we frequently repeated the experiment, saw an evident increase in the action of the heart when we washed off the tobacco."

Dr. Alison, in his former paper, stated, that my inference respecting secretion depending on the nervous power, is in all respects similar to M. le Gallois' inference, respecting the dependence of the heart on that power; and now that I have reminded him that there is this difference between the two cases, that the heart retains all its power after it is separated from the brain and spinal marrow, while the secreting organs wholly lose theirs, he replies, that although the heart had been incapable of its function when in any way deprived of the influence of the brain and spinal marrow, he would still have considered M. le Gallois' opinion as erroneous; because we might still have ascribed its loss of function to the means used for separating it from those organs. On the same principle we cannot be sure that the feeling of a limb depends on its connexion with the brain, because when we divide its nerves or in any other way intercept its communication with the brain, we are not sure that the loss of sensation may not arise from the injury done to the limb by the means we use for this purpose.

Dr. Alison forgets, that, of possible opinions respecting the cause of any phenomena, some of which must be true, that which is most consistent with the other phenomena relating to the subject, must necessarily be admitted. The reply in the case of sensation is; we see the sensation of every part influenced by the state of the brain; if we excite this organ, sensation is every where acute; if we oppress it, sensation is every where benumbed; and when we find it impossible to intercept

the communication between the brain and the limb, without destroying sensation in the latter, the inference is unavoidable, that it is lost in consequence of that communication being cut off. Our inference in the other instance is precisely of the same kind. We see the power of secreting organs under the influence of the brain and spinal marrow, and when we find it impossible to intercept the communication of these organs with the brain and spinal marrow without destroying their powers, a similar inference is unavoidable; and would have been so respecting the heart, were its power destroyed by every means of intercepting the communication between it and the brain and spinal marrow.

Independently of this mode of reasoning, which, as far as I am capable of judging, must be regarded as conclusive, there are other proofs of the power of secreting organs depending on the nervous system, which in my last paper I recapitulated in the latter part of the following short paragraph, and to which Dr. Alison makes no attempt to reply.

"The question before us is, when the function of a secreting surface is deranged by dividing its nerves, is this to be ascribed to its being deprived of its nervous influence, or to its being injured by the act of dividing its nerves? We know that it arises from the former, because when it is deprived of its nervous influence by any other means, the effect is the same; because the effect is not at all proportioned to the degree of injury done to the nerves, but to the degree in which the nervous influence is withdrawn; and because as soon as the nervous influence is restored, the part is again capable of its functions."

With respect to monstrous cases again referred to by Dr. Alison, he admits the force of what I said respecting cases in which the functions of the brain continue after its appearance is so changed that, were it not for the situation in which we find it, we could not recognise it; but if we see an organ so changed by disease still capable of its functions, it need not surprise us, that nature should sometimes give to it originally such a conformation, that we look in vain for any trace of the usual appearances.

If, as I formerly said, these cases prove any thing, they prove

too much for Dr. Alison's purpose. He admits that the function of respiration necessarily implies the presence of the sensorial power (p. 279), so that in some of the monsters to which he refers sensorial power existed, although there was nothing which deserved the name of either brain or spinal marrow. Here it is evident that there was some part substituted for those organs capable of the sensorial function. But who would infer from such cases, that the sensorial function in the perfect animal has no dependence on the brain and spinal marrow. Now Dr. Alison forgets, that the nervous power is as essential to respiration as the sensorial. These monsters, therefore, possessed the nervous as well as sensorial power, howsoever unusual in appearance or situation might be the organs on which they depended. Thus all argument against the nervous power being necessary to secretion, derived from such cases, is silenced. It is evident, that in them both sensorial and nervous power existed, and consequently that some part performed the functions of brain and spinal marrow.

Dr. Alison, forgetting the cases in which respiration was performed without either brain or spinal marrow, of which he gives a detail in the following page, observes in page 279, in commenting on the case detailed by Mr. Laurence: "As this child had breathed, I agree most fully with Dr. Philip's conclusion, that it must have performed certain mental acts, and, in delivering lectures on Physiology, I have quoted this fact along with others, as proving, that the mental acts concerned in respiration are not necessarily connected with more than a very small portion of the base of the brain, probably of the medula oblongata; perhaps not even with that." Let Dr. Alison consider how much these observations must be modified when he takes into the account the cases which he gives us in the next page. Let him also consider to what conclusions his mode of reasoning leads. He ought, for example, on the same principle to teach, that in the perfect animal the nerves of the intercostal muscles and diaphragm are independent of the spinal marrow, because, in the cases here referred to, these muscles were excited by the will where no spinal marrow existed.

ART. VII. *Observations on the Effect of dividing the Eighth Pair of Nerves—communicated in a Letter to the Editor of the Quarterly Journal of the Royal Institution, by* CHARLES HASTINGS, M. D., *Physician to the Worcester Infirmary, &c.*

SIR,

THE division of the eighth pair of nerves is one of the oldest physiological experiments; and a reference to medical writings shews, that the effects produced by it on the animal system have been the subject of frequent discussion. Among our contemporaries, especially, it has excited considerable interest; and the apparent connexion of the most important vital functions with these nerves, has given birth to various speculations. Of these it is not my intention to give any detail. The object I have in view being to bring before the reader some facts, shewing the dependance of the digestive power of the stomach on these nerves.

My attention has been more particularly directed to this subject by a writer who has recently occupied several pages of your Journal, in endeavouring to prove, that the division of the eighth pair of nerves is not necessarily followed by an immediate cessation of digestion; but, on the contrary, that digestion continues after the division of these nerves, so long as the animal is otherwise in a condition to digest *. The above conclusion Mr. Broughton derives from a series of experiments, and declares, that, from a general review of the testimony of former authorities, he cannot perceive that the conclusion to which his experiments have brought him essentially differs from past experience; though it is absolutely at variance, in a most important point, with that of Dr. Wilson Philip and his supporters †.

It is somewhat singular that Mr. Broughton, after having so carefully studied, as he seems to have done, the testimony of

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* See *Journal of Science and the Arts*, No. XX. page 308.

† Ibid. page 310.

former authors, should not be able to perceive that his conclusion is absolutely at variance with that of several physiologists who have divided the eighth pair of nerves. Even Willis, who performed this experiment principally with a view of ascertaining its effects on the action of the heart, seems in part to have attributed death to the state of the stomach. Baglivi thinks that the animals submitted to it sometimes die of inanition; and Valsalva remarks the frequent efforts to vomit, and the derangement of the digestive organs. Haller mentions the dyspnœa, which succeeds the division of the nerves; but the gastric symptoms seem more particularly to have attracted his attention. In each of his experiments he expressly states, that the digestive powers were completely annihilated, and that the contents of the stomach became putrid. Blainville confirms Haller's experiments, and considers the principal cause of death to be the abolition of the digestive powers.

Dr. Haighton, in his inquiry relative to the re-production of nerves, had a good opportunity of observing the effects of wholly and partially withdrawing the influence of the eighth pair of nerves from the stomach. He states, that in those experiments, in which he divided both of these nerves at the same time, their action being suspended, those vital organs which receive their nervous energy from this source, had their functions arrested, so that death followed as a necessary consequence. But when he allowed an interval of six weeks to elapse between the division of the two nerves, the functions of the stomach were deranged, not arrested. "The actions of the stomach," says he, "were for a long time evidently deranged, so that the dog was continually harassed with symptoms of indigestion, and six months had nearly elapsed before he recovered his health, though during five months of the time he took his usual quantity of food. Now to what cause are we to impute his recovery? The most probable one appears to be, that in the interval of six weeks the first nerve had been reproduced; so that the action of those organs depending on this nerve, though somewhat disturbed, were not suspended. But, as the union of the second nerve advanced, and the re-produc-

tion of the first became more perfect, the vital organs gradually recovered their healthy state."

Dr. Macdonald, in his inaugural dissertation, *De Ciborum Concoctione*, after relating various experiments, in which he observed digestion in the healthy stomach, details the appearances that were presented to him after the division of the eighth pair of nerves. He says, that although the meat which he gave to the animals was cut into very small portions, so as to be in the most favourable state for digestion, and a sufficient space of time was allowed to elapse between the performance of the experiments and the death of the animals, yet the meat was undigested, and never passed beyond the pylorus; neither could any chyme, or chyle, ever be discovered in the stomach, intestines, or lacteal vessels.

Moreover in Mr. Brodie's experiments, after the food had continued in the stomachs of animals whose nerves had been divided seven hours, the food had still the appearance of masticated parsley*. And in those of Dr. Clarke Abel, to which Mr. Broughton has made no allusion, it was found, that in those rabbits in which the nerves were divided, and galvanism was not applied, the stomach was greatly distended: when slit open from the pylorus to the cardia, it disclosed a continuous mass of masticated parsley, of a dark green colour, and of its natural odour †.

From the above statement it is evident, that Mr. Broughton's conclusion is not only absolutely at variance with the experience of Dr. Wilson Philip, but also with that of the authors quoted. It is also, as it appears to me, absolutely at variance with the testimony of Le Gallois; although an opposite opinion is held by Mr. Broughton. I do not find that Le Gallois any where denies that the functions of the stomach are greatly disturbed by the division of the nerves in the neck. On the contrary, his experiments seem to confirm those authorities which mention the suspension of the digestive functions. Nei-

* See the Correspondence between Dr. Philip and Mr. Brodie.

† *Medical and Physical Journal*, No. cciv. page 388.

ther does he any where attribute the state of the stomach to the disturbance of the functions of the respiratory organs; indeed, he declares, that the stomach is sometimes even more affected than the lungs. " L'affection de l'estomac est en général beaucoup plus grave que celle du cœur, car les fonctions du premier de ces organes éprovent un dérangement beaucoup plus grand que celles du second. Je pense même que dans certains cas, de toutes les fonctions lésées par la section de la paire vague, celles de l'estomac le sont au plus haut degré*."

It is true, that he believes the contents of the stomach never acquire any peculiar putridity; of which he was satisfied by repeatedly examining the milk in the stomachs of young rabbits. He does not, however, hence infer, that there is not a cessation of digestion, but that those authors are mistaken who consider the corruption of the contents of the stomach the cause of death. " L'affection de l'estomac est en général plus grave. Elle l'est à différens degrés, suivant les espèces, et même suivant les individus dans la même espèce. Mais on ne trouve dans ce viscère aucun état pathologique bien prononcé, si ce n'est quelquefois un léger état de phlogose. Il ne paroît pas que les alimens qu'il contient acquièrent aucune corruption particulière; et lors même que cela auroit lieu, il est fort douteux que cette corruption, non plus que l'abolition entière des fonctions de l'estomac, put être la cause immédiate de la mort. En un mot, la mort survient à une époque et avec un appareil de symptômes qui ne permettent pas d'en placer la cause dans l'estomac†."

In fact, the object of Le Gallois evidently is to prove, that the suspension of the functions of the stomach is not the cause of death (both he and Dr. Wilson Philip regard the affection of the lungs as the cause of death): but he never asserts that such a suspension does not really occur. Nay, he expressly states, that although young guinea pigs do not survive the division of both nerves a sufficient length of time to afford an opportunity of

* *Expérience sur le Principe de la Vie*, page 214.

† Ibid. page 233.

ascertaining the state of the stomach, yet, from the effects produced by the division of one nerve, there can be no doubt that digestion altogether ceases when both are divided.

"Il est clair que dans cette expérience l'estomac avoit entièrement perdu la faculté de digérer et celle de pousser les alimens dans les intestins. Cet effet n'a pas toujours lieu après la section d'un seul nerf, mais on ne peut guère douter que la section des deux nerfs ne le produisse constamment, sur-tout quand on considère combien, dans ce dernier cas, les cochons d'Inde sont tourmentés par les nausées et les efforts pour vomir. Or, après la section des deux nerfs, les cochons d'Inde de l'âge de celui dont il est ici question, périssent dans l'espace de trois ou quatre heures, et quelquefois plus promptement encore*."

He afterwards observes, that although the digestive powers are in these cases completely destroyed, it is not at all fair to conclude that this is the cause of death in the guinea-pig, and much less so in the rabbit; in which animal the gastric symptoms are less severe. Is this maintaining that digestion goes on after the division of the nerves? Is this denying, as Mr. Broughton asserts Le Gallois does, the occurrence of the loss of power in the stomach to digest food after the division of the eighth pair of nerves? Mr. Broughton says, that *many* authors deny this effect, but does not mention the names of any of these numerous writers, and I have, in vain, searched for them. Several authors, no doubt, who have divided the nerves, have confined their observations to the effect produced on the voice, the heart, or the lungs, without noticing the state of the stomach: but none of those, so far as my knowledge extends, who have directed their attention to this point, deny the suspension of the functions of the stomach; and I am, therefore, led to conclude, that the testimony of the authors, to whom Mr. Broughton alludes, would, on inquiry, be found as inimical to his views as that of Le Gallois.

Having shewn that Mr. Broughton is not, as he supposes, sup-

* *Expérience sur le Principe de la Vie*, page 216.

ported by previous authority, I shall beg, as succinctly as possible, to detail the result of my own experience; premising, however, a few observations on the changes which, in given periods, the food undergoes in the stomach of healthy animals, in order that the misapplication of the term *digestion*, into which Mr. Broughton has fallen, may not mislead his readers.

If the stomach of a rabbit be examined immediately after it has eaten, the new food is never found mixed with the old; and the only change in its appearance is that which is produced by mastication, and the admixtures of those fluids which may be met with in the stomach. The degree of moisture, therefore, at this period very much depends on the kind of food that has been eaten. If, on the contrary, the animal be allowed to live four or five hours after a meal, the food last taken into the stomach is found considerably altered. It is still, however, retained in the cardiac portion of the stomach, but is much softer, from the greater abundance of the secreted fluids. Nevertheless, the centre of the new food is still only slightly changed. If a still longer period be allowed to elapse between the last meal and the death of the animal, that is from twelve to eighteen hours, the change in the food last taken will be found nearly complete. The whole of the contents of the cardiac portion of the stomach, which are at this time much less than immediately after a meal, are now in a pulpy semi-fluid state, frequently containing the small round balls which have been particularly described by Sir Everard Home and Dr. Wilson Philip. The food, whether the animal have, or have not, lately eaten, is drier in the pyloric portion of the stomach, and a distinct line of separation may generally be drawn between the cardiac and pyloric portions.

The state of the duodenum, gall bladder, and lacteals, also varies, according as the animal has been killed, soon after a meal, or after a long fast. If the animal be killed at no great length of time after a meal, the duodenum is found to contain much chyme, the lacteals are filled with chyle, and the gall-bladder is flaccid; but, if eighteen or twenty hours be allowed to elapse between the last meal and the death of the animal, the duodenum is found nearly empty, no chyle is seen in the

lacteals, and the gall-bladder is much distended. Of the accuracy of the above statement, which is supported by the testimony of other writers, I am assured, by repeated experiments on dogs and rabbits. A few of these, in order that the healthy appearances of the stomach may be more directly contrasted with those presented after the division of the eighth pair of nerves, shall be here related.

Experiment 1.

A rabbit was kept without food for several hours. It then ate very heartily of cabbage leaves. For twelve hours afterwards it was not allowed to take any food, and was then killed.

The contents of the cardiac portion of the stomach were quite pulpy, and contained many round balls. There was nothing at all like cabbage, the whole appeared entirely digested. The food in the pyloric portion was much drier. The duodenum was nearly empty, but contained some little chyme and bile. The gall-bladder was distended. There was no chyle in the lacteals.

Experiment 2.

After a fast of eighteen hours a full grown rabbit was killed. On opening the stomach, the contents of its cardiac portion were found in a semi-fluid state. There were many round balls. The food contained in the pyloric portion was much drier, and rather more digested. There was no chyme in the duodenum. The gall-bladder was distended. The lacteals were empty.

Experiment 3.

I gave a dog six ounces and a half of raw mutton, and in four hours and a half afterwards had him killed.

When the stomach was opened, three ounces and seven drachms of a thick fluid, somewhat like strong broth, were found in it. There was also some mucus, and a small quantity of yellow matter resembling bile, adhering to the pylorus. The thoracic duct, and the lacteal vessels, near the duodenum, were distended with chyle. The gall-bladder was not distended.

Experiment 4.

I killed a dog three hours after having given him seven ounces of raw mutton.

The stomach was not much distended. Near the pylorus was a yellow matter resembling bile. There was a mass of meat in the stomach considerably changed in its appearance, and some thick fluid, not unlike broth, which altogether weighed four ounces. The duodenum was rather full. The lacteals in the middle of the mesentery carried some chyle. The gall-bladder was rather flaccid.

A few experiments, shewing, that no changes in the food, at all similar to those above detailed, ever take place after the eighth pair of nerves are divided in the neck, may now be laid before the reader.

Experiment 5.

I took two rabbits, of the same age and size, and kept them without food for several hours. I then allowed them to eat some cabbage, taking care to give each the same quantity. Immediately afterwards I cut out a portion of the nerve of the eighth pair on each side of the neck of one of them. The breathing soon became affected. I gave each of them some parsley an hour after the nerves were divided. Soon afterwards the animal which had been operated on made ineffectual efforts to vomit. The breathing soon became much worse, the animal gasped much, and died in eleven hours after the operation. The other rabbit was then killed.

On examining the rabbit in which the nerves were divided, the lungs were found dark in patches, and the bronchia were loaded with mucus. The œsophagus was greatly distended with parsley. The stomach was very large. The cardiac portion of the stomach was very full of a greenish matter, which looked precisely as cabbage does which is contained in the stomach of a rabbit immediately after a meal. On looking over the contents, small portions of cabbage were very evident. Those parts near the surface of the stomach were browner, but were not at all

more digested. There was no chyle in the lacteals. The gall-bladder was distended. The contents of the stomach weighed two ounces and half a drachm.

The stomach of the rabbit which had not been operated on was much smaller. The contents of the cardiac portion were in a pulpy, semi-fluid state, and there were a number of round balls. No one could have distinguished that what was contained in the stomach had once been cabbage, it had lost all the external characters of vegetable substance. The contents of the pyloric portion were much drier. The duodenum contained some chyme. The gall-bladder was full. No chyle could be seen in the lacteals. The contents of the stomach weighed one ounce and a drachm.

Experiment 6.

I fed two rabbits, of the same age and size, with equal quantities of parsley, and immediately afterwards divided, in one of them, the nerve of the eighth pair on each side of the neck. The rabbit operated on immediately made a croaking noise in respiration. In about a quarter of an hour after the nerves had been divided, I gave each of them a small quantity of parsley. They both ate, but the rabbit which had been the subject of the experiment made frequent ineffectual efforts to vomit. The difficulty of breathing became very great immediately afterwards. Each animal was now kept without food. The rabbit survived the division of the nerves eighteen hours. The healthy animal was then killed.

The stomach of the rabbit, whose nerves had been divided, appeared large. On opening it, the food did not seem to have undergone any other change than that which would be effected by mastication, by moisture, and by lying in so high a temperature for such a number of hours. The bits of parsley were quite visible, and the only difference that I could distinguish between what was found in the stomach and chopped parsley was, that the layer of the former, which had been lying next the surface of the stomach, had lost, in some degree, its green colour, having become somewhat brown. The whole of the

contents of the stomach were covered with a mucous semi-fluid secretion. The œsophagus was full of parsley. Contents of the stomach weighed 2½ oz. The bronchia were filled with mucus.

The stomach of the other rabbit appeared smaller. On opening it, the contents of the cardiac portion were pulpy, and completely altered from the state they were in when taken. There was not the least resemblance to parsley remaining to the eye, but a faint smell of parsley was distinguished. The contents of the pyloric portion were much drier and perfectly digested.

Experiment 7.

After some hours' fast, I fed a rabbit with parsley, and at half-past three divided the nerve of the eighth pair on the right side of the neck. Very soon afterwards, the animal ate some parsley No attempts, however, to vomit came on till half-past five. These efforts to vomit were immediately followed by dyspnœa, which had not before been observed At eight o'clock, the vomiting still continued, at intervals, with some difficulty of breathing. The animal, however, passed the night very comfortably without vomiting or dyspnœa.

At nine o'clock on the following morning, after eating some parsley, the animal again made efforts to vomit, and the difficulty of breathing followed. Each of these symptoms went off after the rabbit had remained for a short time without food.

At twelve o'clock the animal again ate, but did not make efforts to vomit, and the respiration was not disturbed.

At three o'clock the animal ate a good deal of parsley It immediately appeared uncomfortable, and in ten minutes afterwards made efforts to vomit, and the breathing became disturbed. Throughout the remainder of the day, the rabbit would not again eat. It appeared uncomfortable, but did not make efforts to vomit, neither was there perceptible difficulty of breathing.

Early on the following morning, it still seemed very ill, and

would not eat. A little before twelve o'clock, it took a bit of parsley and died immediately afterwards, having survived the operation forty-four hours. The stomach was much larger than usual. It contained some flatus. The food in the stomach did not differ much from the masticated parsley found in the stomach of a rabbit soon after a meal, except that the colour was browner. In some parts, however, partial digestion had taken place. Immediately above the cardia, the œsophagus contained some masticated parsley, but there was none higher up. The contents of the stomach had quite the smell of parsley. The duodenum was filled with food, which had passed from the stomach undigested. The contents of the stomach weighed four ounces.

The trachea and bronchia contained mucus, though not nearly so much as when both nerves are divided; and the membrane lining these tubes was red. The lungs collapsed when the thorax was opened. There were several dark-coloured patches on the lungs.

Experiment 8.

I kept a dog without food for forty hours, and then gave him seven ounces of chopped beef. The nerve of the eighth pair was then divided on each side of the neck. Food was offered him soon after the operation, but he refused to eat, and appeared uneasy. In twenty minutes he made ineffectual efforts to vomit. In half an hour he was very restless, and continued so for an hour, when he became quieter, but had a slight tremor. In three hours after the operation, the trembling was much more severe, and the breathing also became distressed. At the end of four hours the animal was killed.

The stomach was found much distended. The food contained in it, which resembled boiled meat, weighed nine ounces. Some mucus was also found. The duodenum also contained some mucus, but no chyme. The gall-bladder was distended.

Experiment 9.

I divided the nerve of the eighth pair in a dog, on each side

of the neck, after a fast of twenty hours; and immediately afterwards gave it four ounces of meat cut into pieces.

Soon after eating, the animal was restless and vomited; and the breathing soon became affected. He was killed in three hours after the operation.

The stomach was much distended, and contained a considerable quantity of gas. The mass of meat was not dissolved. The colour of the exterior part was altered; that of the interior scarcely at all so. The contents weighed four ounces and seven drachms. There was a quantity of mucus in the stomach. The duodenum contained some mucus and some bile, but no chyme. There was no chyle in the lacteals. The gall-bladder was distended.

From the above experiments it appears; 1. That, during life we have symptoms of great disturbance of the functions of the stomach after the division of the eighth pair of nerves in the neck; for in the rabbits, frequent ineffectual efforts to vomit occurred; and in the dog, (Experiment 9.), part of the contents of the stomach was rejected. 2. That examination after death shews, that digestion does not go on after the eighth pair of nerves have been divided in the neck. For parsley and cabbage remained in the stomachs of rabbits nearly eighteen hours, without any other change, than that which had been produced by mastication, and that of becoming rather of a browner colour; whereas, in a healthy rabbit, whose nerves had not been divided, the same substances, in a similar time, were reduced to a pulp, and were in a complete state of chemical decomposition. The stomachs, too, in the animals whose nerves had been divided, were much distended: the contents weighing nearly twice as much as the contents of the healthy stomachs. And in experiment 9, where only one nerve was divided, the food remained in the stomach, nearly unchanged, for forty-four hours.

In the dogs, whose nerves were divided, the stomachs were very much distended, and contained a large portion of gas. Moreover, the contents of the stomach, after a fast of four hours, weighed more than the food which had been given at

the last meal: whereas, in experiments 5 and 6, in four hours after a meal, the contents of the stomachs were found to weigh little more than half as much as the food which had been last taken. The state of the contents of the stomach was also very different. In the healthy stomach the food was chemically altered, and had assumed a fluid form; whereas, in experiment 10 and 11, it remained solid. In the healthy stomachs, four hours after a meal, the duodenum was full of chyme; the lacteals were distended with chyle; and the gall-bladder was flaccid; whereas, in the experiments in which the nerves were divided, the duodenum contained no chyme; the lacteals were empty; and the gall-bladder was distended.

These facts, which are afforded by the experiments above detailed, and supported by previous authority, are so diametrically opposite to the conclusion to which Mr. Broughton has come from a similar set of experiments, that an indifferent observer might at first smile at the fruitless endeavours of the physiologist to extend the boundaries of his science; and might, if such were the instability of the laws of nature, justly ridicule all attempts to investigate her wayward operations. But nature is ever the same, her laws alter not; although her interrogators, by mistaking her replies to their inquiries, often give an appearance of inconsistency to them.

Thus, in the case before us, it will, I think, appear, that Mr. Broughton has mistaken, and consequently mis-stated, the replies to his interrogations. On this subject, however, we shall be enabled to judge more correctly when the facts related by Mr. Broughton are brought forward, and compared with those of other writers.

One of the proofs of digestion which, according to Mr. B.'s representation, was invariably present in the stomachs of rabbits, in a greater or lesser degree, after the division of the eighth pair of nerves, was a quantity of chyme, which, in some cases, was very abundant towards the cardiac portion of the stomach*. This chyme very much resembled mucus, and a

* See Experiments 1, 2, 3, 4, 6, 7, 9, 10, 11, 12, 13, 15.

layer of it covered the parsley, which was of a brownish colour. In most of these experiments, particularly in the third, this chyme was only found about the cardiac portion of the stomach, and was wanting in the pyloric portion; moreover, it never seems to have been found in the duodenum. In fact, if the relation of the experiments be correct, this chyme was almost peculiar to the cardiac portion of the stomach, and, according to experiment 1, was mucus. Therefore, according to these experiments, chyme is like mucus, and is very abundant in the cardiac portion of the stomach; although it seldom appears in the pyloric portion. We are, however, usually taught, that chyme is the food which has been taken into the stomach, and altered there by the action of the gastric juice; and that it is usually found in the pyloric portion of the stomach, ready to pass into the duodenum, where it is separated into chyle, and excrementitious matter. We also find that this substance, which, by Mr. B., is called chyme, is described by Dr. Wilson Philip*, as a semi-fluid, which is usually found covering the contents of the stomach, whether the nerves have or have not been divided. The reader, therefore, may judge how far the presence of this matter is any proof of the digestion of the food.

Another proof, adduced by Mr. B. of digestion having gone on after the division of the nerves in the neck, is that of parsley assuming a brown colour! In seven of the experiments on rabbits, the parsley remained in the stomach from fifteen to twenty hours, and no other alteration was observed in it than this change of colour!! Now, had a healthy animal been similarly fed with one of those operated on, and killed at the same time with it, a complete chemical change would have been found to have taken place in the parsley after so long a fast; and the contents of the cardiac portion of the stomach would have been in a semi-fluid state. It is to be regretted, that

* "It deserves notice that, although the eighth pair of nerves have been divided, the food is found covered with the same semi-fluid which we find covering the food in a healthy stomach."—Wilson Philip *on the Vital Functions*, p. 124.

Mr. B. should not have had recourse to this mode of making the experiment, as he could not then have been so entirely deceived by the appearances of the parsley, which certainly afford the strongest possible evidence, that digestion did not go on after the division of the nerves. Of this, Mr. B. might also have convinced himself by consulting the chapter on the process of digestion, in Dr. WILSON PHILIP'S *Essay on the Vital Functions*, where that author observes, that, "when rabbits have fasted sixteen or eighteen hours, the whole food found in the cardiac portion, which is in small quantity compared to what is found in it immediately after a full meal, seems frequently to be all nearly in the same state with that next its surface, the gastric juice having pervaded and acted upon the whole, and it is consequently apparently fitted to be sent to the pyloric end." p. 162.

In the five remaining experiments, the rabbits did not survive the operation more than from two to nine hours, and in none of them either, did the food shew any marks of digestion. The alteration of colour being alone observed.

In the 8th experiment, which was performed on a puppy dog, the eighth pair of nerves, *it is said*, were divided. The animal lapped milk several times after the operation, and the stomach regularly rejected all that was taken. At length, however, the account goes on to state, that the stomach became quiet, and retained some milk. The animal died some hours afterwards. On examination, the stomach was entirely free from redness, and contained merely a little fluid resembling whey. "Hence it appears," says the author, "that the milk taken subsequently to the last vomiting had been regularly separated by the digestive process, and the curd dissolved and passed away." Now I am inclined to be sceptical with regard to the last milk having been retained on the stomach; for it does not appear that the dog was watched through the night, during which period a fit of vomiting, in all probability, came on, and the milk was rejected. But, even admitting that this was not the case, the author should certainly have satisfied himself, after death, that the eighth pair of nerves were divided. The sympathetic nerves are near, and have occa-

sionally been divided in mistake; still further, the eighth pair of nerves may have been divided, but care not being taken to prevent their divided ends coming in contact, partial union may have taken place, and thus the functions of the stomach would be partially performed. This once happened in my own experiments. After dividing the nerves, I found digestion of the food went on, and I was at a loss to account for the circumstance until, on examining the nerves, the divided ends were found united together. The same thing has been noticed by others. Dr. Macdonald, in his *Thesis de Ciborum Concoctione*, observes, "Insuper in experimento 30^mo^ animal necatum fuit, horis viginti quinque et quadraginta quinque minutis post pastum, et horis viginti quinque septemque minutis post nervos vagos persectos. In eo exemplo, nervus, ubi discussus fuerat, fibrinâ sanguinis effusâ quodammodo conglùtinatus reperiebatur; necnon indicia quædam chymi et chyli in duodeno observabantur; et (quæ sane res memoria dignissima est) vesicula fellis non turgida et distenta fuit, ut in experimentis 19^mo^ et 20^mo^, ubi pare vago diviso, concoctio cibi prorsus impedita et suspensa fuit; sed, e contrario, hæc vesicula quodammodo flaccida et vacua erat; non aliter ac si cibus, inter coquendum, eam contrahere et humorem suum expellere stimulasset." p. 41. Thus the conclusion which Mr. B. wishes to draw from the eighth experiment is by no means free from suspicion of error.

Neither are the experiments on the three horses at all favourable to this gentleman's views. In the 5th experiment the horse expired too soon to admit of any observation on the contents of the stomach. In the 14th, the animal lived fifty hours, and hay was found in the stomach in a masticated state; but, as the account says, considerably less than the horse had eaten. It will be admitted, that this indefinite mode of stating the result of an experiment can never be satisfactory. We cannot be certain that the quantity of food found in the stomach was less than that which was eaten, unless the food and the contents of the stomach had each been weighed. The only result, therefore, that can now be certainly known is, that there was some masticated hay in the stomach, and that the duodenum was empty;

both of which circumstances pretty certainly evince that digestion did not go on after the division of the nerves. Let, however, this point be settled as it may, the experiment by Mr. Field, in which he ascertained, after death, that the proper nerves were divided, must be regarded as quite fatal to Mr. Broughton's inference. For, in this case, the animal lived sixty hours after the division of the nerves, and yet not the slightest degree of digestion took place during that time. This one fact carries with it a conviction which appears perfectly irresistible; for, if the result of this experiment were fairly contradicted, there would be an end to the consistency of nature's laws. And yet Mr. B. does not express any surprise that the experiments which he relates should contradict each other; nor does it once seem to strike him, that he may have been deceived in his judgment of what he considers proofs of digestion after the division of the nerves.

The affection of the respiratory organs was, in all my experiments, manifest, from the moment of the division of the nerves, although the degree of dyspnœa varied considerably. Where the animals were allowed to die I was invariably convinced of the truth of Le Gallois' position, that the immediate cause of death, from the division of the eighth pair of nerves, is referable to the disorder of the respiratory functions; and he appears to have demonstrated satisfactorily, that the circulation ceases, from the opening of the glottis being diminished; from the lungs being congested, and from the bronchia becoming clogged by extravasated fluids.

The remarks with which Mr. B. concludes his paper are intended to explain the symptoms which follow the division of the eighth pair of nerves. He considers that Mr. Brodie has put this in a very clear light, in his Lectures at the College of Surgeons; by observing, that the lungs are endowed with sensation through the influence of the par vagum; and that, being deprived of sensation from the division of the nerves, on the sides of the neck, they gradually cease to act, and the muscles of respiration in vain strive to effect the proper circulation of air. The consequences must be apparent, the blood is prevented from imbibing

the wholesome influence of the atmosphere, and it becomes dark, discoloured, and unfit for the secretions of the stomach, and by degrees ceases to circulate altogether, the lungs become collapsed and turgid, and the heart loaded with coagulum.

This idea that the entire cessation of digestion, consequent on the division of the eighth pair of nerves in the neck, arises from a primary affection of the lungs is contradicted by many facts. The respiration sometimes continues tolerably free for some hours after great derangement of the functions of the stomach has been shewn by frequent vomiting. If the animal be killed before great difficulty of breathing comes on, as was the case in the experiment above related in one of the dogs, the same proofs of non-digestion appear as when the lungs are most affected previous to death. Besides, the very reverse of the position which the above gentlemen are anxious to maintain, often obtains: the first signs of difficult respiration are often seen to come on after repeated efforts to vomit; from which we might as fairly conclude, that the vomiting is the cause of the difficulty of breathing, as Mr. Brodie does, that the disorder of the lungs is the cause of the failure of the functions of the stomach. But in addition to all this convincing evidence it is well known that the lungs are often most severely injured without the functions of the stomach being sensibly impaired. Vomiting certainly is not a common symptom in the most severe asthmatic affections. Neither does the food in such instances remain in the stomach perfectly undigested.

On the whole, it may be maintained that no fact in physiology is better established than that the division of the eighth pair of nerves in the neck is followed by a suspension of the functions of the stomach.

It seems quite unnecessary to extend the present communication to the investigation of the evidence which proves that a certain power of galvanism will restore the functions of the stomach after the division of the eighth pair of nerves. We may, however, remark, that the experiments related by Dr. Wilson Philip remain uncontradicted. The inaccuracies of that experiment which was made at the Royal Society have been admitted

by one of the three conductors of it; and that gentleman has since related one more nearly corresponding in its result with those of Dr. Wilson Philip, in which a weak power of galvanism was employed. In addition to this Dr. Clarke Abel of Brighton has, in his experiments, met with results in all respects similar to those detailed by the latter author.

In alluding to the above report, Mr. Broughton's candour, I have no doubt, would have led him to mention Dr. Wilson Philip's reply to it, which appeared in the following number of the same journal, if he had been aware of that reply.

Art. VIII. *On Jasper.* *By* Dr. Mac Culloch.

[Communicated by the Author.]

Although Jasper occurs in many different situations in nature, and in almost every part of the world, it has scarcely yet obtained a place in a geological arrangement of rocks; its description being chiefly to be found in the works of mineralogical writers, by whom it is ranked among the simple minerals. If not very abundant any where, compared to the other more common rocks with which it is associated, it yet forms a member of the series, and cannot be omitted in an arrangement of this nature without inconvenience. I need, therefore, make no apology for the following imperfect remarks, which are all I have to offer on a substance that has hitherto experienced unmerited neglect; as it occurs often under very interesting circumstances, independently of the recommendation contained in its beauty and in its utility for ornamental works in stone.

Our geological information respecting this rock is, in particular, incomplete, as it does not seem to have received much attention from geologists. Mineralogists have been content to consider it abstractedly from its connexions, and there is consequently no want of descriptions of its varieties.

As yet it does not appear to have been observed occupying large spaces, or forming mountain masses. It will probably even be found that in many cases where it has been conceived to pos-

sess a considerable extent, the specimens from which that judgment had been formed, have been merely of an occasional nature, and that the leading mass has been some other rock of which certain portions assume the characters of jasper.

It is said, indeed, to form a range of mountains in Siberia; but the testimony on this head is of such a nature as not to claim much credit. It is not difficult for those who are practically acquainted with geological investigations, to account for errors of this nature, as well as for the apparent confidence with which such statements are made, not merely by ordinary travellers, but by geologists indulging in rapid and superficial views. It has, however, been asserted on authority that cannot be questioned, that it does actually occur in the country just mentioned, in very large masses embedded among the primary strata. It has also been observed lying in the same manner in the Apennines; and both these observations, as far as concerns the mode of its occurrence, are confirmed by similar facts in our own; as, in Scotland it is found on the southern skirts of the mountains near Fettercairn with similar connexions.

In France, according to Soulavie, it is found in a position intermediate between granite and basalt; and although no such instance has yet occurred in Britain, as far as I am aware, it is a situation extremely consonant to its general habits in many other cases.

Under these circumstances, and with these doubts about the care which has been applied to its examination, it has been found impossible to derive from the works of such authors as I have examined, any accurate geological information that could be relied on.

It must moreover be remarked that the term jasper has itself been applied in so vague a manner as to lead to great confusion. Being an ancient term, and having been commonly used in a commercial rather than in a mineralogical sense, it often is impossible at present, without an actual sight of the specimens in question, to understand what is intended in descriptions where it is named. It would be a sufficient example of this laxity to remark, that even the calcareous stalagmites of Sicily have gone

by this name; but it will not be uninstructive to point out a few of the substances, of which the real nature is actually known, which have been indiscriminately included under this popular term. To render such a catalogue complete, would require an access, not only to the specimens themselves, but to a history of their connexions, which is unattainable.

Siliceous schists, whether found among the primary strata, or among the secondary shales in the vicinity of trap, have been known by the names of black jasper and of striped jasper, according to the peculiarities of their colours.

The cherts that are coloured by chlorite or by the brown oxydes of iron, and which are modifications of that chalcedony which, in the same situations, forms heliotrope and brown carnelian, have also been enumerated among the jaspers.

Veinstones, consisting of various fragments entangled in agate, have also acquired this name among collectors; and it has indeed frequently been applied in a very vague manner to many agates where their chalcedonic characters were imperfect; and indeed, to any hard and uniform siliceous rock not included under quartz, and distinguished by brilliancy or intermixture of tints.

Several of the coloured cherts, often arising from the influence of trap upon neighbouring masses of argillaceous, or argillo-calcareous strata, have been, like the siliceous schist of the same origin, included under this head.

The term has also been indiscriminately applied to any hard substance of uniform texture, of an aspect more or less earthy yet indurated, and of an ornamental appearance, whatever may have been its origin and connexions, and of however accidental a nature.

Lastly, I shall only further enumerate all those siliceo-argillaceous substances, or highly indurated clays, of an uniform and fine texture, which do not easily admit of being ranked with the clay stones or the clink stones of the over-lying family of rocks, though it has occasionally been extended to these. The application of the term has been chiefly made to those varieties which possess strong colours, or ornamental mixtures of two or more of these.

It is to this last class of varieties that the term ought perhaps to be limited; as they cannot well be expressed by any other, and as all the preceding substances may easily be ranked under the several heads to which they strictly belong. Thus the term jasper will become useful in a scientific view without any material innovation, merely by confining it to one of the best characterized of the various substances to which it is indiscriminately applied.

If used in this sense it will be found that jasper occurs in different geological situations, in many of which it is evidently a substance changed, like the siliceous schists, from a different original condition into its present form, in consequence of the influence of trap, perhaps sometimes also, of granite.

The most obvious case of this nature is where it is found in beds, of greater or less extent, lying under masses of trap, or else interstratified with it. In these cases its true origin is often easily traced, as certain portions of the same beds will often be found retaining their natural and original characters, apparently from being more remote from the surrounding influence. The analogy, in this case, between such jaspers and those artificial substances known by the name of porcelain-jasper is very striking; and it is scarcely necessary to point out their resemblance in every respect to those that occur among volcanic rocks. The transitions of this variety are generally into yellow clay, or into the red iron clay which accompanies the trap rocks; and the colours accordingly vary. It is perhaps almost superfluous to remark, that the same substances are occasionally found where trap veins pass through strata capable of undergoing the same change. This variety has occasionally been confounded with pitchstone, as will immediately be explained.

The transition into clay, here mentioned, points to the cause to which the jasper in this case owes its origin, and forms an interesting fact among many others in the history of the trap rocks; confirming the peculiar influence which they exert on all those substances in contact with them, which are susceptible of alterations from heat. The unchanged parts of the beds, in these cases, are common ferruginous clay, red, yellow, or green,

or else argillaceous sandstones of different colours; and it is interesting to remark that, both the colours and the texture of the resulting jaspers, vary precisely as might be expected from the nature of these various substances.

The varieties, accordingly, which originate in fine clay, are generally characterized by a high degree of resinous lustre, and a conchoidal fracture, with a smooth surface. It is particularly important to be able to recognise and distinguish these rocks, as they have often been mistaken for pitchstone, and have given rise to the belief that this substance is occasionally stratified; that it is, in fact, a regularly stratified rock. But the two are essentially different, both in their mineralogical characters and their geological relations; nor, as I shall shew in a succeeding paper, is pitchstone a stratified substance. The prevalence of this important error, and the improper conclusions regarding pitchstone that have been drawn from it, will be very apparent in examining the usual collections of Hungarian or other foreign pitchstones, in which it will be seen that the greater number of specimens consist of this particular variety of jasper.

In the rocks of the overlying character which appertain to the division of claystone, the progress of induration generally causes them to pass into compact feldspar, as it is (perhaps with no great propriety) called. But, in certain situations, the same claystones are found passing into jasper; being highly indurated, without acquiring that peculiar character by which in their ordinary states of change, they are characterized. This change seems to occur chiefly among the more ancient rocks of this character, namely, the claystones and porphyries that accompany granite and the older rocks; and there is no difficulty in tracing its progress, even into the porphyries that so often predominate among this division of the overlying family. Thus there are found porphyries with a base of jasper. It must, however, be apparent, that whenever such transitions exist in the case of any rock, exact distinctions are unattainable; and many specimens of doubtful character are, therefore, the inevitable consequence. Such jasper is not, however, limited to the overlying rocks when in these situations only; as it also occurs, in certain cases,

among the secondary claystones where these are in contact with masses or veins of trap. The cases are here analogous to those first mentioned; but such local jaspers are often found to possess a peculiar concretionary structure; being either laminar, or formed of spheroidal or other analogous concretions. These are the substances frequently found forming the spotted and striped jaspers of collectors.

Lastly, jasper occurs in irregular masses among the primary rocks, occupying situations analogous to porphyry, claystone, or trap, but presenting no transitions into these by which to indicate a similarity of origin. That origin appears nevertheless to be, in some cases, the same: but, on this part of the subject, we are yet in want of much information.

From the preceding statement, it is apparent that jasper must belong indifferently both to the primary and to the secondary division; and it would be an unnecessary sacrifice to an imperfect arrangement, in this as in many other cases, to form two divisions of this rock, or to separate it into primary and secondary from any considerations of this nature.

The forms of jasper vary according to these several circumstances of position. Like limestone or serpentine, it is sometimes found in irregular masses, obscurely, or not at all stratified. In other cases, in the primary rocks, it appears to form true strata; a circumstance to be expected. Among the secondary rocks, it is massive and shapeless where it passes into claystone, and is stratified where it forms a portion of the series of strata connected with trap.

As it is also found in a state of transition into the ordinary stratified rocks, in both classes, it is easy to conceive how it may occur in small portions, of no determinate form or character, in those parts only of the beds where the granite or trap, to which its origin is referred, are immediately present.

Lastly, it exists in the form of veins, often very minute; and, in these cases, it is probably a mere modification of some venous rock of the trap family, analogous to that case where basalt becomes, in the progress of ramification, converted into pitchstone.

Jasper presents a few modifications of internal structure which

require notice. It sometimes gives indications of a spheroidal concretionary disposition, more or less perfect, and resembling that which, under circumstances of a similar nature, occurs in chert and in siliceous schist.

In the same way, it sometimes possesses a laminar structure and thus also it approximates to the siliceous schists. It is easy to see how, from similarity of origin, connexions, and composition, it may be thus a matter of doubt to which of those two rocks any given specimen or bed should be referred. The well known striped and spotted jaspers already noticed, owe their appearance to the two structures above-mentioned; and, occasionally, the two are combined in the same specimen.

It is much more rare to find jasper possessing a minute columnar structure resembling that of the madreporite limestone, or of ironstone. But this, when it occurs, is easily explained, when it is recollected that it often differs from this latter substance, only in the degree of hardness. The transition into ironstone is similar to that into the ferruginous clay of the strata which lie under trap.

The large columnar structure is still more rare, but one remarkable instance occurs at Dunbar, in Scotland; the columns being of considerable dimensions and resembling, in their general forms and disposition, those so common in the members of the trap family. In this case, the jasper passes into the sandstone and ferriferous shale, or argillaceous ironstone, from which it appears to have been derived; but as the whole of the circumstances attending this case are of great importance, I shall communicate a fuller account of them at some future opportunity.

I know not that it is possible to frame any general description or definition of the characters of jasper by which it could be recognised; at least by beginners in this science. It is superfluous to accumulate characters if they do not conduce to this end, and I shall not therefore attempt it. In a general sense, it may be conceived to be an extremely indurated clay, of which certain varieties approximate in their characters to hard pottery and others to porcelain. It has no predominant texture, and is

equally frangible in every direction, unless when it possesses some peculiar internal structure. Although the fracture is generally minutely granular, and the surface arid, it is occasionally also more or less splintery, or even conchoidal; while the minuter fragments may also be sometimes translucent. It is not easily confounded with any rocks except the indurated claystones and the pitchstones. From the intimate nature of their connexions it is obvious that it cannot always be distinguished from the former. From the latter it is more easily discriminated, by the more frequent absence of the resinous lustre and of the peculiar transparency of the fragments; although even pitchstone sometimes puts on characters by which it occasionally approximates to jasper. If nature has not always created definite boundaries, it is in vain for mineralogists to attempt it.

The colours of jasper are infinitely various, and are the principal cause of its estimation among mineralogists and lapidaries. They are also, in general, much more brilliant and decided than those of any other rock except limestone; yet the student will, from the preceding description, beware of using them as an empirical character to the neglect of others. Red of various hues, ochre yellow, greens, browns, greys of all tones, and black, are the prevailing tints; and they occur in every mode of intermixture, so as to present almost infinite varieties. From a wish to conform to the popular practice respecting this rock, of which the mere mineral characters are not much varied, these distinctions have, therefore, been introduced into the synopsis in a more conspicuous manner than has been adopted with regard to any other of the substances which I have attempted to render intelligible to beginners by a synoptical detail of varieties.

Synopsis of Jasper.

A. *With a dull earthy fracture, and passing into claystone, of which it appears to be a modification.*

This varies much in colour, but the term is generally limited to those varieties which possess decided or brilliant hues. Reds

and yellows are the most remarkable; but it also occurs of grey, brown, purplish, and greenish tints.

B. *More indurated, and resembling the base of certain porphyries.*

a. Simple, and of one colour, green, red, brown, yellow, or even black.

As this substance is generally collected for the sake of its colour, the more decided tints only are commonly found in cabinets, but it occurs of various hues.[1]

b. Striped with different colours, in consequence of a laminar structure.

The Siberian green and red variety belongs to this; it also occurs of different tints of red alternating, or of greys, or of other colours. The latter are also enumerated among the siliceous schists.

c. Spotted or variously mottled, in consequence of a concretionary spheroidal structure.

The Siberian spotted jasper ranks under this variety. The most common colours are, reddish and pale ochre, obscure red and white, and brown and ochre.

C. *Highly indurated, with an aspect approaching to that of chert, or even to agate; into which it passes, as it does into chert and quartz.*

a. With a somewhat granular fracture.

b. With a granular splintery fracture.

c. With a splintery fracture passing to the conchoidal.

d. With a flat fracture passing to the larger conchoidal.

The two latter varieties are among the most esteemed, as assuming the best polish. The colours most prevalent are reds and yellows, simple or intermixed in various ways. The varieties of C occur chiefly among the primary rocks.

D. *Intermixed in various ways with chalcedony either white or coloured, and apparently at times passing into that substance: jasper-agate of lapidaries.*

The ornamental appearance is often produced by the veins; and, as these become numerous in proportion to the base, it forms brecciated jaspers. The colours are much varied, but red and yellow, with white or colourless veins, are the most common. Sicily appears to abound in the most beautiful specimens of this variety.

E. *Minutely columnar, and resembling, except in hardness, the columnar ironstones. Found in the Isle of Man.*

F. *With a conchoidal fracture and resinous lustre: pseudo-pitchstones.*

These have been generally enumerated among the pitchstones, as already remarked, and as the colours have been considered important, they are here made a ground for distinguishing the subvarieties.

a. Pale yellow.
b. Ochre yellow.
c. Brick red.
d. Brown and purple brown.
e. Green.
f. Mottled with different colours.

The green variety is coloured by chlorite, and occurs in Iceland. They all pass into clay, and the transition is often found even in hard specimens. They appear to occur in volcanic, as well as in trap, countries. St. Helena and St. Vincent produce examples of this nature.

Some of the jaspers appear to pass into common opal, as they do into agate.

Art. IX. *A Translation of* Rey's *Essays on the Calcination of Metals, &c.*

[Communicated by John George Children, Esq., F.R.S., &c.]

The original edition of Rey's *Essays*, of which there is a copy in the library of the British Museum, was published at Bazas, a town about thirty miles S.E. of Bourdeaux, in the year 1630. In 1777, it was re-

printed with notes, by M. Gobet, at Paris, and published by Renault, Rue-de-la-Harpe. The copies of this reprint disappeared in a very sudden and remarkable manner, and the work was so little known in this country, that Doctor James Curry, at the sale of whose library, in 1820, I purchased a copy of it, states in a note at the beginning of the work, apparently in his own hand-writing, that he had sought it, in vain, for more than ten years, in every bookseller's catalogue in London, till, at last, the present copy rewarded his trouble, and he adds, that he had seen but one other copy since. The suppression of this edition, almost immediately after its publication, which took place in about three years from the promulgation of Lavoisier's first experiments * would naturally lead to the suspicion, that it was effected by that celebrated philosopher or his friends, to avoid the imputation of plagiarism, which might sully the brilliancy of his recent discoveries, and this imputation is, in the opinion of many, but too probable. Mr. Brande, however, has given an interesting note in his Dissertation on the Progress of Chemical Philosophy, prefixed to the third volume of the Supplement to the 4th and 5th editions of the *Encyclopædia Britannica*, containing a quotation from two scarce volumes of the posthumous works of Lavoisier, in Mr. Hatchett's library, in which Lavoisier expressly states, that he knew nothing of Rey's *Essays*, when, in 1772, he undertook a series of experiments on the different kinds of air or gas, disengaged during effervescence, and in many chemical operations; whence he learnt the true cause of the increase of weight, which metals acquire by the action of fire †. At the end of that note, he further states a precaution he had taken in November, 1772, to secure to himself the sole merit of the new French theory, claiming it exclusively for his own. It would be uncourteous, were that celebrated philosopher still living, and ungenerous now he is dead, to question the truth of his assertion; nor do I conceive I have any more right than I have inclination, to do so. His ignorance of Rey's work is, perhaps, not very extraordinary, since, as will appear by M. Bayen's letter, at the end of the *Avertissement*, that the book was extremely rare, and probably known to very few. After the publication of that letter, however, in 1775, Lavoisier must have known, and have read Rey's *Essays*, as, indeed, appears from his own words in the note already quoted; and, it is matter of

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* Read at the Academy of Sciences at Paris, November 12th, 1774, and published in the *Journal de Physique*, Vol. IV. p. 448.

† "J'ignorois alors ce que Jean Rey, avoit écrit à ce sujet in 1630, et quand je l'aurois connu, je n'aurois pu regarder son opinion à cet égard, que comme une assertion vague, propre à faire honeur au génie de l'auteur, mais qui ne dispensait pas les chimistes de constater la vérité de son opinion par des expériences."

regret, that he never did that extraordinary man the justice of mentioning his name, either in his papers read before the Academy of Sciences, or in his *Elements of Chemistry*, published in 1789. This, probably, proceeded from his considering Rey's opinions as mere speculations (see the preceding note); the reader will judge, whether they deserved no higher praise. How ready Lavoisier was to do justice to his cotemporaries, may be gathered from his conduct towards Doctor Priestley, respecting the discovery of oxygen gas; and it will hardly be considered an uncharitable inference, if we suspect something of the same spirit to have influenced him with regard to Jean Rey.

The following translation is from the copy of 1630. I have endeavoured to keep as close to the style of the original as I could, and perhaps the reader may sometimes think I had better have been less curious in my attempts to imitate its quaintness. He will too, occasionally, find the matter redundant, and the argument tedious. I once proposed to abridge it, but better judgments preferred giving it in its entire form, as a work, when divested of the rude philosophy of the day, of unquestionable genius and singular penetration, and one which, if not itself the real basis of modern chemistry, contains at least, such principles, that, had they been duly appreciated and followed up from the instant of their promulgation, could hardly have failed, long since, to have raised the science to an equal, or, perhaps, even a greater, height than that which it at present holds.

The re-print contains an *Avertissement*, parts of which I have thought worth inserting, as well as a letter of M. Bayen to the Abbé Rozier.—The first gives a short account of Rey, and mentions some facts which shew his work to have been well known and highly esteemed by Professor Spielman of Strasbourg, as late as the year 1766, and to have been honourably spoken of by M. de Bordeu, circumstances which make Lavoisier's ignorance of its existence still more extraordinary.

J. G. CHILDREN.

ADVERTISEMENT.

"JOHN Rey, M.D., was a native of Bugue, on the Dordogne, in the dependencies of the Barony of Lymeil, a city of the province of Perigord, situated above the confluence of the Dordogne with the Vizère, and belonging to the Duke de Bouillon, to whom his Essays are dedicated. It is not known in what university JOHN REY took his Doctor's degree, but he tells us he had a brother of the same name, the profes-

tor of an iron foundry, with whom he lived, and where he studied Chemistry and Natural Philosophy."

..... "Reputation is a strange thing! JOHN REY, who preceded the immortal Pascal, the celebrated Descartes, and the great Newton, is almost unknown in the republic of letters. His style resembles that of Michel de Montaigne,—he has the same energy, and less diffusiveness,—it is surprising that so powerful a writer should have been absolutely forgotten. His book, which treats of one single experiment, was not calculated for his time,—it belonged wholly to our own:—printed in a small provincial town, for the use of some friends, it had none of those celebrated *puffers, (proneurs,)* who assign the various ranks in science; for they who would receive the wreath of immortal fame must address their adoration to those great cabals, which have established their thrones in the scientific world,—but the glories that surround the heads of the spirits, so cried up in these circles, gradually fade away. Literary usurpations are, in time, discovered, and some celebrated geniuses, who were the wonder of their age, have ended like Ronsard, who was no more thought of when Malherbe appeared. In short, the academy of sciences was not yet in existence, and the spirit of sect prevailed in all the little committees of science *(bureaux)* that were then held at a few private houses."

The author of the *Avertissement* next mentions several of Rey's correspondents, and especially, Marin Mersene. He then states, that "the Essays" are very scarce, that when they appeared Mersene had doubts on the subject, which Rey answered in a masterly manner. Raphael Trichet copied these letters, and in the catalogue of his library, *Rey's Essays* are inserted in the class of Philosophy, Natural History, &c.

The volume passed from thence into the king's library, and was mentioned to the editor, (M. Gobet,) by M. L'Abbé Desaunays. The reprint was from a copy furnished by M. de Villiers, from his own library, who had the liberality to sacrifice it for the public good*. I translate the following, *verbatim.*

* M. de Villier's, "*a bien voulu sacrifier son exemplaire en faveur du bien*

"Mr. Spielman, professor of chemistry, at Strasbourg, recommends the Essays of John Rey to students, in the edition of his "*Institutions de Chimie*" of 1766, also translated into French. M. de Bordeu, in his "*Recherches sur les Maladies Chroniques*," No. 93, octavo, one volume, makes so honourable mention of John Rey, that we invite the curious to have recourse to it.—M. Jean Frederick Corvin sustained a thesis, entitled *Historia aëris factitii*, under the presidency of M. Spielman, at Strasbourg, on the 4th of December, 1776, a pamphlet of sixty pages in 4to., with figures, in which John Rey is named as the first author, who has written on this important subject. The elements of chemistry for the public course of the academy of Dijon, of this year, may also be consulted. M. Sage praises it in his *Mineralogie Docimatique*, lately reprinted. Finally, M. Bayen, a celebrated chemist, was the first to do justice to John Rey, and he has permitted his Letter to M. L'Abbé Rozier to be printed at the beginning of this edition."

A Letter from M. Bayen*, *chief Apothecary of the Army, &c., to M.* L'Abbe' Rozier, *Dignitary of the Church of Lyons, &c.*

Monsieur,

The cause of the increased weight that certain metals experience by calcination, has, at all times, been a subject of speculation and research with chemists and natural philosophers. Cardan, Cæsalpin, Libavius, and many others, formerly endeavoured to explain this phænomenon; but amongst them all, we must, in justice, distinguish John Rey, a physician of Perigord, who lived at the beginning of the last century. His work, perhaps unknown to all the chemists and naturalists of the present day, appears the more deserving to be rescued from oblivion,

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public.—" Why sacrifice it? Could it not have been copied without injuring the original? Or, was it in 1777, as more recently, found easiest to *print at once* from the lacerated pages of the unfortunate original? Your own, or whose else, no matter!

* Published in the 5th volume of the *Journal de Physique*, part I. 1775.

because the reason which he assigns for the increased weight of the calces of lead and tin, has an immediate relation with that, which is on the point of being acknowledged by all chemists.

It was not, Monsieur, till after I had published in your journal, the second part of my experiments on the calces of mercury, that I became acquainted with Rey's book. I could not mention it in the very short enumeration I then gave of the different opinions on the cause of the increased weight of metallic calces; my fault, involuntary as it was, must be repaired, and to this end, I hasten to do justice to an author, who, by the profoundness of his speculations, has succeeded in assigning the true cause of that increase.

I hope, Monsieur, that you will concur with me, in making known Rey's excellent work. Your journal is read throughout France, it is spread over foreign countries; if you would insert this notice in it, the chemists of all countries would soon know, that it was a Frenchman, who by the power of his genius and reflections, first guessed the cause of the increased weight that certain metals experience when converted into calces, by exposure to the action of fire, and that it is precisely the same as that, whose truth has just been proved by the experiments which M. Lavoisier read at the last public sitting of the academy of sciences.

P. S. We may presume, that copies of Rey's work are rare. That which I have before me belongs to M. de Villiers, physician of the faculty of Paris, who has the best chemical library in France, and which he has sincere pleasure in laying open to the cultivators of the science. M. de Villiers's copy came from the library of the late M. Villars, physician at Rochelle, which was sold by his heirs in the course of last year. This copy was defective, it ended at p. 142, containing only the beginning of the 28th Essay. I requested M. Capperonier to allow me to transcribe, from the copy in the king's library, the two pages that are wanting in M. de Villiers's,—which he had the goodness to accede to. Thus, they who may wish to read John Rey's work, are informed, that there is a copy of it in the king's library, at the end of which they will find two manuscript letters; the first

from Father Mersene to the physician, John Rey, in which he attacks the natural philosophy of that author,—the second, Rey's answer to Mersene, in which he defends himself with all his might.

TITLE.

Essays by JOHN REY, Doctor of Physic, *on an Inquiry into the Cause why Tin and Lead Increase in Weight by Calcination.*

A BAZAS.

Par Guillaume Melanges, Imprimeur ordinaire du Roy, 1630. The Essays are dedicated to Monseigneur the Prince of Sedan, and the dedication itself is curious and diverting, but too long to be inserted.

M. BRUN'S *Letter, which gave rise to the present Essays.*

To M. REY.,

Sir,—Wishing a few days since, to calcine some tin, I weighed out two pounds six ounces of the finest sort, from England, put it into an iron vessel fitted to an open furnace, and keeping it continually stirred over a strong fire, without adding any thing, I converted it in six hours, into a very white calx. I weighed it to ascertain the loss, and found there were two pounds *thirteen* ounces of it. This occasioned me incredible astonishment, not being able to imagine, from whence the seven ounces of increase could be derived. I made the same trial with lead, of which I calcined six pounds, but in this I found a loss of six ounces. I have inquired the cause of many learned men, particularly of Dr. N.* but no one has been able to declare it. Your ingenuity which, when it pleases, soars beyond the common flight, will here find matter of occupation, and I beseech you most earnestly to inquire into the cause of so rare an effect, and so far to oblige me that, by your means, I may be enlightened in regard to this miracle.

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* *Deschamps.* All the notes with a numeral reference, are from the reprint by Gobet.

Essays by JOHN REY, Doctor of Physic, *on an Inquiry into the Cause why Tin and Lead increase in Weight by Calcination**.

PREFACE.

Some illustrious persons, having remarked with admiration, that tin and lead increase in weight, when we calcine them, conceived a laudable desire to ascertain the cause. The subject was fine, the inquiry painful, the fruit small; for, having turned their thoughts on all sides, they adduced such weak reasons for it, that no man of good judgment could venture to rely on them, nor feel his mind thereby relieved of its doubt.

M. Brun, an apothecary at Bergerac, having lately observed this increase, and thinking, as I believe, that no one had noticed it before him, requested me, by letter, to turn the subject in my mind, and furnish him with the cause of it. Now, because he is a person whom the integrity of his life, his rare experience in his art, and other virtues, compel all worthy men to wish well to, I avow, they have had such power over my affections, that I could not deny his request. At his *prayers and amuable solicitation*†, therefore, I have devoted some hours to the subject, and thinking that I have hit the mark, I publish these, my Essays, respecting it. Not, however, without very well foreseeing that I shall incur the imputation of rashness, since I hereby encounter opinions, for ages approved of by most philosophers. But what rashness can there be, in bringing truth, when known, to light? Might I not more justly be deemed puerilely timid, if I dared not divulge it, or sordidly envious, if I kept it concealed? I protest against these two last censures, hoping also to be acquitted of the first, by all liberal minds, who, having *tasted* my reasons, if they find them palatable, will thank me for having brought them

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* The increased weight of these metals by calcination has been observed from the beginning of chemistry. Geber, *de Saturno*, says of lead, "*non conservat proprium pondus in transmutatione, sed in norum pondus mutatur, et hoc totum magisterio acquirit.*" The same author says also, of tin, "*et pondus acquirit in magisterio hujus artis.*"

† Literal.

forward; but, if otherwise, will not fail to approve my search after the truth, in so difficult a question, and will be stimulated by my example, to treat the matter more skilfully, which I invite them to do; at all events, I shall have shewn my desire to benefit the public, by suffering this manuscript to appear before it, though it stamp an injurious stigma on my own reputation.

Essay I.

All matter under the compass of the Heavens, has weight.

God, creating the universe, neither made it perfectly like Himself, nor perfectly unlike, for He, being *One*, has made the world as *not one*, from the diverse multiplicity of its innumerable parts, ordaining, nevertheless, that they should collect into a certain unity by their exact contiguity. The upper world has no connexion with this subject; the lower, and elementary world, owes this contiguity to the weight divinely impressed on its parts, aided by the subtle fluidity of some of its simple bodies. It is by this quality, with which the matter of the four elements is more or less invested, that they are separated from one another, and each transported to its proper place, as the generation of compounds*, and the beauty of the universe requires. For this matter, every where filling the space closed in by the curvature of heaven, is continually pushed, by its own weight, towards the centre of the world. Earth, it is true, as the heaviest, readily occupies this situation, and forcing its fellow-elements to retire, causes water, the second in weight, to be also second in place; so that the air, driven from the lowest, as well as the second station, holds the third place, leaving the highest region to be occupied by fire, the lightest of all.

The chemists furnish us with a pretty representation of this, by taking pulverized black enamel, liquor of tartar, brandy tinged blue with litmus, and spirit of turpentine reddened by alkanet, and shaking the whole together in a phial, till it

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* "*Des mixtes*"

forms one confused mixture. The vessel being then left at rest, it is pleasant to see the clearing off of the confusion* The enamel gains the lowest station, representing earth; the liquor of tartar settles close by it, representing water; the brandy, like the air, occupies the third place; and spirit of turpentine, to shew the nature of fire, arranges itself above them all. All this is effected by the influence of weight, according as it is largely or sparingly distributed amongst these bodies. In the same manner the elements acknowledge no other cause that arranges and disposes each in its proper place, it being needless to introduce levity, which our predecessors vainly devised for that purpose.

Essay II.

There is nothing absolutely light in Nature.

Almost all philosophers, ancient and modern, fearing an eternal confusion of the elements, were they all endowed with weight, conceived the two uppermost to be furnished with a certain levity, by means of which they bounded up on high, each to occupy its peculiar place, like as the two lower ones are pushed downwards by their own weight. But having clearly shewn in the last Essay, that levity is not necessary for that effect, weight alone being sufficient, I embrace the maxim, which they themselves have prudently laid down, that we should never multiply *existences*† unnecessarily; assuming that God and Nature do nothing in vain, (which they also teach.) I think it would be otherwise were we to admit *levity*, since it is of no use. I say much more; that fire, being of so subtile a nature, that it can hardly be called a body, is consequently almost stripped of all resistance; whence it follows, that the air, mounting up without impediment, would reach the skies, driving fire from its place, and compelling it to seek a lower station, to the injury of their own doctrine. To this I will add another inconvenience, namely, the perpetual and unprofitable strife, which would ensue between the heavy and the light elements, the latter

* "*Le debrouillement se faire.*" † "*L'estre des choses.*"

pulling upwards, and the former downwards, with all their might; whence would arise, at the place of their contiguity, incomparably greater *distress** than the *packthread*† experiences which is pulled in opposite directions by two strong hands, till at last it is broken by their efforts: far different from that knot of friendship, in which nature has been pleased to unite the neighbouring elements, planting in their bosoms similar qualities, whence they communicate and amicably sympathize with each other‡. It follows from all this, that levity is a term that signifies nothing absolute in nature, and must be rejected; or, if we retain it, it must only be to denote the relation of one substance, having less weight, to another which has more.

Essay III.

There is no natural Motion in the upper Regions.

What shadows would be if there were no bodies, such natural motion in the upper regions will become, without levity. For, of a truth, it would be monstrous, to see natural effects without a natural cause. That is said to move naturally, whose cause of motion is in itself. Now, casting a look on all that moves, I see nothing that ascends by its own proper motion. Water, indeed, rises in a glass, if we throw earth into it; but all will allow, that it is not from any levity that is in the water, but rather, that the earth, by falling to the bottom, makes the water ascend. Now, if water does not acknowledge levity as the cause of this motion upwards, why should air confess it, which ascends in like manner when pressed on by water? Why fire, which does the same? It will be said, I doubt not, that if the upward motion of the elements be not natural to them, it must be violent; whence this absurdity follows, that each obtains its place in the universe by force. To this I answer, that the elements not

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* *Souffrance.*

† *Fiscelle.*

‡ This is a good specimen of the style of philosophizing, in our author's day; and we wonder less at the visions of alchymists and transmuters, when we find such stuff proceed from such a man as Jean Rey.

having the cause of these motions in themselves, they may, so far, be called violent; but that this violence is gentle, and nowise ruinous. Thus, the motion of the orbits of the planets* from east to west, having its cause in a higher heaven, is called by all violent, without, however, its doing them any injury. Moreover, they who argue thus condemn themselves, since they are compelled to admit, that not only the motion of water and air, but their very abiding places, are held by violence:—that of the latter, under fire, and that of the former, above earth.

Having thus vanished levity, and its upward motion, from all the boundaries of nature, we aver anew, that the elements of air and fire, which alone come into the dispute, are endowed with gravity.

[*To be continued.*]

ART. X. *Remarks on the Depression of Mercury in Glass Tubes. In a Letter to the Editor.*

SIR,

Entertaining, as I unfeignedly do, the profoundest respect for the analytical talents of Mr. Ivory, and admitting most readily, that he has contributed, more than any person now living, to advance the reputation of this country, among our cotemporaries abroad, with regard to abstract mathematics, I am almost surprised at my own temerity in taking up, as one of the fraternity of anonymous writers, the gauntlet, which he has thrown down, in silent contempt, at the feet of one of my brethren, who is also a fellow-labourer of his own, in the Supplement of the *Encyclopædia Britannica*.

The elaborate speculations of his unfortunate co-operator, the author of the article on CONVERSION, who had flattered himself, in a sort of fool's paradise, that he had obtained a glance, "like a flash of lightning," into the intimate constitution of matter, so as even to form a rough estimate of the magnitude

* "*Les cieux des Planètes.*" So Milton,

"Moon, that now meet'st the orient sun, now fly'st
With the fixed stars, fixed in their *orbs* that fly;"

and the generality of the philosophers of that day.

of its elementary particles; these day-dreams, as they appear to Mr. Ivory, are despatched in so summary a way, that no room is left for dispute respecting them, under the sweeping operation of the general remark, that, "*if the truth is to be told*, it may be affirmed, that reckoning back from the present time to the speculations of the Florentine academicians, the formula of Laplace, and the remark of Professor Leslie, relating to the lateral force, are the only *approaches* that have been made to a sound *physical* account of the phenomena."

Certainly, the *truth is to be told*, and the truth only; but upon a question of a *physical* nature, it may be presumed, that other persons may think their own *conjectures* as good as Mr. Ivory's; and since he has not stated any reasons for rejecting the opinions of his predecessor, it would be useless to enter into any argument on this part of the subject.

But where a *mathematical* computation is concerned, Mr. Ivory's authority is far too high to be rejected without examination; and I had no doubt whatever, at first sight, that his "Table of the depression of mercury in glass tubes," was, as he asserts, so carefully calculated, that all the numbers might "be reckoned exact, with the uncertainty of one unit in the last place of figures," and I copied his numbers at once into the table of the former article, with the impression, that it ought to hide its diminished head before them, taking it for granted, that Mr. Ivory would not have made an assertion so positive, and so derogatory to the labours of another, without having fully assured himself of its perfect accuracy: but a little further examination convinced me, that I had been too much influenced, in this admission, by the authority of a great name, and too much dazzled, by the brilliant display of logarithms and exponentials, exhibited in the article FLUIDS.

The general equations employed in both methods are the same; the quantities derived from experiment are not materially different; and even the results of the two tables agree within the limits of the errors of observation in single cases; so that Mr. Ivory will not be disposed to deny, that the series of the article COHESION is perfectly true, as far as it is sufficiently

convergent; and its want of convergence in extreme cases might be easily remedied, if it were necessary, by means of Taylor's theorem; but its convergence is already abundantly sufficient to prove, that, in some instances, Mr. Ivory's numbers are erroneous, and not to the amount of "one unit in the last place of figures" only, but to *twenty times* that amount, or to *two units* in the *last place but one.*

The depression in a tube of six-tenths of an inch in diameter, for example, is made .00443, while in the article COHESION it stands either .00416, or .00413, according to the different bases assumed from experiment. Now the series, computed for this case (P.221), becomes .75 = .7160 + .0254 + .0060 + [.0026], the last term only being added from considering the ratios of the preceding; and it will appear upon examining the formation of the co-efficients, that the first two numbers are determined with perfect precision, and the third with a very small degree of uncertainty; so that the sum of the parts depending on strict demonstration amounts to .7160 + .0254 + .0050 = .7464, differing from the whole supposed sum by $\frac{1}{200}$ only; and if we omitted altogether this undemonstrated part, we should have, instead of .00416, about .00418, for the utmost possible value of the depression in this case. Indeed, the first term of the ultimate series alone affords a more correct result than the whole of Mr. Ivory's approximation.

It seems, therefore, manifest, either that Mr. Ivory has neglected in his formulas some terms which ought to have been comprehended, or that some numerical error has occurred in one of the two computations. But considering that the tables differ throughout from each other in a regular manner, the diversity can scarcely have arisen from any blunder of this kind; and it remains for Mr. Ivory, if he think proper, to account in some other way for the paradox.

I am, Sir,

Your very obedient servant,

London, Dec. 31, 1820. S. B. L.

Art. XI.— *Additional Observations respecting the Oil Question, by* Samuel Parkes, F.L.S., *&c.*

Dear Sir, *March 12th*, 1821.

A work has just appeared which purports to be Remarks on a paper of mine in the last number of your Journal, entitled, *Observations on the Chemical Part of the Evidence given upon the late Trial of the Action brought by* Messrs. Severn, King, and Co. *against the Imperial Insurance Company*, and as this volume contains many inaccuracies and mis-statements, I think it necessary to trouble you with some observations upon it. I am sorry to occupy any of your valuable pages with matter of this sort; but when you see the nature of the charges which have been adduced, I am sure that you will not wonder at my desire to refute them.

I am, Dear Sir,

Yours, &c.

To W. T. Brande, Esq. Samuel Parkes.

Ex fumo dare lucem. Horace.

The volume in question has the advantage of being the combined effort of six Gentlemen, whose names are, in some measure, already known to the public. They are of very different classes in society—some are professional—some commercial, and others purely scientific—but they all appeared in the Court of Common Pleas at Guildhall, as witnesses for the Insurance Offices. For the sake of brevity, therefore, and to avoid any personality, I shall call them the Associated Witnesses. Solicitous, however, not to occupy more room in the *Journal of Science, Literature, and the Arts*, than will be absolutely necessary, I shall use all possible despatch with the book; and shall pay no regard to the respective writers individually, but shall direct my attention to one object—that of correcting the misrepresentations which their book contains. Confining myself, therefore, to this line of reply, so far as it is consistent with the desire

which I have of unravelling the mystery in which these Associates have endeavoured to involve the whole of the investigations respecting the conflagration at Messrs. Severn, King, and Co's., I proceed to enter upon the task which now lies before me.

In conformity with this determination, I am desirous, in the first place, to direct the attention of those who may possess this publication to what the authors of it say at pages 4 and 5, respecting the mixture of recent oil with oil that had been boiled, and from which it is argued that I have *intentionally* suppressed a fact of great importance to their case. However, as the passage is garbled by these writers, I think it is necessary to quote it entire, as it was delivered by Wilkinson from the witness-box at Guildhall. He was speaking of the addition of recent oil to oil that had been boiled, and he expressed himself thus:—"It was to endeavour to know, for Mr. Taylor wished to know, whether a certain quantity of common oil, mixed with that oil which had been boiled, would produce inflammable vapour at a low temperature*."

The obvious reply to this is, could there have been any legitimate or fair reason either for boiling the oil, or for mixing different oils? Was it not to be expected that liberal and dispassionate men would have endeavoured to assimilate their experiments as much as possible to the operation at the sugar-house—and, instead of racking their ingenuity to discover some mixture, or some *modus operandi*, that would produce dangerous results, and give inflammable vapour at a low heat, would have confined their attention to experiments on oil similar to that which had been used by Messrs. Severn and Co., and at similar temperatures, and with an apparatus which approximated as nearly as possible to the one which they supposed had occasioned the conflagration.

This is not my opinion only, but it is the general feeling of the public—and I believe it was the consciousness of this which occasioned the formation of this very singular *Association*; and

* See Mr. Gurney's printed Report of the Trial, page 116

that it was the hope of giving a different impression to the public mind, which induced these Associates to print their Remarks in a *cheap* form, suitable for circulation among the community at large, rather than to address the Chemical Public, as I had done in a respectable scientific Journal. Their attempt, however, will prove abortive, and ere long the whole of the theory which their book was designed to support, will pass away, and be lost in forgetfulness:

"Tenues fugit ceu fumus in auras." VIRGIL.

But to proceed—at page 4, it is remarked, "When Mr. Parkes' object is to disprove Wilkinson's evidence, by a comparison with Mr. Faraday's, he suppresses the important fact, that the oil used by Mr. Faraday was new, and Mr. Parkes is perfectly aware, that to this difference in the state of the oil, the witnesses for the defendants ascribe much importance." Again, in observing upon what I say at page 342, of the Journal, they remark, "this is the second occasion on which Mr. Parkes confounds the properties of recent oil, and that which has undergone change by long exposure to heat; thus attempting to discredit the results detailed, by suppressing the very material circumstance, that the oil was not fresh, as that was with which he compares it."

To this I reply, that I deny the charge of having intentionally "suppressed an important fact;" and I am sure that those who witnessed the evidence on both sides of the question will see that I had no *reason* to wish to suppress a single argument, or a single experiment, that had been adduced in vindication of the theory of those who had been labouring to induce the Insurance Offices not to satisfy the legal claims of Messrs. Severn, King, and Co. Aware of the anomalous and unsightly appearance of the edifice they were attempting to erect, and knowing the unsoundness of its foundation, I could not possibly have any desire of concealing from the public the nature of the materials which they were employing in its fabrication. I shall reserve, however, some other observations on this charge of "suppression" to be offered hereafter; and I am in no appre-

hension of forgetting this promise, for I have noticed, as far as I have proceeded in the book, that what one of these associates asserts, the others repeat*.

The charge of having "attempted to discredit the detail of results" I also deny, for I had no interest in attempting any thing but the exposure of sophistry and the establishment of truth. And it was not likely that I should expatiate on the difference between *old* and *new* oil, when my own experiments had convinced me, that all which had been said upon this part of the subject amounted to nothing. Mr. Dalton and Dr. Thomson positively proved in court, that the continued heating of whale-oil produces no such change. "I have made several experiments," said Mr. Dalton, "on that subject, and there is scarcely any difference between the two, as far as concerns the production of inflammable vapour†." And when Dr. Thomson was examined by the Solicitor-General to the same point, he stated that "he had kept a quantity of whale-oil constantly at the temperature of 350° or 360° for six weeks, and that this produced no change, except that the colour of the oil became darker, and its consistence, when cold, greater. But that the property of the oil, as far as relates to the production of inflammable vapour, had not undergone the least alteration‡." Other gentlemen, possessing considerable chemical knowledge, and of unimpeachable reputation, offered the same opinion; and yet these co-partners, without adducing one solitary new experiment, still persist in endeavouring to persuade the public, that the mere heating and cooling of a mass of whale-oil, day by day, for three months, will so far change its nature, as to render it capable of giving out, at a low temperature, such a quantity of inflammable vapour as would endanger the safety of the building in which it was placed. A theory of this kind, if

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* "By many strokes that worke is done,
Which cannot be performed by one."—WITHERS' EMBLEMS.

† Mr. Gurney's Report of the Trial *versus* the Phœnix Insurance Office, page 172.

‡ Ibid. page 125.

it could be established, would render such effectual service to the cause which these zealous Associates have undertaken to support, that I shall not be at all surprised to find these sentiments echoed and re-echoed in various parts of the book, among the other numerous repetitions which have been employed to swell its size, and increase its extrinsic importance. However this may be, I shall not think it necessary to say another word to refute that unfounded and unfortunate speculation.

Respecting my observations on the expulsion of the oil from the vessel in Whitecross-street, they remark, "Mr. Parkes cannot be so ignorant of the effects produced by the formation of vapour in a viscid fluid by ebullition, as not to know, that the fluid itself may be carried over with the vapour." All I need say upon this is, that I leave them to form their own opinion; and, without attaching much value to that opinion, I assure them, without hesitation, that I feel no shame in being charged with ignorance from any quarter, especially as no individual can know every thing on any one subject that can be mentioned; and the longer I live the oftener do I see occasion to deplore my ignorance of many subjects which I am desirous of investigating; but this I do know, that if they had charged the vessel in Whitecross-street with a quantity of oil that bore the same proportion to the size of their vessel, as the oil at the sugar-house did to the size of that vessel, there would have been no spouting up of the oil, however fierce the fire had been that was put under it; but the oil would have expanded gradually and without disturbance, and the oleaginous and aqueous vapours would have passed off quietly through the tube as fast as they were generated. This may be considered as my reply to their exulting statements on the spouting up of the oil, which they refer to, not only at pages 5 and 6, but also at pages 11, 12, 39, 42, 47, and 50.

Do these gentlemen imagine that, if we had wished to have created an alarm, we could not have summoned our friends around us, and have filled our oil vessel in such a manner as to have had "a sort of irregular fountain" also; and "the

jerks and the concussions," as well as "the expulsion of the oil," and "the striking of the ceiling," which these associates have all so seriously described. This, however, was not our intention; our object was to assimilate our apparatus to that which had been constructed by Mr. Wilson, the ingenious inventor of this most important mode of boiling sugar, and other inflammable substances, by means of heated oil; and to confine ourselves to such experiments as we thought calculated to discover whether the danger which these Associates had attributed to such an operation did really exist or not. It was to this object that the experiments of all the gentlemen who had been engaged by Messrs. Severn, King, and Co. were directed, and I congratulate them as well as myself, and even these Associates, that our labours have been crowned with triumphant success. I say *triumphant*, because I feel, and I think every good man must feel, that he has cause for triumph whenever he can reflect that he has been instrumental in extricating a fellow-creature from a situation in which he could contemplate nothing less than a considerable diminution of his fortune, and the consequent destruction of many of his legitimate prospects of independence and comfort.

Again, "As Mr. Parkes cannot conceive how the expansion of an elastic fluid can expel a common fluid from a boiler, it is very natural to find him thus attributing extreme violence to expansion,—an operation which, especially in a fluid, is as gradual and regular as can be imagined." No where have I said, or even intimated, that the usual expansion of oil by heat is a *violent* operation; and I do not conceive that there is one individual among these six Associates that would so far risque his reputation as to make a direct assertion that I had. But, as their book might be expected to be seen by many persons who have no opportunity of reading this *Journal*, I do not at all wonder that they were unable to resist the opportunity which my commentary upon their oil-fountains had afforded them of taking an unfair advantage; in return for my having had the temerity to espouse an opposite side on a great public chemical question.

I know, as well as they, that oil, if allowed to expand freely in an open vessel, would exhibit none of the fearful phenomena which they have described; but when confined in a vessel with no orifice except that of a small tube, and in such a quantity, as when expanded by heat would be more than sufficient to fill the vessel, then the liquor not getting vent so fast as the fire produces the expansion, it would act like a syringe, and the oil would be forcibly expelled from the mouth of the tube. If the heat of the fire were intermitting, this expulsion would be intermitting also, and we should then have all the spoutings and the dashings against the ceiling, from the circumstance of the expansion alone. Did these gentlemen never see the water dart suddenly out of the spout of a tea-kettle from expansion, and this before the water had acquired a boiling heat?

But even were there two or three inches of vacancy in the vessel (and there could not have been more, even by their own shewing*, for they say they put in twenty-four gallons, which we know would be expanded by heat to twenty-nine gallons), this vacuity would be filled with vapour, in consequence of the large quantity of water which is always formed in oil at a high temperature; and this being generated faster than it could be carried off by the tube, would press with a force on the surface of the oil, that would be sufficient to produce all the effects which they have related.

Respecting my notice of the term "silent explosion," they have made the following observations: "In the *Rudiments of Chemistry*, p. 115, Mr. Parkes tells us, that prussic *acid* has a *sweet* taste; and there is even reason to suppose, that he is actually a believer in explosions without noise, for in page 570 of the *Chemical Catechism*, he defines detonation to be an explosion *with* noise, which manifestly implies that, in his opinion, noise is not necessary to explosion. Now, in all fairness, I think Mr. Parkes must allow, that he who describes

* They have made two calculations as to the quantity of oil which their vessel would hold, *viz.*, one at page 44, and the other at page 51 of their book. And though they are calculations which any school-boy could have made, they are both erroneous.

acids as being sweet, and detonations to be explosions with noise, cannot reasonably censure the term 'silent explosion' as an absurdity." From the length of this passage, it will be most convenient to divide it into two, and pay my respects to each of them separately. It may however be observed in passing, that it was matter of surprise to me that these Associates did not fix upon some real error in a work like the *Chemical Catechism*, but were obliged to go also to a little school book, printed eleven years ago, to find a blot (and this is no blot) to exhibit to general notice. What can this be attributed to, but to their impatient and eager anxiety to lose no time in presenting their volume to the public?

Mr. Parkes tells us that prussic acid has a sweet taste; and what of that? The great Scheele, who made the first important discoveries on prussic acid, called it sweet; and at the time when the *Rudiments of Chemistry* were printed, in the year 1809, every chemical writer of any note, called it sweet. A few volumes taken from the shelves of my library, as they accidentally occur, will prove this to the satisfaction of any individual who may entertain doubts on the subject. "Prussic acid has a strong odour, its taste is sweetish and pungent."—Dr. Murray's *System of Chemistry*, Vol. IV. p. 713, 2d edition. "Its taste, which at first is sweetish, soon becomes acrid," *&c.*—*Fourcroy*, Vol. IX. p. 127, English translation. "Prussic acid is a colourless liquid; its taste is sweetish," *&c.*—Doctor Thomson's *System of Chemistry*, second edition, Vol. II. p. 183. "This acid has a sweet taste."—*Bouillon Lagrange*, English translation, Vol. II. p. 356. "The prussic acid has a sweetish taste, and a smell resembling that of bitter almonds." Doctor Henry's *Epitome of Chemistry*, 4th edition, p. 396. "The prussic acid has a sweetish and acrid taste."—Aikin's *Chemical Dictionary*, Vol. II. p. 254.

Again, in all fairness, I think Mr. Parkes must allow, that he who describes acids as being sweet, cannot reasonably censure the term, &c. This statement, though said to be made "in all fairness," is surely any thing but fair, and I must leave

it to those who may understand the subject, and are, at the same time, well acquainted with the attainments of the Associates, whether it arises from ignorance or design. According to them, any one who is not acquainted with chemistry, would imagine that all acids must necessarily *taste* sour; whereas, it is well known, that the molybdic, the tungstic, the uric, and some other acids are *tasteless*.

For the other part of the charge respecting explosions, a very few words will suffice. "There is reason to suppose," say they, "that Mr. Parkes is actually a believer in explosions without noise, for in page 570 of the *Chemical Catechism*, he defines detonation to be an explosion *with* noise, which manifestly implies that, in his opinion, noise is not necessary to explosion." Not quite so manifest, for though I am aware that there may be much sound with little sense*, I am still of opinion, that noise of some kind must always accompany explosion. To consider my expression, "explosion with noise," to be equally incorrect with that of "silent explosion," is to confound that which, according to their own notion, is a mere redundancy, with that which is a direct absurdity; but if these Associates had been acquainted with the etymology of the words explosion and detonation, they would surely not have hazarded so ridiculous an observation. *Explosion* is evidently derived from the Latin word *explodo*, and this was formed from *ex* and *plaudo*. With the Latins, *plaudo* meant "to make a noise by clapping, to clap in token of applause," as at the close of an entertainment; and consequently *explodo* meant "to drive out with clapping of hands, to hiss off the stage." Agreeably to this, Dr. Johnson explains our English word, *explode*, to mean, "to drive out with *some noise* of contempt," and *explosion* to be "the act of driving out." Our great poet gives the word a similar explanation, and without meaning any invidious appli-

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* Bullatis mihi nugis,
"Pagina turgescat, dare pondus idonea fumo."
PERSIUS

cation of the sentiment it contains, I may be allowed to quote the passage in illustration of my definition:

> " ——————Thus was the applause they meant
> Turn'd to *exploding hiss*, triumph to shame,
> Cast on themselves from their own mouths." MILTON.

From these various authorities, we may surely be justified in considering explosion to signify a hissing, or an *inferior* kind of noise; whereas, *detonation* means noise of a more violent kind, which will appear by turning to its primitive, *detono*, which means, according to Ainsworth, " to thunder mightily." I, therefore, contend, that the explanation which I have given of DETONATION in the vocabulary of the *Chemical Catechism*, as " an explosion with noise," is the true definition.

As to the circumstance, whether the pipe in their experiment vessel dipped into the oil or not, it appeared to me very strange that they should, when examined in court, have acknowledged their ignorance of so important a fact; but as I had noticed the circumstance, I ought, perhaps, to have noticed the manner in which Wilkinson explained the matter.

In commenting upon a passage of mine, in which I express a wish that the gentlemen who instituted the public experiment, as it has been called, would investigate the matter thoroughly for the credit of us all; these Associates write thus:—" Who would not suppose from this passage, that the gentlemen to whom Mr. Parkes alludes were less anxious than himself for such investigation as he appears to wish for?"

In answer to this, I have no hesitation in saying, that I believe some of the gentlemen who were engaged for the Insurance Companies, were anxious to investigate the subject, and that if they had had the conducting of the experiments, they would not only have discovered the truth, but would candidly have avowed it. But when I examine the volume which has been put forth, and perceive that the writers have not adduced one new experiment, nor have taken the least pains to explain to the public how they came to conclusions so diametrically oppo-

site to those which have been avowed by the gentlemen with whom I act, I cannot believe that these Associates, now that they find the facts are against them, are desirous that the manner in which their experiments were conducted should be investigated.

One of them, however, at page 10, thus proceeds: "I think," says he, "it will excite something more than the surprise of the reader when I assert that Mr. Parkes, and others on the same side of the question, were repeatedly invited, and did as repeatedly refuse to make experiments in common for the credit of us all." He adds, "it remains, therefore, for Mr. Parkes to shew; why he refused the repeated invitations which they sent him," &c.

I know not what these Associates may think, but it is my opinion, that they will not acquire much credit for having adverted to this circumstance; and I shall wonder if it does not "excite not only the surprise," but the indignation of the reader, when I tell him, that they know as well as I do why "Mr. Parkes, and others on the same side of the question," did not attend to witness their experiments; and if it had suited their purpose, they would not have withheld that information from the public. They have thought fit, however, in order to impute to us bad motives, to complain that we would not meet them, and yet have not had the candour to disclose the reasons which we gave for not complying with their invitations.

The facts are these; the solicitors for the defendants invited two persons on behalf of Messrs. Severn and Co. to meet two others, chosen also by themselves, to make joint experiments. The reasons for not acceding to this proposal were, among others, as follow:

1st. Because all the chemists for the plaintiffs were not invited, and we did not see why we alone should be expected to witness a series of experiments, to be detailed by us in court, and thus take upon ourselves a responsibility which we conceived ought to be shared alike by all.

2d. Because of the impossibility of giving our personal at-

tention to the management of experiments to be conducted at a distance from our own laboratories, which would require several weeks for their completion.

3d. Because, when this proposal was offered, we had made a number of experiments ourselves, and these were sufficient to satisfy us that no danger arose from the oil apparatus; and, therefore, as the question in our minds admitted of no doubt, we felt no inclination to waste our time in witnessing any experiments which the opposite chemists might think fit to institute. And above all,

4th. Because no results which we could possibly have witnessed at their place, in opposition to the evidence of our own senses, could have overturned, or even shaken our confidence in, those which had been obtained by our own apparatus—by our own oil—and in our own laboratories.

When several months had elapsed, another invitation was sent to some of the gentlemen consulted by the plaintiffs to meet several gentlemen at Bromley, on the part of the defendants, "for the purpose of ascertaining results from oil which had then been under the process of heating for a considerable time past." When I received this invitation I felt inclined to accede to the proposal, and I wrote to the defendant's solicitors to say that I would attend; but when I found that several of the chemical gentlemen who were acting for Messrs. Severn and Co., were of opinion that it would be absurd and useless to go to witness the *latter end* of a long experiment, and become a party to a proces[illegible]hich had been commenced, and for a long time conducted [illegible]ut our superintendence or control, I thought it necessary to decline the invitation. And I was more especially induced to come to this determination, when I learned that some of my chemical friends considered these invitations as part of a dexterous piece of management, by which it would have been attempted on the day of trial to shew, that these experiments were in fact ours, and thus have added weight to them with the court and jury, by an appearance of our having sanctioned the way in which they were conducted. So much for this accusation, which is urged and re-urged in various parts of the book.

The next charge which these Associates have thought proper to make is respecting Wilkinson's evidence, for that I professed to have "gone through the whole with great care and to have collected the principal points into one view," and yet have neglected to comment upon his *last* experiment which they consider the most important of all. They say that I have "not scrupled to conceal this experiment," that "Mr. Parkes, aware of the importance of this person's evidence, has not scrupled so to garble and curtail it, as to render it a more easy task for him afterwards to dispose of the evidence of the scientific gentlemen who followed" — that "I have entirely omitted his, (Wilkinson's) account of the only experiment that was witnessed by the several scientific gentlemen who attended in behalf of the defendants, and that this was purposely omitted," — that I have "wilfully misquoted his words, in order to give a different meaning to what was intended," — and that I have given "no description of the experiments at all, and cannot even understand them."

To all this I reply, Can any candid and ingenuous person suppose that I should, without being publicly called upon, have undertaken to give a correct account of the chemical evidence that was adduced on this important trial, and then have entered upon the task with a determination to conceal some parts, to garble and curtail others, and to misrepresent and pervert what had been adduced by the defendants' witnesses, for the purpose of making a false impression upon the public? Is it likely that any man of common understanding, even if he were destitute of all good principle, could act thus? and when it is considered that the experiments which I had made were corroborated by so many respectable and scientific gentlemen, and that a most intelligent jury had paid the highest possible compliment to our testimony by giving a verdict for the plaintiffs, I am sure it will not be believed that I could possibly have had any motive powerful enough to induce me to misrepresent a single circumstance respecting either the one party or the other.

In abridging Mr. Gurney's report of the trial, which occupies 248 closely printed pages of royal octavo, I was obliged to study

the utmost brevity to prevent my paper from swelling beyond the limits to which it was necessary to confine it, in order to render it admissible in this Journal; and when I read over the report of Wilkinson's evidence, I did not think that his account of his *last* experiment, after what I had said of the former, required any notice whatever, nor can I, on re-perusing it, perceive that I ought to have attached any importance to it. These associates, however, now please to say, that this was "the only experiment that was witnessed by the several scientific gentlemen who attended in behalf of the defendants;" but how could I possibly know this; not a word is said by the witness when relating the experiment, that any gentlemen were present; whereas when giving an account of the other two experiments that he was employed to conduct, he tells us that gentlemen were present and mentions their names. He says that for his last experiment, twenty-four gallons of oil were put in, and that "it was upon that oil that the gentlemen met afterwards to make experiments:" but how could I tell that this was the exact quantity of oil that was in operation when "the jerks, the concussions and the spouting of the oil" occurred. For, as the same witness had told us that in one experiment he operated upon thirty-three gallons of oil, it was natural for me to suppose that this was the quantity which had occasioned these phenomena; especially as the witness himself, who never hinted throughout the whole of his long evidence at the circumstance of the oil being thrown out of the boiler, had afforded no clue by which we could discover any thing respecting it.

No one surely, who has read the printed report of Wilkinson's evidence, can wonder at my not attempting to waste more time on such a testimony: for what dependence can be placed upon a man who tells us that he procured, at the end of a tube, inflammable vapour from whale oil heated only to 280°, and that the vapour took fire in sudden gusts as an explosion*; a man who had very recently been operating upon whale oil and noting

* See Mr. Gurney's report of the first trial, p. 146. According to my experiments oil vapour is not inflammable at the end of a tube, unless the oil be heated to the temperature of about 600° of Fahrenheit. S. P.

down the temperature of that oil day by day, and yet did not know whether his thermometer was graduated to 700° or 900°*—who doubted whether it was open at top or close†—and who had measured thirty-three gallons of *fresh* oil into an iron vessel which, according to his own shewing would not hold more than thirty-five gallons, and had contrived to keep the oil *within this vessel* for five or six days, though it was generally kept at a temperature of 400°; and every one who has made experiments on the expansion of oil, must know that thirty-three gallons of whale oil measured at the usual temperature of the atmosphere in the month of February (say from 40° to 50°) would, when brought to the temperature of 400°, have measured thirty-nine gallons. Thus it seems that this Mr. Samuel Wilkinson, by some wonderful "dexterity" which I am not acquainted with, contrived to keep thirty-nine gallons of oil in a vessel of the capacity of thirty-five gallons.—This Mr. Samuel Wilkinson was the operator in chief:—the man of whose accuracy they so proudly boast, "who has been long accustomed," they say, "to the preparation of various chemical products, and such as demand attention and dexterity‡." Who "has been in the habit of managing chemical processes of the most delicate kind;" and whom they affirm "they will challenge against Mr. Parkes himself for manipulation in experiment §."—And well they may!

Something also might be said of Mr. Wilkinson's assistant, in conducting these experiments, for he likewise must have his assistant. This man, who acknowledged that he "had never been employed upon a process of this sort before," as he "had been at sea most of his life-time‖," the Associates thought proper to put into the witness-box, to support Wilkinson's testimony, and when questioned by one of their own counsel, it appeared that he did not understand what was meant by the

* Mr. Gurney's Report of the First Trial, p. 147.

† In same answer, p. 147.

‡ See their book, entitled *Remarks*, &c., p. 13.

§ Ibid. p. 62.

‖ See Mr. Gurney's Report of the First Trial, p. 157.

graduation of a thermometer. I will not, however, insult the authors of the book, by giving a copy of his evidence. It is printed entire at p. 156 of Mr. Gurney's *Report of the First Trial*, and is a perfect chemical curiosity.

With regard to the fact which I stated, at p. 342 of the last *Journal*, respecting the expansion of oil, *viz.*, that "I had found by direct experiment, that whale oil expands more than one-fifth in bulk, when heated from 58°. to 460°," these Associates have exhibited a notable degree of unfairness and want of candour; inasmuch as they are desirous of casting a suspicion on the assertion, and yet have neither the courage to contradict it, nor yet the liberality to acknowledge its correctness. Hear them on this point. "Let us suppose for a moment," say they, "that Mr. Parkes has given a correct statement as to the degree of expansion which oil undergoes by heat*." "Mr. Parkes states, as already noticed, that oil, on being heated from 58° to 460°, expands one-fifth†." "Now, then, as to the quantity having nearly filled the vessel, so as not to allow for its expansion, which Mr. Parkes says he finds to be more than one-fifth in heating from 58° to 460°‡." "Allowing for a moment the accuracy of Mr. Parkes' statement, that oil when heated expands one-fifth in volume §." "Mr. Parkes *assumes*, that whale oil expands, &c.‖" "With respect to the expansion of oil, I believe that the estimate rests upon Mr. Parkes' own authority, &c.¶." In the name of common civility, I would ask, what can all this mean respecting an assertion of so simple a nature? Are these Associates all incapable, now that their experimenter is "absent in a remote part of the world," of finding other evidence?

The next thing to which I shall advert is, the apparatus that was employed by the defendants' chemists, for producing the results which they were desirous of detailing to the Court and Jury; and upon this question, the associates write thus: "The only material difference," say they, "between the apparatus

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* The Associates' *Remarks*, p. 6. † Ibid. p. 7. ‡ Ibid. p. 43.
§ Ibid. p. 47. ‖ Ibid. p. 51. ¶ Ibid. p. 51.

at the sugar-house, and that with which our experiments were tried, was, that a pump and sugar-pan were not attached to our oil-boiler*." And why were they not? Let us hear what the Solicitor-General said upon this point. "In our apparatus," said he, "we have a pump; there was no pump in this. Mark the importance of that. Will not the pump equalize the temperature? Certainly. Did it never occur to these gentlemen, full and abounding with science, that the pump might be a material part of this apparatus? Is it not most extraordinary, that they should never have made an experiment with that part of the apparatus; but it appears, as if by intention, they had studiously omitted it†."

They say, however, "there was no other difference in their apparatus, than the want of a pump and a sugar-pan." A more unfounded assertion was surely never made in any book that was designed to direct the judgment of the public. Let us see how this will bear examination. They varied in nothing they say, but in the want of the pump and the sugar-pan. Had they a leaden pipe sixteen feet long, fixed in the top of the oil vessel, to carry off any vapour that might be generated? Had they a chimney seventy feet high, into which their pipe was conducted, to convey away whatever might issue from that pipe, and thus prevent the possibility of danger? Did their fire-place bear the same proportion to their boiler, as that at the sugar-house did to theirs? or was it of twice the size? Was their oil vessel fixed, with regard to its distance from the fuel, and in other respects like the one at the sugar-house, or was it varied for the sake of producing different effects? Was there any similarity between a large quantity of oil-vapour collected in an inverted cask, and made to burn when the water had been separated from it, and a small stream of such vapour issuing from a small leaden tube, sixteen feet long; even supposing the oil were made hot enough to enable the vapour to rise to that height?

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* The Associates' book, p. 28.

† See Mr. Gurney's Report of the Trial, *versus* the Phœnix Office, p. 445.

But though their apparatus did actually differ in all these respects, they might, notwithstanding, have endeavoured to conduct their experiments in a way, the most likely to produce results similar to those which would have occurred from the usual management of the apparatus at the sugar-house.

Was this the case? No. Did they charge their oil vessel with a quantity of oil, that bore a proportion to it similar to that which the oil at Whitechapel bore to the capacity of the vessel belonging to Messrs. Severn and Co.? No. Did they heat the oil in a way which bore any resemblance to the manner in which the oil at Whitechapel was heated, in the ordinary process of boiling sugar? No. With all these variations, how could these associated witnesses come to a determination to tell the public that the only material difference between the two apparatus was in the want of the pump and the sugar-pan!!!

But this is not all. When they find that the vapour, which rises from the oil at the temperature of 600°, will not burn; what do they do,— they suspend a metallic still-head over the vapour, for the purpose of condensing the water which it contains, and then they find the vapour will burn at a temperature of 460°*. They have still a further expedient. They are desirous of producing some volatile oil in court, that will be more inflammable than common whale oil. How do they proceed to attain this object? They pass the common oil vapour through a worm which is 23 feet long, and immersed in water, for the purpose of condensing the water, which is always combined with oil vapour that is produced from oil at a high temperature; and then the product of the distillation is submitted to a second distillation, and from this a volatile and inflammable oil is procured †.

Was this the way that disinterested and scientific men might have been expected to proceed, for the purpose of ascertaining whether any thing was emitted from the safety-tube in the oil

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* See Mr. Gurney's Report of the Trial *versus* the Phœnix Office, page 291.

† Ibid. page 314.

vessel at Messrs. Severn's, that was capable of producing the conflagration! Had any of Messrs. Severn's men formed the intention of setting their masters premises on fire, by means of the oil vapour I doubt much whether, so ingenious a mode of separating the water from it, and rendering it combustible, would have occurred to them.

If the chemists, engaged on the part of the defendants, were all present to witness these experiments, I am astonished that those among them, who valued their reputation more than the support of a favourite theory, did not at once exclaim against the illiberality of such a proceeding.

Some of those gentlemen were probably not aware of the tendency of the experiments, or conceived it would be useless to protest against them; and therefore they continued,—

"Th' entangled slaves to folly not their own!"

Although I have thus endeavoured to explain the diversified nature of the numerous deviations from the original apparatus which these Associates seem now so anxious to disguise, I should still not do justice to the subject, if I did not advert once more to the opinion of the Solicitor General, who spared no pains to make himself complete master of every point that had any bearing on the question at issue. When speaking, in his address to the jury, of the chemists who were engaged by the Phœnix Insurance Office, he made use of these very remarkable words:—

"They seem," said he, "in their experiments, *studiously to have departed from this apparatus*, for the purpose, in some other way, under some other form, in some other mode, to deduce results and conclusions which never could be deduced from the apparatus itself, which is the only subject of your inquiry. And, Gentlemen, knowing what I do of these individuals; knowing what I do of their intelligence and their sagacity, it does appear to me that this has been an *intentional* deviation from the apparatus, from a kind of presentiment, or conviction, existing in their minds, that if they had adhered to the apparatus in question, it would not have led to those conclusions they have

given in evidence; and, of course, not to a conclusion consistent with the wishes and dispositions of their employers*."

Some of the chemists employed by the insurance offices had the candour to acknowledge in court, that, in selecting their apparatus, they had *intentionally* deviated from the model of that on the premises of Messrs. Severn, and Co.

"I asked Mr. Faraday, and I asked Dr. Bostock," said the Solicitor General, in his address to the jury, "why they did not adhere to the apparatus? and both of them said, their *object* was *different* †." Another thing, in which they varied very materially, was, that they took all possible pains to apply the heat with the utmost rapidity. "It was our intention," said Mr. Faraday, "to heat as rapidly as possible,—the oil was heated as rapidly as it could be done." Then says Dr. Bostock, "very much depends upon the rapidity of the application of the heat." Mr. Faraday is asked, "Was it not your object to heat the oil as rapidly as it could be done?" "Oh, certainly ‡." And yet these Associates tell the public, that no violent measures were resorted to for the sake of effecting their purpose.

"During the late trials, attempts were made," say they, "to promulgate an idea that violent measures had been used to extort the results described by those who witnessed these experiments; but we must say, these insinuations were totally groundless §."

Totally groundless, were they? How then do these gentlemen account for the size of their fire-place, which was more than twice as large, in proportion, as the one at the sugar-house? Why did they fix their oil vessel as near as could well be to the fuel? and why did they use the utmost exertion "to heat the oil as rapidly as possible?" Not to notice the artifice of the still-head, or of the worm-tub, or of the repeated distillations of the oil, as these, they will say, were not *violent* measures.

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* Mr. Gurney's Report of the Trial, Severn *versus* the Phœnix Office, page 441.

† Ibid. page 443.

‡ Ibid. page 444.

§ The Associates' Book, page 25.

"Can you, Gentlemen," said the Solicitor General, when explaining to the jury the *manœuvre* of this very rapid heating of the oil, "can you, Gentlemen, come to a conclusion, where the properties of men are at stake, on experiments made by a *departure* from the apparatus which is in use, and the substitution of another,—the persons making them pursuing with zeal and eagerness a particular object, and endeavouring to come to a conclusion, inconsistent with the practical conclusion which would be built upon the apparatus itself*?" And when the Solicitor General said to Mr. Faraday, "If the size of the fireplace should exceed, in the proportion of *two* to *one*, that in the apparatus, might it not produce very different results?" He candidly answered, "certainly†."

Having mentioned the name of Mr. Faraday, it is but justice to that gentleman to state, that he had the good sense and the virtue to resist all solicitations to become one of the party. I mean the party that associated for the purpose of writing this particular book, that was intended, as I have before explained, to divert the current of public opinion which certain individuals found was setting in so powerfully against them. Whether other chemists were solicited, I have not yet been able to learn.

When adverting to the observations which the Solicitor-General made upon the variations in the apparatus, I ought not to have omitted what he so forcibly expressed, respecting the omission of the long leaden tube, which was attached to the oil-vessel at Whitechapel. "You find," said he, when addressing the Jury, "you find attached to that apparatus, a pipe *sixteen feet long*. This does not come by surprise upon these gentlemen. Upon the last trial it was pointed out,—it was a matter of observation,—it was a matter of argument;—much was said upon the subject—they had their attention drawn to it; how extraordinary—nay, how miraculous is it, then, that in spite of all their experience, the length of the pipe has been studiously omitted‡." He added, "I *rest* upon that substantial part of the case, *the entire deviation from that apparatus*

* Mr. Gurney's Report, page 415.
† Ibid. page 299. ‡ Ibid. page 446.

*which forms the subject of our inquiry**." I have only a few more words to offer, and then I hope I may dismiss every consideration respecting the apparatus which has been chosen, either by the one party or the other. My ideas are briefly these:

Had Messrs. Severn and Co., afraid to trust to the justice of their cause, sought to gain their end by means which men of honour would not have deigned to employ;—had they, instead of resting on plain facts, searched this metropolis for persons willing to aid a system which might make the worse appear the better reason; what course would such a person have pursued? He would have procured a vessel, as unlike as possible to the oil-vessel in question,—much larger in size, with a tube four inches wide instead of one, and sixty feet in height in place of sixteen. Instead of communicating with one sugar-pan, it would have communicated with six or eight; and with a fire-place so constructed, and the oil in such quantity, that it would have been impossible to have raised the fluid to 400°.

Experiments under such circumstances might have been made. They might have been pompously detailed in court, and second-rate operators might have been brought forward as witnesses to those experiments, and their effects:—But what must the judge—what must the jury—what must the public, have said of such conduct? Would they have termed it the conduct of honourable men?

It is impossible for me to notice in the compass of a paper suitable for this Journal, all the absurd statements and groundless charges that appear in the book which the associated witnesses have published; but there are some parts, which have not yet been remarked upon, that must not be allowed to pass without observation. At page 31, they say. "All the changes which take place in the nature of whale-oil, when exposed to various temperatures, both for a short time and a long one, admit of easy and philosophical explanation; and it has surprised us, that

* Mr. Gurney's Report on the trial, Severn *versus* the Phœnix Office, page 451.

the subject should have been so much *obscured* by those whose business it was to throw light upon it." If the changes which they speak of, do actually take place in oil, and if they admit of such easy and philosophical explanation, why have they not condescended to explain them? They know full well what the public expects from them; how extraordinary then is it, that they have not given these explanations long ago, and put to the blush any one who might assert, that they found themselves in a deplorable difficulty, and not having courage to confess their errors, intended extricating themselves by the use of many words. Instead of a book of mere invective, had they published a volume of facts faithfully detailed, and such as bore properly on the question, the public would have given them credit for good intention, and would have been inclined to believe, that whatever was still mysterious, might possibly "admit of easy and philosophical explanation." But they are "surprised that the subject should have been so much obscured by those whose business it was to throw light upon it."

Yes, indeed, the subject has been obscured, and it may be observed, by the bye, that the difference in the results of those experiments, which were detailed by the Associates in court, was of itself enough to obscure the subject; to say nothing of the perplexity which their deviation in the construction of the apparatus threw into it. The public, however, know by this time, which party has obscured the subject.—it is evident also, that the Juries who tried the three separate actions which were brought by Messrs. Severn and Co., well knew—and that the Directors of the Globe Insurance Office well knew—as they not only completely abandoned the idea of danger from the oil-apparatus—the very thing which these Associates had for twelve months been labouring to establish, but to the utter discomfiture of these Associates, actually directed the counsel to declare in court, that they were perfectly satisfied, that this apparatus *lessened* the danger—and so convinced were the Directors of the Globe Fire Office respecting the question, whether these Associates, or the gentlemen with whom I act, had *obscured* the subject, that they did not think fit to put any one of these

associated witnesses into the witness-box, to ask him a single question on the last trial; although all of them were in court, in full expectation of being examined.

It may be added, that Messrs. Severn, King, and Co. have no *obscurity* on their minds respecting the subject; as they are at this moment actually preparing to renew the oil-process on a much larger scale than it was before, and are busily engaged in constructing the apparatus for that purpose. The oil-vessel, more than three times as large as the former one, is already finished; it has a capacity of more than one thousand gallons; and the sugar-pan, which is also under operation, will be of dimensions fully adapted to the size of the oil-vessel. Such is the answer which Messrs. Severn and Co. are preparing to give to the declarations and opinions of these Associates, respecting the *extreme danger* of using oil in a sugar-house, as delivered in court, on the 16th of December last, on the trial *versus* the Directors of the Phœnix Office—all which may be seen at pages 321, 345, 350, 354, 367, and 371, of Mr. Gurney's printed Report of that trial.

Some persons have the talent, when they find themselves in a difficulty, of making broad assertions without adducing any proofs. Almost every page of their work would furnish an illustration. Let those, however, who may have the book, look at the following instances: at page 36, is a charge of my having made "partial and distorted comments," and yet not a single example is adduced. At page 21, is a complaint of my not having "addressed the scientific public," without pointing out a more suitable path than that which I have pursued; or imparting the secret by which, independent of gratuitous distribution, they have contrived to circulate their book among the "scientific public." At page 55, they say, "He has so stated his case, as to lead his readers to draw false conclusions; this may be done without saying any thing strictly untrue, but the effect is as bad." This passage mischievously assumes an air of honest indignation; but would not the "scientific public" have been better satisfied, if they had taken the pains of trying to exhibit one or two instances of this venial mendacity?

At page 61, is a very solemn passage, "whoever will pervert what has been said for the purpose of his own argument, may expect severe animadversion when he is exposed." The trembling which seized me on reading this, can easier be imagined than described, but my apprehension subsided when I found that their threat, instead of being put into summary execution, "vanished into thin air;" and that the words were merely designed to close a paragraph handsomely and without trouble. Had they attempted to prove all they assert, their book would, perhaps, have distended itself to more than desirable dimensions. It was better, therefore, to register their ideas exactly as they occurred, and leave it to others to apply them. Thus as Dryden says:

> "They fagotted their notions as they fell,
> And if they rhymed and rattled, all was well."

In the same summary way do they attempt to make the public believe, that I was the editor of the report of the first trial, though they have no foundation whatever for their suspicions or insinuations. I was neither consulted respecting the printing of the report, nor had any concern whatever in editing it.

In page 65, we read, "Mr. Parkes makes me say, that I had four hundred thermometers of Pastorelli; that the one used in the experiments was one of them.' It is not so printed in the book, and if it had it would have been wrong; it shews how Mr. Parkes makes free with other people's words." To shew the nature of this exquisite quibble, read the words themselves, as taken down by Mr. Gurney. "Was it Pastorelli's thermometer," said Mr. Scarlet.—Answer, "Yes: we had four hundred made by him, and I proved some of them and found them good for common thermometers *."

That no opportunity of making an unfavourable impression might be neglected, they return to the charge, of my having in court accused one of them of having adulterated the oil on which he operated. This is often hinted at, and in page 16,

* Mr. Gurney's Report, p. 183.

they say, "We come now to another, and perhaps, not the least extraordinary, of Mr. Parkes' charges against the *scientific* witnesses to whom he was opposed. The first time he brought this accusation was in court during the trial." In reply, I have no intention of calling in question the title which these gentlemen give themselves of *scientific witnesses*; yet I must say, that if they had been ambitious of being considered fair and candid witnesses, they would have felt it to be obligatory upon them to have added what I said in court on this subject, and also what the learned Judge said in his address to the complainant from the bench; "I can assure you," said he, "that to me, instead of conveying any imputation on you, it was entirely the reverse; it went only to say, in spite of human skill and observation, there might be a mixture in oil, but that no person could be less suspected than you *."

As to the sample of oil which one of these associates thought fit to send to me in April, 1820, and the offence which they have taken at my returning it, I would ask any impartial person whether, if he had been professionally engaged as I was, in a cause where 70,000*l.* were depending in some measure upon the caution and prudence with which he acted, he would have received any sample of oil that an opponent might have chosen to send him, and would have undertaken to analyze it, and subject himself to be examined in court upon the result of that analysis; or whether he would not have returned it unopened, as I did? It will hardly be credited, but this frivolous complaint is made in three different parts of the Associates' book.

> "Laudes, Gaure, nihil; reprehendis cuncta, videto
> Ne placeas nulli, dum tibi nemo placet."

At page 63, they say what I confess did rather surprise me: "Wilkinson spoke of concussions in the boiler, which Mr. Parkes tells us *he* never witnessed; we do not think he did. *He* never perhaps, operated upon such a quantity of oil, at a temperature sufficient to produce them."

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* See Mr. Gurney's Report of the First Trial, pages 207, 209, and 212.

This passage reminds me of a very important experiment, at which I assisted in the month of December last; and which I think, notwithstanding the above assertion, the Associates themselves will allow to exceed, both in magnitude and importance, any of their experiments; besides its possessing an advantage which *none of theirs can boast*, viz., that it was made in an apparatus exactly similar, both in form and size, to the one which existed at the sugar-house at the time when the fire happened. The particulars are as follow:—

On the second trial, the chemists, who were examined on behalf of the defendants, advanced a new theory, viz., that if heat be applied to whale oil with *great rapidity*, and to a high degree, results very different from what might be expected in the common mode of heating will be obtained. In consequence of this, it was suggested that it would be advisable to ascertain what would be the effect of the rapid heating which they described, if tried upon the oil in the large vessel at Whitechapel, and which had been kept heated to 340° and 380° for forty-one days, and for ten hours in each day.

Accordingly, on Saturday evening, the 16th of December, while the trial was pending, Mr. Dalton, Mr. Cooper, Mr. Wilson, and I, went down to the sugar-house, for the express purpose of putting this new theory to the test of actual experiment. And in order completely to meet their assertions, we did not light the fire under the oil in the usual manner, but determined to prepare the fire previously, that it might be applied to the oil as *suddenly* as possible; and we proceeded thus:

At nine o'clock at night, a fire was lighted upon a blacksmith's hearth, which, fortunately, was situated close to the room in which the oil-vessel stood. This fire was urged by the smith's bellows until the fuel was completely lighted up, and then it was put at once from thence into the fire-place, over which the oil-vessel stood; and at that time the vessel contained more than 100 gallons of that oil, which had for so long a time been submitted to the repeated heating already mentioned.

While the fire was preparing, two long thermometers, highly graduated, were fixed in the vessel, one of which was merely

immersed in the oil, while the other was made to rest directly upon the bottom of the oil vessel, and both dipped into that part of the vessel which was immediately over the fire.

When every thing was thus arranged, Mr. Cooper undertook the management of the fire; and, by close attention thereto, the heat was brought up as rapidly as possible; while another of our party attended to note down the increasing temperature of each thermometer every five minutes. At similar intervals one or two of us attended to the leaden tube, which issued perpendicularly from the top of the oil vessel to the height of twelve feet, in order to examine the nature of the vapour which issued from it. It is necessary to observe, that this tube was originally sixteen feet long, but the end of it having been accidentally injured by the workmen, we cut off four feet before we commenced our operations.

In consequence of the great fire which was kept up, the oil very soon began to give out a large quantity of vapour, the nature of which was examined every five minutes, by the application of lighted candles and pieces of ignited deal; but in no instance did we find any thing that was inflammable issuing from the tube; but, on the contrary, every ignited substance was immediately extinguished.

When the oil attained the temperature of between 400° and 500°, the smell became very offensive, and the vapour which issued from the leaden pipe was very abundant, but it was still uninflammable.

All this time the fire was urged as much as possible, and was so intense, that the door of the furnace was continually red-hot. Every five minutes the temperatur of the oil, as indicated by the two thermometers, was noted down, and two of us went up stairs to examine the safety-tube connected with the oil vessel, and though vapour was constantly passing through it, in no instance did we find it inflammable, but the reverse.

When the temperature of the oil was raised to something under 550°, I was obliged to desist from trying the inflammability of that which issued from the tube, as the large room, which is fifty-four feet by sixty feet, and nearly twelve feet

high, had become so full of vapour, which was so disagreeable and oppressive to the lungs, that I could not breathe in it. It is further observable, that this experiment was made in a part of the newly-erected sugar-house, before any of the windows had been put in, or the outer door hung; circumstances which allowed of the escape of much of the vapour into the atmosphere.

When the oil had acquired the temperature of nearly 580°, Mr. Dalton also was obliged to desist from carrying a light to the orifice of the safety tube, the suffocating effect of the oil vapour was so intolerable.

It was now found very difficult to raise the temperature, though the draught of the fire-place was very great, and the fire was made as large as possible. Still, Mr. Wilson and Mr. Cooper persisted in going every five minutes across the large room already mentioned, to apply a lighted candle or a lighted strip of deal to the end of the safety-pipe: but in none of these trials did these gentlemen find the vapour in the least inflammable.

During the whole of this time, the stench continued to increase, although the mercury in the thermometers mounted slowly; and when the oil was brought up to 600°, the two gentlemen last named were compelled to say, that they were unable to go through the cloud of vapour any more, for the purpose of applying a lighted candle to the end of the tube.

Being still desirous of prosecuting the experiment as far as possible, we had no means left but to engage two of the men who had long been inured to the heats and offensive smells of a sugar-house, whom we instructed to go with a lantern across the room while we stood on the outside of the door to witness their application of lighted candles to the end of the tube. This was done every five minutes, and we took care every time to place ourselves in such a situation that we could witness their going up to the tube, and applying a light to the orifice. In every trial the vapour extinguished the light, instead of being itself ignited by it.

In this way, by dint of painful perseverance, the oil was brought up to the temperature of 610° without our having perceived the least sign of the inflammation of the oil vapour. We were, however, now under the necessity of closing the experiment, in consequence of the luting which stopped the holes in the arched roof of the oil vessel having cracked, and allowed the vapour to come into the room where the vessel stood. This accident occasioned such an intolerable stench in the room, where there was no window or second opening through which it might have been ventilated, that no person was able to remain in it a sufficient time for keeping the fire up to the necessary pitch. This was attempted in several ways, but all our endeavours to raise the oil beyond 610°, proved fruitless. It must however be mentioned that when the oil had acquired the greatest heat which we were capable of giving it, the vapour that issued from it was still unnflammable, and always extinguished the light when presented to it.

It required two hours and ten minutes to bring this large body of oil to the temperature of 610°; and during the latter part of this time the neighbouring streets were, to all appearance, full of vapour. The process occasioned a general astonishment throughout the vicinity of the sugar-house, in consequence of the smell, by which the inhabitants of the houses round about were annoyed. The quantity of coal consumed was somewhat more than five bushels.

This experiment, unfortunately, was not made until all the evidence for the plaintiffs had been gone through, or the particulars of it would have been related in court; as it must be considered, by every impartial person, who has attended to the subject, to afford a complete answer to all the theories which have been brought forward to prove that the vapour, or gas, from the oil apparatus had set fire to the buildings in which it was placed.

Thus I have endeavoured to reply to the principal parts of the work which these Associates have thought proper to publish. Their book has evidently not been written for the purpose of en-

lightening the public——not for the purpose of making the best apology they could to Messrs. Severn and Co. for the obscurity in which they had involved the question between them and the Insurance Offices——not for the purpose of making any apology to the Directors of the Insurance Offices for having, by means of fallacious experiments and illusive speculations, induced them to entail upon themselves enormous expenses in trying in the courts of law the validity of the claims of the insured——not for the purpose of apologizing to Mr. Wilson for the pecuniary losses which they had occasioned to him by the unmerited odium which they had cast upon his Patent-invention——not for the purpose of apologizing to the Philosophical Public for the uncertainty and temporary disgrace which their proceedings had cast upon the science of chemical philosophy——not for the purpose of explaining why they were induced to operate with apparatus so totally dissimilar to the original apparatus——nor for the purpose of detailing some experiments and results, until then untold, which had tended to mislead themselves and their employers ——but they have laboured solely with the vain hope of injuring an individual for the part he had taken to discover where the truth lay, and to make that truth manifest to the public.

Having now nearly finished what I intended, I earnestly call upon these Associates to ask themselves seriously, whether they ought not to make ample apologies, and give satisfactory explanations to all parties concerned; for I can assure them, the Scientific Public will require it; unless they can make such an exhibition of new, pertinent, unanswerable, and conclusive experiments as will carry conviction to the minds of all, that their opinions were founded in truth—and ours in error.

But, if the experiments of these Associates have already elicited any new facts to enrich the Science of Chemistry;—if their investigations have in any way tended to illustrate the case for which they were undertaken, the experimenters have been most unfortunate indeed. Like Cassandra, they have

repeatedly prophesied, and like her, have been fated not to be believed.

> "Tunc etiam fatis aperit Cassandra futuris
> Ora, Dei jussu non unquam credita Teucris."

I am aware that there are but few occasions on which I ought to speak of myself in public; but, called upon as I have been, I trust I may be excused in saying, that nothing can ever deprive me of the satisfaction which results from the recollection of the successful exertions which I have made, in conjunction with others, to expose the absurdity and dangerous tendency of a series of extravagant and delusive theories, which had been promulgated for the purpose of propping an unworthy cause; and which would, if either of them could have been established, have had the effect of depriving a company of deserving individuals of no less a sum than sixty-three thousand pounds, to which they were in law and equity entitled.

It is also no small source of gratification to me to reflect, that I have in some measure been instrumental in warding off the attacks which had been so repeatedly aimed at the Patentee of the Oil Apparatus, and also of proving to the satisfaction of the public, that his process for boiling sugar and many other substances by means of heated oil, is not only ingenious and easy of application, but perfectly safe and economical; and that, in a variety of ways, it is likely to become one of the most useful inventions that has for many years past been presented by Philosophy to the Arts.

Art. XII. *Proceedings of the Royal Society of London.*

The following papers have been read at the table of the Royal Society since the Christmas vacation.

January 18, 1821.—An account of the comparison of various British standards of linear measure, by Captain Henry Kater, F.R.S.

An account of the urinary organs and urine of two species of the genus *Rana*. by John Davy, M.D. F.R.S.

January 25. An account of a micrometer made of rock crystal, by Mr. George Dollond.

Feb. 1.—The Bakerian lecture: on the best kind of steel and form of a compass needle, by Captain Henry Kater.

Feb. 8.—Notice respecting a lunar volcano, by Captain Henry Kater.

Feb. 15.—An account of observations of the eclipse of the sun on the 7th September, 1820, with the equatorial sector belonging to the Radcliffe Observatory, by the Reverend Abram Robertson, D.D. Sav. Prof. Astron.

Feb 22.—A further account of fossil bones, discovered in caverns in the limestone rock of Plymouth, by Joseph Whidbey, Esq.

March 1.—On the aëriform compounds of charcoal and hydrogen, with an account of some additional experiments on the gases from oil and coal, by William Henry, M.D.

March 8.—On the length of the seconds pendulum in different latitudes, by Captain Edward Sabine.

March 15.—Observations on naphthaline, a peculiar substance resembling a concrete essential oil, which is apparently produced during the decomposition of coal tar by exposure to a red heat, by J. Kidd, M.D. Professor of Anatomy at Oxford

March 21.—On the papyri of Herculaneum, by Sir H. Davy, bart. P.R.S.

March 28.—On the aberration of compound lenses and object glasses, by J. F. W. Herschel, Esq.

An account of the skeleton of the Dugong, by Sir E. Home, Bart.

Art. XIII. ANALYSIS OF SCIENTIFIC BOOKS.

1. *A System of Chemistry, in Four Volumes, by* Thomas Thomson, M. D., *the Sixth Edition, London*, 1820.

In conjecturing the future fortunes of mankind, from their past and present condition, moralists have confined their views chiefly to political and religious considerations. The diffusion of general literature has been regarded by many as of equivocal benefit, since in this respect the press has been too often a pander to the basest propensities of our nature. But there is one ameliorating power, of modern growth, which has been almost entirely overlooked, though its influence is great, and unalloyed with evil. The cultivation of chemical science, by all ranks, from the archduke to the artisan, has spread a spirit of tranquil research and philanthropic sympathy, through the whole family of Europe. In each of its enlightened states, there is a society, respectable by its numbers, talents, and virtues, who are ardently devoted to this fascinating and fruitful study; and who find in its pursuit, an inexhaustible source of intellectual vigour and delight. Every new discovery and improvement being a real benefaction and positive increase of enjoyment, to the whole chemical world, excites in every well-constituted bosom, a feeling of gratitude and friendship towards the successful investigator, in whatever country or condition he may be placed. Hence, though human infirmity has introduced a few blemishes into the history of modern chemistry, its career has not been disgraced by such angry and jealous defiances as were banded about over Europe a century ago, by Leibnitz, the Bernouillis, and other leading geometers. With some trivial exceptions, resulting from temper and situation, the chemical history of our time exhibits a fairer picture of human nature and purer patterns of liberality, truth, and justice, than can be paralleled in any extensive association, since the primitive Christian church. What a contrast between the cordial co-operation of European chemists, and the polemic sectarianism of the philosophers of Greece. The names of Davy, Wollaston, Berthollet, Gay-Lussac, Berzelius, Klaproth, and Werner, create a regard even for the country which each respectively adorns, and will minister to its renown, in ages and regions too remote to feel any interest about those bustling spirits, who now flutter round the summit of its political pyramid. Into what insignificance do the grandees and demagogues of ancient Syracuse dwindle, in the venerable presence of Archimedes!

The activity of the modern chemical world is no less remark-

able than its liberality. The busy fermentation of its spirits is strikingly manifested in the prodigious multitude of chemical journals and compilations, which annually appear. These indicate a corresponding multitude of scientific readers. Besides the number of general students, who, from the cloisters of classical literature, and of geometry, have been allured to enter the temple of the modern Hermes, by the singularity, splendour, and importance of the trophies which it displays; an immense crowd of votaries have been summoned from the manufacturing classes of society. Chemistry has thus created a new population of practical philosophers, who reason more profoundly and accurately than the old masters of Grecian wisdom. Every metallurgist, bleacher, dyer, calico-printer, vitriol or soda manufacturer, *&c.*, who aims at precision and perfection in his processes, becomes a student of chemical science, and follows, with a lively interest, every discovery which may bear, in any way, on his peculiar art.

While chemistry invites the scholar to the laboratory of nature, by the wonders which she is ready to reveal, and the manufacturer by the hope of applying to pecuniary advantage the secrets thus acquired, there is no formidable obstacle placed at the entrance-gate. The candidate of mechanical science must, on the other hand, plod first of all through the fatiguing and intricate avenues of mathematics, before he can become a successful student. He derives but a few general principles from observation and experiment; and from these he must obtain, by mathematical procedure, an infinite number of deductions. The chemist can but rarely venture to employ the rigid methods of geometrical research. His general facts are not numerous; and are modified by a thousand peculiarities which experiment alone can ascertain. Mechanical science considers the most general qualities and actions of bodies; chemical science, the differential and specific. The former launches out at once into the wide ocean of research; and, guided by her quadrant and compass, discovers new lands. The latter steers along a strange and ever-varying shore, with the plumb-line in her hand, and examines at every turn, the structure of the coast and the nature of its productions. A few spirits, impatient of this servile, though sure navigation, have stood venturously out to sea, trusting to their charts and instruments, but after a vague and perilous voyage have either returned into soundings, fatigued and disappointed, or have perished in the vortex of hypothesis.

If we estimate the proportion of chemical students, in a country, from the number and variety of its chemical publications, Great Britain would seem entitled to high pre-eminence. We possess five journals devoted in a great measure to chemistry, besides the Transactions of the different scientific societies in which chemistry and its subordinate studies occupy a pro-

minent place. Our press pours forth, almost annually, rudiments, catechisms, elements, principles, manuals, dictionaries, systems of chemistry, and conversations on it, in great profusion; of which the successive editions prove the demand for them to be constantly renewed. We have, at least, six respectable chemical compilations, by different persons, while the French nation are satisfied with one treatise, that of M. Thenard.

Over all the British compilers, Dr. Thomson claims precedence. Some of the others are content to transcribe from his collection, but he seldom or never condescends to pay any of his brother compilers a similar compliment. Possessing the minute patience of an index framer, rather than the enlarged capacity of a systematist, he has contrived to bring together, in his successive editions, a great number of chemical facts, with copious references, convenient to the student, and imposing on the general reader; but in our opinion not entitling his work to be called a *System of Chemistry*. The account of this science which he drew up for the supplement to the *Edinburgh Encyclopædia*, which appeared about 1800, presents all the peculiar qualities of Dr. Thomson's manner of writing, and is by far the best compend of chemistry which he has yet offered to the public. When the detail of phænomena is condensed within such a compass, his scholastic divisions and subdivisions on the Peripatetic plan, answer very well; and form a convenient *catalogue raisonnée* of the facts. But we think that he has failed on attempting to extend that sketch into a system. Whenever he begins to generalize, his technical decision of manner leaves him, and, to the surprise of the readers of those clear details, which he had merely transcribed from experimental chemists, he becomes obscure and contradictory. To this defect, a more serious fault has been added; and which, progressively gaining force, has of late grown almost intolerable; we mean, the preference of hypothesis to fact on innumerable occasions, so that it is difficult for the experienced chemist, and impossible for the tyro, to distinguish between them in his works.

In his earlier editions, Dr. Thomson was content to transcribe, with decent fidelity, the researches of practical chemists, and thus acquired deserved reputation as a compiler. When, however, he commenced his *Annals of Philosophy*, he assumed a new character; erecting himself into a supreme judge of all scientific publications, he dealt forth his praise and censure with a dogmatism, very imposing on superficial minds. His annual summaries of chemical improvements apportioned to each chemist his share of public reputation; and though flimsy and incorrect, were regarded by tyros, as specimens of singular sagacity. Even these, however, have since found out, that keenness of temper is not synonymous with philosophical acumen; for there

is scarcely a single determination of Dr. Thomson's on any chemical subject of difficulty, during the last eight years, which has not been reversed. The angry tone, moreover, which he has of late introduced into scientific discussion, merits castigation; as it tends to disturb the harmony which, with slight exceptions, has long prevailed among all the eminent cultivators of chemistry.

Every contributor, indeed, to the general stock of knowledge, however slender his contribution, should be viewed with friendly eyes, unless his motives be obviously corrupt and his communication either equivocal or calculated to mislead. But above all things, we ought to receive with gratitude every gift from the distinguished votaries of the science, to whose disinterested genius its astonishing progression is due. Such men ought not to have their researches controverted on frivolous grounds, or be treated with contumely and petulance.

Dr. Thomson's attacks on the exalted reputation of the President of the Royal Society have long excited our surprise and indignation, and as we observe them still persevered in, and still unanswered, we shall use our humble endeavours to expose their injustice and futility. In the second volume of the *Annals of Philosophy*, page 33, he says, "Sir Humphry Davy has embraced the Daltonian theory, with some modifications and alterations of terms; but his notions are not quite so perspicuous as those of Mr. Dalton, and they do not appear to me so agreeable to the principles of sound philosophy." Sir H. Davy's view is the simple representation of facts; Mr. Dalton's is mixed with hypothesis; *which* is most consonant to *sound philosophy*, we leave our readers to determine. In Dr. Thomson's comment on the claim of Mr. Higgins to the atomic theory, we have the following passage: "I wrote the note which has occasioned all this discussion, because I thought Sir H. Davy treated Mr. Dalton *harshly and unjustly*, in the notes to which I had formerly alluded. I was not ignorant of the reasons which prepossessed Davy against him, and his notes struck me as something like an attempt to crush him by the superior weight of his own name and situation*." But the full force of Dr. Thomson's hostility was exerted in depreciating the Miner's Safety-Lamp. In reporting the proceedings of the Royal Society for January 11, 1816, Dr. Thomson, speaking of the property of a wire-sieve to intercept flame, says, "This is certainly one of the most extraordinary and *unaccountable* facts connected with the propagation of heat and combustion. It is possible *(supposing the fact to be correct)* that so great an attraction may exist between the wire and the air surrounding them, that the internal combustion and expansion is not able to

* *Annals of Philosophy*, v. IV. p. 65

displace it. If we suppose such a fixedness to exist, it would account for the explosion not kindling the surrounding mixture on the outside of the sieve. This contrivance *(supposing it effectual)* would completely answer the purpose of the miner*."

The Doctor's insinuations and theory are of a piece. In Sir H. Davy's *previous* paper in the Transactions, reprinted in the *Philosophical Magazine*, for December, 1815, we find the following luminous passage: "In comparing the power of tubes of metal, and those of glass, it appeared that the flame passed more readily through glass tubes of the same diameter; and that explosions were stopped by metallic tubes of $\frac{1}{2}$ of an inch, when they were $1\frac{1}{2}$ inch long; and this phœnomenon probably depends upon the heat lost during the explosion, in contact with so great a cooling surface, which brings the temperature of the first portions exploded, below that required for the firing of the other portions. Metal is a better conductor of heat than glass; and it has been already shewn, that the fire-damp requires a very strong heat for its inflammation." So much with respect to the fact being "*most unaccountable*," at the time when Dr. Thomson wrote the above report. The absurdity of his own theory will appear, from a decisive experiment in Sir H. Davy's *preceding* paper. "But I was not contented with these trials, and I submitted the safety canals, tubes, and wire-gauze sieves, to much more severe tests; I made them the medium of communication between a large glass vessel filled with the strongest explosive mixture of carburetted hydrogen and air, and a bladder $\frac{3}{4}$ or $\frac{1}{2}$ full of the same mixture, both insulated from the atmosphere. By means of wires passing near the stop-cock of the glass vessel, I fired the explosive mixture in it, by the discharge of a Leyden jar. The *bladder always expanded* at the moment the explosion was made; a contraction as rapidly took place; and a lambent flame played round the mouths of the safety apertures, open in the glass vessel; but the mixture in the bladder did not explode." Here the air was displaced, but not kindled; in direct confutation of Dr. Thomson's theory, and in justification of Sir H. Davy's expression, *chemical fire-sieve*.

The pages of Dr. Thomson's Annals became for some time thereafter, the receptacle of much criticism, and invective, against the safety-lamp and its inventor. Since that period, however, Doctor Thomson has set up as the Autocrat of Chemistry, assigning to each of his cotemporaries, the rank he ought to occupy, with despotic decision. Of M. Gay-Lussac, he says, "We may pity the pusillanimity†;" and he arraigns M. Thenard with downright dishonesty, as we shall shew in the sequel. "Mere experimenters," says he, "may relinquish the field; for there is not a great deal more, which they

* *Annals of Philosophy*, Feb. 1816, p. 135. † Ibid. Jan. 1816.

can do, and to mere mathematicians, chemistry at present, and probably for some time to come, is forbidden ground*." How does this ridiculous interdiction against mere experimenters tally with his panegyrics on Mr. Donovan's merely experimental results, on the oxides of mercury, which are incompatible with the atomic theory, as taught by Dr. Thomson? "The methods followed by Dr. Wollaston," adds he, in the same paper, "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†." Or, in other words, Dr. Thomson is the only chemist whose methods are susceptible of any great degree of precision. All the rest "may relinquish the field." These interdicted chemists are much obliged to him for the information, that "their analytical researches have been guided entirely without any general principle." The world happens to be of a different opinion with regard, at least, to the celebrated author of the equivalent scale.

Having depreciated most of his eminent cotemporaries in detail, the next step was to attack them *en masse*. Accordingly, he drew up an analysis of a book‡, which he evidently did not understand, that he might have an opportunity of attacking the most illustrious scientific association in Europe, the Royal Society of London. The accusation of haughtiness, as made by Dr. Thomson, is quite comical, as well as his code of instructions to the Council of that Society. "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-men-

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* *Annals of Philosophy*, Sept. 1820. p. 161.
† Ibid Sept 1820. pp. 176. 177
‡ *Barlow's Essay on Magnetism*

tioned, 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 good qualities, but certainly not for impartiality. 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*."

It is not difficult to discover one source of Dr. Thomson's animosity against that illustrious body, in addition to his desire to be considered the most exact and impartial chemist of the age. While its records hold forth to admiration, the discoveries and improvements of so many of his cotemporaries, they do not present one memoir of his, which posterity will respect. His paper on oxalic acid we shall consider in the sequel. It would have been well for his reputation, had it been quietly withdrawn; a favour permitted in 1816, to its fellow in error, on phosphoric acid and the phosphates.

We also observe, that in all his recent communications to the public, he carries to their utmost extent, *into literature*, Berthollet's principle of attraction,—that mass may compensate for weakness of chemical force. The whole information contained in his four papers on the specific gravities of the gases, and the true weights of the atoms, might have been easily conveyed in one-twentieth of the compass.

This preliminary developement will enable us to understand the composition of his *System*; the following survey of which we shall endeavour to render instructive to our readers, and useful to chemical science.

And, in the first place, we may apply to our modern atomist Bacon's censure of his prototype Democritus. *Ille enim ita versatur in* particulis *rerum, ut fabricas fere negligat*†. In a *critique* on the preceding edition of Dr. Thomson's work, we pointed out the want of systematic arrangement, and the absurdity of his general divisions; which, as we expected are retained in the present. "This work, therefore," says he, "will be divided into two parts: The first will comprehend the science of chemistry, properly so called; the second will consist of a chemical examination of nature‡."

This distinction is preposterous in the extreme. All the physical sciences are merely examinations of nature, and the science of chemistry, minutely so. *His* notions of chemistry must be strangely confused, who thinks he can first construct the science,

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* *Annals of Philosophy*, Oct. 1820. pp. 296, 297.
† *Novum Organum*, Lib. I. Aph. 57.
‡ *System*, I. 10.

and afterwards enter on a chemical examination of nature. As in every sheet of his four volumes, he violates the rules of inductive logic, we cannot suppose him versant in the great work of Lord Bacon. Had he read the first aphorism, he would never have ventured on so vicious a division of chemistry. "Homo naturæ minister et interpres, tantum facit et intelligit, quantum de naturæ ordine, re, vel mente, observaverit: *nec amplius scit aut potest.*" Hence, if his first part be not an examination of nature, it is worthless. Why should ulmin, nicotin, emetin, asparagin, cerasin, inulin, starch, indigo, gluten, pollenin, fibrin, olivile, medullin, fungin, and all his other *ins*, be discarded from the science of chemistry, *properly so called;* while morphia, strychnia, brucia, picrotoxia, delphia, the acids, benzoic, boletic, moroxylic, meconic, oxalic, mellitic, citric, isaguric, krameric, ellagic, gallic, with tannin, oils, fats, bitumens, and many other *native* products, are all removed from his chemical examination of nature? The consequence of this violent disruption and dislocation of objects, naturally allied in their origin and composition, is a perplexity and prolixity, disgusting to the reader, and worthy of the darkest days of the schoolmen, from whose endless artificial distinctions Dr. Thomson's are manifestly copied.

He has reprinted the preface of 1817, in which he says, "concerning the arrangement which I have adopted, it appears unnecessary to say much. It is merely an improvement of the arrangement, followed in the preceding editions of this work." We agree in thinking, that it is not for the Doctor's interest, "to say much" about his arrangements; but we differ entirely as to the pretended improvement. It is, on the contrary, a deterioration, as we shall presently prove. In a brief "advertisement to the sixth edition," *he* says, "the additions have been very numerous, and will be found of considerable importance." Now, *we* find the additions to be trifling, and for the most part unimportant. Of the 2600 pages contained in his four volumes, there are not fifty new written; while the grossest errors and mis-statements are reprinted from the former edition, with book-making despatch. Indeed we are at a loss to learn why a new edition has come forth. It was not spontaneously called for, and nothing but a decidedly superior work should have been tendered to the public. Instead of which, the present book is, relative to the actual state of the science which it professes to represent, incomparably worse, and more defective than any preceding edition. In every thing regarding the philosophy of chemistry, or the developement of general principles, it is ten years behind. Even the atomic theory, which Mr. Dalton placed under his protection, is expounded in a confused and partial manner.

We shall now proceed to support these allegations by individual examples. His introduction is a poor specimen of composition. "As soon as begins to think, and to reason, the

different objects which surround him on all sides *naturally* engage his attention. He cannot fail to be struck with their number, diversity, and beauty; and *naturally* feels a desire to be better acquainted with their properties and uses." His division of science into two great branches is awkwardly copied from Professor Robison*; "the first comprehending all those natural events, which are accompanied by *sensible* motions; the second, all those which are *not* accompanied by *sensible* motions. The first is *Natural Philosophy;* the second is *Chemistry.*" "Chemistry, then," he subjoins, "is that science which treats of those events or changes in *natural* bodies, which are *not* accompanied by *sensible* motions." Are chloride, iodide of azote, and the detonating metals, *natural* bodies? And, are fusion, evaporation, effervescence, combustion, explosion, *&c.* events, *not* accompanied by *sensible* motions? On the other hand, the whole doctrines of statics are independent of sensible motions. Change of interior constitution is the criterion which distinguishes chemical, from mechanical, action. When this change does not supervene, the event should be referred to natural philosophy. Thus the study of doubly-refracted and polarized light, belongs to physics; that of the oxidating and hydrogenating rays, to chemistry. A similar distinction ought to be observed with regard to the distinct actions of both electricity and caloric.

"The science," says Dr. T., "therefore *naturally* divides itself into three parts: 1st. A description of the component parts of bodies, or of *simple substances*, as they are called; 2d. A description of the compound bodies, formed by the union of simple substances: 3d. An account of the nature of the power which produces these combinations. This power is known in chemistry by the name of affinity. These three particulars will form the subject of the three following books." The above enunciation constitutes nearly the whole of one head, entitled *Principles of Chemistry*, which stands at the beginning of the work. Now, one might certainly expect some general ideas on the *principles* of chemistry, to usher in the details of simple and compound substances, whose elimination and combination are to occupy the first two volumes. Yet no preliminary explanation whatever is offered, to guide the student through this intricate maze. To supply this grievous omission, should he have recourse to the third volume, where affinity is discussed, he will derive little benefit; because the subject is treated in a manner unintelligible by the beginner, and unsatisfactory to the proficient. Nothing can be more untrue, therefore, than his assertion in the preface, of "its being better adapted to convey a clear idea of the present state of the science, in all its bearings,

* *Encyclopædia Britannica*, Artic. PHILOSOPHY.

to the tyro, who is just commencing the study, than any other that I have yet seen." On the contrary, we affirm, without fear of contradiction from any chemist of the age, that it is *worse adapted for the tyro, who is just commencing the study*, than any other compilation which we have yet seen.

Dr. Thomson divides his simple bodies into two classes; *imponderable* and *ponderable*. The former are light, heat, electricity, and magnetism; the last of which he declines to speak of as being foreign to chemistry. But surely magnetism has a closer connexion with the science, particularly in its metallic and mineralogical department, than many of his trite transcripts, on the velocity, refraction, and reflection of light, from elementary books of physics. On the article light, we have few remarks to offer, except to complain of its darkness. The unintelligible paragraph on *polarization* shews that the Doctor does not understand one iota of the subject, as indeed the world has been long assured, from his reports of papers on polarized light inserted in the *Annals of Philosophy*. His theory of the colours of bodies is very characteristic of his style. "Some absorb one coloured ray, others another, while they reflect the rest. This is the cause of the different colours of bodies. A *red* body, for instance, reflects the *red* rays, while it absorbs the rest. A *green* reflects the *green* rays, and perhaps also the blue and the yellow, and absorbs the rest. A white body reflects all the rays, and absorbs none; while a black body, on the contrary, absorbs all the rays and reflects none*." Moliere's professor theorized with equal spirit and profundity;

Domandabo causam et rationem quare opium facit dormire.
Mihi a docto doctore, domandatur causam et rationem, quare opium facit dormire? A cui respondeo, quia est in eo, virtus dormitiva, cujus est natura, sensus assoupire.
Bene, bene, bene respondere! Dignus, dignus est intrare in nostro docto corpore †.

The change produced on plants by the sun, he ascribes gratuitously and erroneously to the absorption of light. "The third and not the last singular of its peculiar properties, is, that its particles are never found cohering together, so as to form masses of any sensible magnitude‡. "Its particles *repel* each other, while the particles of other bodies *attract* each other; and accordingly are found cohering together in masses of more or less magnitude." This is sad prosing. Have the sun and stars no sensible magnitude? Do the particles of gaseous bodies "cohere together?" He states the velocity as a *peculiar* property of light, though afterwards, with due inconsistency, he ascribes the same velocity to caloric. "Light is emitted in every case of combustion." If Dr. Thomson had been well versed in modern discovery, he would have said, "Light is *not* emitted in every

* Vol. I. page 20. † *Le Malade Imaginaire*. ‡ *System*, I. 23.

case of combustion." Has Dr. T. never heard of *invisible* combustion? We refer him to the *Philosophical Transactions for* 1817, Part I. The Doctor's enumeration of the sources of light is very defective. Why does he not mention among them gaseous expansion, as shewn in the interesting experiment of bursting in vacuo a spherule of thin glass, containing air?

The commencement of caloric is a fair specimen of Dr. Thomson's style, when he writes from his own resources. "Nothing is more familiar to us than *heat;* to attempt, therefore, to define it is unnecessary. When we say, that a *person feels heat*, that a *stone is hot*, the expressions are understood without difficulty; yet in each of these propositions, the word *heat* has a distinct meaning. In the one it signifies the *sensation of heat;* in the other, the *cause* of that sensation. This ambiguity, though of little consequence in common life, may lead in philosophical discussions to confusion and perplexity. It was to prevent this, that the word *caloric* has been chosen to signify the *cause of heat.* When I put my hand on a hot stone, I experience a certain sensation, which I call the *sensation of heat*; the cause of this sensation is *caloric**." By the aid of many Italics, the Doctor tries, but in vain, to give emphasis to his favourite mode of writing, which from its extreme rarefaction of ideas, might be called the *vacuous.* Conscious that his long dissertation on heat, was, in most respects, identical with that in the former edition, he has distorted its arrangement to make it pass for a new article. In the fifth edition, the subjects were discussed with some shew of method. "I shall divide this chapter (on heat) into six sections: the first will be occupied with the nature of caloric; in the second, I shall consider its propagation through bodies; in the third, its distribution; in the fourth, the effects which it produces on bodies; in the fifth, the quantity of it which exists in bodies; and in the sixth, the different sources from which it is obtained †." "I shall divide this chapter into eight sections. In the first, I shall consider the phenomena of expansion, because they have furnished us with an instrument to which we are indebted for all our accurate notions respecting heat; I mean the thermometer; in the second section, I shall consider the *changes of state* induced in bodies by heat, and endeavour to deduce the laws upon which these changes depend; in the third section, I shall treat of the radiation of heat; in the fourth, of its *conduction* through bodies; in the fifth, of specific heat; in the sixth, of the laws of cooling; in the seventh, of the nature of heat; and, in the eighth, of the sources of heat‡."

A more involved distribution of a scientific subject was never before presented to the public. Radiation, conduction, laws of

* *System*, I. p. 26. † 3d Edition, I. p. 26. ‡ 6th Edition, I. p. 26.

cooling, and specific heat, all naturally belong to one head,—the *distribution* of this power. Expansion (or, in its general enunciation, change of volume) and change of state are two of its *effects*. As to the *nature* of heat, Dr. Thomson was well aware that he had no definite information to communicate. Its sources scarcely require a distinct section, for they would spontaneously spring out of the previous discussions, if they were skilfully handled. He has at last discarded the whole of his speculations, concerning the absolute quantity of heat in bodies, or the natural zero, and we wish he had applied the same salutary pruning to his preface on combustion. His table of the expansions of air, in which unity is placed at 32° and 1.375 at 212°, is of no use to the practical chemist. It is merely a transcript from that *rare* work, called *A Ready Reckoner*, the tables of which the Doctor seems fond of reprinting; for in his *Annals for August*, 1817, we find columns containing many hundred figures, which run thus: if 1 gives 8, 2 will give 16; 3, 24; 4, 32; 5, 40; and so on, up to the fatiguing length of 300 multiplications of the number 8. Most readers would be satisfied with one century of such inventions.

Whenever the Doctor generalizes on his own bottom, he constantly sinks into downright absurdity. Thus; "But if this explanation be correct, those bodies ought to expand most, whose attraction of cohesion is least*." Now observe, that in the very same section, his expansion tables concur in proving, that all the metals expand more than glass, therefore their "attraction of cohesion is least." Mr. Rennie has shewn, that wrought iron, has a cohesive attraction three times greater than cast iron; therefore by Dr. Thomson's *law*, the latter should expand *most*; whereas it certainly expands *least*. Cast copper and cast iron have the same cohesive force, according to Mr. Rennie's accurate experiments; yet by Dr. Thomson's tables, the former expands more than the latter in the ratio of 19 to 11, instead of being equal, agreeably to his *law*. To the want of the comparing faculty (or *organ of inductiveness*), we must ascribe the perpetual contradictions which we meet with in this system. Thus in transcribing the results of Dulong and Petit on expansion, he says, "Different glass tubes from different manufactories gave precisely the same result†." And at the bottom of the next leaf, he says, "But different kinds of glass differ so much from each other (in expansion), that no general rule can be laid down." His account of the thermometer is contemptible. He gives no directions whatever, by which an accurate thermometer may be made, or by which its indications may be verified; but, after some historical notices, concludes his discussion of this important instrument, with the common

* *System*, 6th Edit. I. 30. † 6th Edit. I. 34.

arithmetical rules for reducing the different European scales to each other. When he copied Biot's plan, in beginning the subject of heat with expansion, because this furnished an instrumental aid of research, why did he not likewise copy, from that philosopher, some instructions for making or verifying a thermometer? "The reader has only to peruse the chemical part of Biot's *Traité de Physique*, to perceive how very superior it is to the parallel discussions in the last two editions of Dr. Thomson's system, published since*." If he has neglected Biot, and Gay-Lussac on thermometers, he has been careful to give sufficient prominence to Sir Charles Blagden's experiments on expansion.

The only alterations of any consequence, which the Doctor has introduced into his first seven sections on heat, are a few extracts from the papers of Dulong and Petit, and of Ure and Southern; the first chiefly on the laws of cooling, and the second, on the elasticity of steam and other vapours. His eighth section treats of the sources of heat, which are referred to six heads; the sun, combustion, percussion, friction, chemical combination, and electricity. He ought to have included combustion under chemical combination, of which it is merely an *accident*; and he should have enrolled *decomposition* among the sources of heat, as we see in the cases of euchlorine, chloride and iodide of azote, and in fulminating silver, gold, and platina. Under the second head, combustion, we had hoped he would have indemnified the public, in the present edition, for the obsolete and useless matter contained in the last. But we have been grievously disappointed. Only nineteen pages are allotted to this most important subject, of which not more than three are new written. These are taken from Sir H. Davy's admirable researches on flame, published some years ago in the *Philosophical Transactions*. But the whole spirit of the original memoirs has been dissipated. What remains is a mere *caput mortuum*, calculated to convey the most inadequate ideas of Sir H. Davy's discoveries. Dr. Thomson's first sixteen pages on this subject are absolute verbiage, to which the reproof of Lord Bacon, is more strictly applicable, than to any modern speculations which we know. "Et *certè* habent id quod puerorum est; ut ad garriendum prompti sint, generare autem non possint: Nam verbosa videtur sapientia eorum, et *operum sterilis*†." The exploded and unprofitable fancies of Stahl, Priestley, Crawford, Kirwan, Lavoisier, Brugnatelli, and Thomson, are given with nauseous prolixity; but the experimental researches of Sir H. Davy, incomparably more beautiful, and full of fruit, are curtailed of their fair proportion, and discreditably travestied.

* Slightly altered from Dr. Thomson's words in his *Annals*.

† *Novum Organum*, Lib. I. Aph. 71.

Dr. Thomson lays down his own scheme of combustion, with abundant technology. "All bodies in nature," says he, "as far as combustion is concerned, may be divided into three classes; namely, supporters, combustibles, and incombustibles. By supporters, I mean substances which are not themselves, strictly speaking, capable of undergoing combustion; but their presence is absolutely necessary in order that this process may take place. Combustibles and incombustibles require no definition. The simple supporters, at present known, are three in number; namely, oxygen, chlorine, iodine. The compounds which these three bodies make with each other, and with azote, are likewise supporters." "During combustion, the supporter (supposing it simple, or if compound, the oxygen, chlorine, or iodine, excluding the base,) always unites with the combustible, and forms with it a new substance, which I shall call a product of combustion. Hence the reason of the change which combustibles undergo by combustion. Now every product is either, 1. an acid; 2. an oxide; 3. a chloride; or, 4. an iodide. As light and heat are always emitted during combustion, but never when a supporter combines with a combustible without combustion, it is natural to suppose, that the supporters contain either the one or the other of these bodies, or both of them. I am disposed to believe that the supporters contain heat, while that body in other cases is wanting, or at least not present in sufficient quantity. My reason for this opinion is, that the heat which is evolved during combustion, is always greatest when the quantity of supporter which combines with the burning body is greatest; but this is by no means the case with regard to light."

We have quoted the passage at some length, because it exhibits the characteristic vice of our author, who is always mistaking scholastic subdivisions for inductive generalization. That man must take a narrow view of chemical phænomena, who ventures to construct the above crude distinctions; for theory it cannot be called. Does he not know that protoxide of chlorine, as it comes in contact with iodine, both in the cold, produces combustion? Here we have his supporters, strictly speaking, of themselves and by themselves, capable of undergoing combustion. Again, potassium burns in sulphuretted hydrogen; but where is his supporter, "whose presence is *absolutely necessary*, in order that this process (combustion) may take place?" Potassium burns likewise in cyanogen, in the absence of all his three supporters. What more powerful instances of combustion can we adduce, than are exhibited in the explosion of the chloride and iodide of azote? Yet none of Dr. Thomson's *combustibles* is present. We deny that every product of combustion is either an acid, an oxide, a chloride, or an iodide. The formation of the sulphurets, even *in vacuo*, is accompanied with vivid

combustion; and the products of the combustion of euchlorine, by a gentle elevation of temperature, and of the chloride and iodide of azote, can be referred to none of the above heads. "Light and heat are always emitted during combustion; but never when a supporter combines with a combustible without combustion." This is a notable discovery. The phænomena of combustion do not appear without combustion! Had Dr. Thomson read Sir H. Davy's papers on flame, he would have seen investigations of the relation between the light and heat, emitted in combustion which would have made him ashamed of the ridiculous fancies, with which the above extract concludes. It has been demonstrated many years ago, that no *peculiar* substance or form of matter is necessary for the effect of combustion; that it is a general result of the actions of any substances possessed of strong chemical relations, or different electrical relations, and that it takes place in all cases in which an intense and violent motion can be conceived to be communicated to the corpuscles of bodies*. It is amusing to find in the Doctor's brief and unsatisfactory abstract of Sir H. Davy's researches, two short sentences, which render all his previous arguments nugatory. "It is only when opaque bodies are evolved during the combustion of gases, that flame appears. And the intensity of the flame depends upon the quantity of such matter evolved. This notion first advanced by Davy, *seems* correct †." Dr. Thomson should reserve his term *notion*, for his own crude conceptions; and *pitch upon* a more becoming appellation for an experimental truth of the first interest and importance. But he cannot even transcribe without vitiating the statements. Flame consists of light and heat. Now it is the intensity of the *light*, and not of the flame in general, which depends on the quantity of solid matter evolved, and then ignited. In fact, this "system" seems as if simultaneously written by two different hands, neither of which knew what the other was about.

"If any confidence can be put in the accuracy of the preceding tables, (Rumford's), it is pretty obvious, that the quantity of heat evolved in combustion, is not proportional to the quantity of oxygen which unites with the burning body‡." As he elsewhere eulogizes Rumford's method of experimenting, we ask what becomes of his opinion so formally announced above, that the "heat which is evolved during combustion is *always* greatest when the quantity of supporter (oxygen, for example,) which combines with the burning body, is greatest." His fancy of *semi-combustion* in the formation of sulphurets and phosphurets, "indicating by the term, that it possesses precisely one half of the characteristic marks of combustion," is too grotesque to

* Sir H. Davy's *Elements*, p. 223. † *Thomson's System*, I. p. 145.
‡ *Ibid.* p. 144.

require refutation, especially as we have formerly paid our respects to it*. If the rapid emission of heat and light, with a change of properties in the bodies concerned, be not combustion, we beg the Doctor to define what combustion is.

In his meagre couple of pages on the heat produced by chemical combination, which is a *verbatim re-print* of the article "Mixture," in the 5th Edition, he travesties M. Gay-Lussac. "From the experiments of Gay-Lussac, it appears still more clearly, that the *evolution* of heat or cold in such cases depends upon the change of the water, from a state of solidity to a state of liquidity, or *vice versâ*. He mixed together a solution of nitrate of ammonia of the specific gravity 1.302, at the temperature of 61°.3 with water, in the proportion of 44.05 of the former, and 33.76 of the latter. The temperature of the mixture sunk 8°.9, yet the density increased; for the mean density would have been 1.151, while the density of the mixture was 1.159. This acute experimenter mentions several similar examples; though in none of them was the absorption of heat so great, as in the instance which I have selected." Now, we ask the Doctor, what have these phænomena to do with the change of water from a state of solidity to a state of liquidity, or *vice versâ*. We have two liquids mixed, which continue liquid, and though the density be *increased*, heat is not *evolved* but *absorbed*, to use our author's phraseology. Compare with this fact his previous explanation. "It is not difficult to see why condensation should occasion the evolution of caloric and rarefaction the contrary. When the particles of a body are forced nearer each other, the repulsive power of the caloric combined with them is increased, and consequently a part of it will be apt to fly off†." But it will be difficult, nay, impossible for the Doctor, on these principles, to see why *rarefaction* should occasion the evolution of caloric, and *condensation* the contrary, as happens in the above indisputable experiments of the French philosopher.

His three pages on the sixth source of heat, electricity, are also a *re-print*, and are remarkable only for the prominence which they give to Berzelius's electric hypothesis of combustion, a disfigured plagiarism in 1813, from Sir H. Davy's Bakerian Lecture of 1806. Instead of quoting a *crambe recocta* of Doctor Thomson, who admits that the compounds of oxygen with chlorine, and phosphorus with sulphur, are "incompatible with Berzelius's doctrine of chemical affinity taken in its broadest extent;" we shall give more satisfaction to our readers by the following extract from the memoir of the learned Swede himself. "The chemical action effected by the discharge of the

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* Vol. IV., p. 306. † *System*, vol. I. p. 150.

pile, consists in the particles in a combination being repolarized. In a combination of particles having the same unipolarity, the pile merely restores by the decomposition, the general polarity, because their specific unipolarity was not changed by their union; but in combinations of opposite unipolarity it likewise restores the specific unipolarity of the elements. May we conclude that, in the first case, the general repolarization takes place in the same manner as the loadstone gives magnetism to a small particle of steel, and that in the second, the pile contributes by its own specific energies to restore the predominating poles *." "We may, therefore, at least with some probability, imagine caloric and the electricities to be matter destitute of gravitation, but possessing affinity to gravitating bodies. When they are not confined by these affinities, they tend to place themselves in equilibrium in the universe; the suns destroy at every moment this equilibrium, and they send the re-united electricities in the form of luminous rays towards the planetary bodies, upon the surface of which the rays being arrested, manifest themselves as caloric; and this last in its turn, during the time required to replace it in equilibrio in the universe, supports the chemical activity of organic and morganic nature. If we can imagine all this to be possible, we possess a notion how the sun can cause a body to emanate from itself without loss of its own volume, and without this emanated body producing on the bodies which arrest it the effects of a gravitating and falling matter †." Such are two entire paragraphs given without garbling, of Berzelius's theory of combustion and chemical affinity, which Dr. Thomson says "has a very plausible appearance, and which has been embraced either entirely or with some modifications, by several of the most eminent chemists of the present day ‡." We should like to know, what eminent chemists of the present day have embraced these shapeless chimeras of Berzelius. Wherever his notions of chemical affinity are intelligible, they are borrowed from the Bakerian Lecture.

Doctor Thomson's ten pages on electricity are also re-printed without alteration, though he surely might have taken the trouble when he plunged so deeply into magnetism with Mr. Barlow, to have said something about the newly-discovered relations of that power and the electric. After giving a dull detail of ordinary electrical phenomena, he enters on galvanism, or electro-chemistry, to which he devotes rather less than five pages. These, indeed, contain so little matter, either interesting or true, that he might have omitted the subject altogether, without loss to his readers. He has repeated the gross blunder,

* *Nicholson's Journal*, March 1813, p. 196. † *Ibid.* p. 165.
‡ *System*, vol. I p. 161.

which, in our former critique we pointed out, relative to the energy of the pile, which he very ignorantly says, "at least, as far as chemical phenomena are concerned, increases in proportion to the size of the pieces."

All the world, at least all who take any interest in science, have heard of the electro-chemical researches of Sir H. Davy, in admiration of which, the National Institute of France decreed him the Napoleon prize during the hottest period of Buonaparte's hostility against England. Yet, to these magnificent researches, from which a brilliant train of marvellous discoveries have sprung, Dr. Thomson allots only *one-third of a page*, in a voluminous collection of 2600 pages, in which he bestows from nine to ten on the fire-phantoms of Stahl and Brugnatelli. It is not merely of omitting the most beautiful investigations of modern times, that his readers have to complain. The little he does say is evidently distorted. "Sir H. Davy," says the Doctor, "took up the subject where Berzelius and Hisinger laid it down; his celebrated dissertation, for which Buonaparte's galvanic prize was awarded to him, contains merely a verification of the law discovered by Berzelius and Hisinger*." The National Institute of France must have been equally stupid, prodigal, and unjust, to bestow, in 1807, on a native of England, against which their despotic master was waging a furious warfare, the Imperial prize for a discovery, which two natives of Sweden, their ally and friend, had previously made. Nor can it be said that the Institute was ignorant of these Swedish researches, for a detail of them is inserted in the *Annales de Chimie* for 1803, a work conducted by some of its leading members†. We do not wish to undervalue these galvanic experiments *(Expériences Galvaniques)* of M. M. Hisinger and Berzelius; Sir H. Davy has, indeed, himself allowed them their full merit, in his first Bakerian Lecture; but the discoveries of the English philosopher are evidently the spontaneous growth of his own mind, and are not, in any respect, traceable to the Swedish chemists, whose memoir, however ingenious, had been so little regarded as not even to be translated, in the space of three years, from the French into the English Journals, though every other galvanic inquiry of the least note had been speedily imported from the continent. The former differ from the latter, just as the systematic investigations of Newton on the laws of gravitation, differ from the prior suggestions of Galileo, Kepler, and Hooke, on the laws of reciprocal attraction; Hisinger and

* *System*, vol. I. p. 171.

† The following are the editors named on the title-page of the above volume: Les Cit. Guyton, Monge, Berthollet, Fourcroy, Adet, Hassenfratz, Seguin, Vauquelin, C. A. Prieur, Chaptal, Parmentier, Dey, Bouillon Lagrange, et Collet Descotils.

Berzelius may be ranked among those scattered stars which preceded the day, but had no influence on the rising of our "philosophic sun," in whose beams they were absorbed.

The curious facts which Hisinger and Berzelius had observed, were actually *laid down* by them, abandoned for many years in an insulated and unproductive state. Sir H. Davy, departing from another point, laid open a similar series of facts, but he traced them out in all their scientific bearings, and to consequences of the most magnificent kind. Galvanism was not a field which the Swedish chemists had explored before the English philosopher chose to enter it; on the contrary, the latter, as Doctor Thomson well knows, had reaped many laurels on that field, long ere the names of the Swedish experimentalists were heard in it. Instead, therefore, of saying, "Sir H. Davy took up the subject where Berzelius and Hisinger laid it down;" Dr. T. would have spoken more truly, had he said, "Berzelius and Hisinger took up the subject where Sir H. Davy laid it down." In *Nicholson's Journal* for September 1800, a very few months after the discovery of Volta's pile was announced in England, there is a communication from Sir H. Davy, in which we find the following sentence: "Reasoning on this *separate* production of oxygen and hydrogen, from *different* quantities of water, and on the experiments of Mr. Henry, jun., on the action of galvanic electricity on different compound bodies, I was led to suppose, that the constituent parts of such bodies (supposing them immediately decomposable by the galvanic influence,) might be separately extricated from the wires, and in consequence obtained distinct from each other." He then proceeds to detail experiments which prove, that solution of caustic ammonia gives out azote and oxygen at the positive pole, and hydrogen at the negative; while sulphuric acid afforded oxygen at the former, and sulphur, with sulphuretted hydrogen, at the latter. Every candid person must recognise here the germ of those ideas, which are so admirably developed in his Bakerian Lecture of 1806, and which yielded so rich a harvest in his subsequent lectures.

In fact, Sir H. Davy entered on his electro-chemical career at the earliest possible period. The first article of *Nicholson's Journal* for November 1800, is an elaborate paper of his "On the causes of the galvanic phenomena, and on certain modes of increasing the powers of the galvanic pile of Volta," not inferior in novelty and interest to any thing published on the subject, except by Volta and himself. In the *Philosophical Transactions* for 1801, appeared his "account of some galvanic combinations, formed by the arrangement of single metallic plates and fluids." About the same time were published in the Journals of the Royal Institution, "Outlines of a view of galvanism, chiefly extracted from a course of lectures on the galvanic phenomena, read at

the theatre of the Royal Institution," by Sir H. Davy. This paper contains an immense number of ingenious views, which he has since expanded and verified. "When our galvanic instruments," says he, "are rendered more perfect and more powerful, we may be readily enabled by means of them, to procure the pure metals." Another curious discovery there announced by him, is "a method of constructing simple and compound galvanic combinations, without the use of *metallic* substances, by means of charcoal and different fluids." This discovery constitutes at the present day, the most singular and mysterious part of Voltaic electricity.

The circumstances which led Sir H. Davy to institute, in 1806, those electro-chemical researches, which were destined to enlarge the boundaries of science, to extend the empire of man over the domain of matter, and to shed new glory over the country of Newton, are sufficiently known to all scientific men, and to no person better than the compiler before us. In the *Philosophical Magazine* for April, 1805, a short letter was inserted, dated Cambridge, and signed W. Peel, intimating, that by the galvanic decomposition of distilled water, he had generated muriate of soda. The *Edinburgh Medical and Surgical Journal* of the following July, contains a letter from Professor Pacchiani, bearing date, Pisa, May 9th, 1805, in which he announces, that he has succeeded by galvanism, in proving muriatic and oxymuriatic acids to be oxides of hydrogen; but that, in the latter, there is *less* oxygen than exists in water; or, as he states it afterwards in a new letter to Fabroni, "In short, all substances proper for decomposing water, as soon as they are traversed by an electrical current strong enough to disengage oxygen, have the property of converting water into oxygenated muriatic acid *. In the *Philosophical Magazine* for July, 1805, we find another letter, signed W. Peel, announcing that, on repeating his experiment on water, he obtained muriate of potash, instead of muriate of soda. In the same number, Dr. Henry inserted a letter, shewing, that muriatic acid was produced by the electrization of distilled water, with platina points, in a glass tube. To this letter the editor subjoins a judicious note, in which he says, "From all that has yet occurred on this subject, a strong presumption is furnished, that we are on the verge of, perhaps, more than one important discovery in chemistry," a prediction soon verified by Sir H. Davy. A third letter, signed W. Peel, is printed in the *Philosophical Magazine* for December, 1805, in which it is asserted, that doubly-distilled snow-water, yielded by galvanism, muriate of soda. About the same time

* *Annales de Chimie*, tom. LVI.; or, *Tilloch's Magazine* for March, 1806.

appeared in the *Annales*, Pacchiani's second letter, confirming his first statement. "This astonishing change," says he, "of water into oxygenated muriatic acid, creates an agreeable surprise in the mind; *felix qui potuit rerum cognoscere causas!*" In the same volume, we find the Galvanic Society of Paris controverting Pacchiani's results; which, on the other hand, were affirmed by Brugnatelli *.

The fermentation which had been thus excited in the chemical world was extreme. It was reserved for Sir H. Davy to allay it, by developing the true causes of all these perplexing anomalies. This he effectually accomplished in his first Bakerian Lecture, "on some chemical effects of electricity," read before the Royal Society in November, 1806, and published in the first part of their *Transactions* for 1807. The experiments detailed there were not made in a corner; they were carried on with the knowledge of some of the first philosophers of England; and their origin, progress, and conclusion, were altogether independent of the results of Berzelius and Hisinger.

Sir H. Davy's lecture is divided into ten sections, of which the first is a brief introduction; the second developes, in a masterly manner, the changes produced by electricity on water. This research obviously furnishes the key to all his subsequent discoveries; for he finds that water electrized in contact with almost any substance, mineral or organic, except gold and platinum, is altered by them; and that acid and alkaline matter is eliminated from substances in which nothing of that kind was suspected to exist. Electricity was thus shewn to be an agent of analysis, incomparably more *delicate* than the nicest chemical tests. In his subsequent sections, he comes to infer, that it may also be rendered an agent of decomposition more *powerful* than any hitherto known, and suggests its application to discover the true constituents of matter, to reach the *ima penetralia naturæ*. "This fact," says he, "may induce us to hope, that the new mode of analysis (the electrical,) may lead us to the discovery of the true elements of bodies, if the materials acted on be employed in a certain state of concentration, and the electricity be sufficiently exalted." How soon, and how amply, he verified this anticipation, his discoveries of potassium, sodium, calcium, barium, boron, and the chloridic theory, will attest to every future age.

Having thus endeavoured to rectify the erroneous impressions, which the perusal of Doctor Thomson's account of electro-chemistry is calculated to make on the unwary reader, we proceed to the second division of the first book of his system, comprehending ponderable bodies, which are handled in a very heavy

* See *Philosophical Magazine* for June, 1806.

style. He distributes them into supporters of combustion, incombustibles, and combustibles. "The term *supporter of combustion*, I apply to those substances, which must be present before combustible bodies will burn." That a chemical author, at the present day, should assume the narrow notions of Becher and Stahl, as the groundwork of his system, is truly wonderful. Dr. Thomson contemplates chemistry solely as a process of combustion, instead of considering combustion as an accident of chemical combination; what is merely an adventitious and occasional accompaniment of its phenomena he mistakes for the soul of the science.

Oxygen, chlorine, iodine, and fluorine, are the subjects he chooses to begin with; and seeing that he furnished no preliminary explanations of the principles of chemical action and research, they are quite sufficient to perplex and confound the student at his outset. In treating of oxygen, he gives a wooden cut of an iron bottle, and two of a pneumatic cistern, which he describes twice over, in two consecutive pages, in nearly the same words; these water-troughs differ only in the second having four clumsy feet, while the first has none. The whole article oxygen, is re-printed *verbatim*. He never mentions chlorate of potash, nor red oxide of mercury, as convenient substances from which it may be obtained pure; though the former is always used with that view for accurate researches, and the latter affords it elegantly to the student, by means of a bent glass tube, sealed at one end. He gives no instructions for ascertaining, whether the gas which comes over from the bleacher's manganese, be pure enough for collecting. "Oxygen gas," says he, "is not sensibly absorbed by water, though jarfuls of it be left in contact with that liquid." Why does he not state, that if "jarfuls of it be left in contact with that liquid" in the pneumatic trough for a few days, it is so altered as to be unfit for experimental purposes? He omits, also, to direct "the tyro just beginning the study," to withdraw the iron bottle from the fire, or to remove its beak from the water, as soon as gas ceases to issue, and before the heat subsides, otherwise the water may be forced into the bottle, so as to cause explosion.

"The weight of an atom of oxygen in the subsequent part of this work will be denoted by, 1st. a volume of oxygen is equivalent to two atoms, provided, *we* suppose, as *I* have done, that water is a compound of one atom of oxygen and one atom of hydrogen." This hypothetical sentence is thrown at once on the student without preface or commentary.

The second section treats of chlorine, and is also a *verbatim re-print*. "As soon as the phial is full, it is to be withdrawn, and its mouth carefully stopped with a glass-stopper, accurately ground so as to fit, and *which must be previously provided*." It

is, undoubtedly, sound advice, to provide a stopper before you try to stop a phial with it; but it is rather unsound to desire the phial to be withdrawn *before* you stop it; it should be stopped *before* it is withdrawn, unless you wish the dense chlorine to fall out. The Doctor's love of detracting from English chemistry is displayed in this article. "The experiments of Scheele and Berthollet were repeated and varied by all the eminent chemists of the time. But the first great addition to the discoveries of these philosophers was made by Gay-Lussac and Thenard, and published by them in 1811, in the second volume of the *Recherches Physico-Chimiques*, p. 94. They shewed, that the opinion that oxymuriatic acid contains no oxygen, might be supported; but, at the same time, assigned their reasons for considering the old opinion as well founded. An abstract of these important experiments had been published, however, in 1809; these experiments *drew the attention* of Sir H. Davy to the subject." This statement is mischievously incorrect. The Bakerian Lecture, read before the Royal Society in December, 1808, and January, 1809, contains a great many ingenious experiments on muriatic and oxymuriatic acid, the constitution of which bodies was naturally one of the first objects to which Sir H. Davy directed his attention, after the discovery of potassium and boron. At the end of the *supplement* to that lecture, he says, "The process in which this decomposition (of muriatic acid) may most reasonably be conceived to take place, is in the combustion of potassium in the phosphuretted muriatic acid, deprived by simple distillation with potassium of as much phosphorus as possible. I am preparing an apparatus for performing this experiment, in a manner which, I hope, will lead to distinct conclusions." His subsequent paper, read in 1810 and 1811, before the Royal Society, finally decided the difficult point of chemical doctrine concerning oxymuriatic acid. The paper in the second volume of the *Mémoires D'Arcueil*, to which Doctor Thomson refers his readers for authority against Sir H. Davy's claims, concludes thus, "Le gas muriatique oxigéné n'est pas, en effet, decomposé par le charbon, et on pourroit d'après ce fait, et ceux qui sont rapportés dans ce mémoire, supposer que ce gas est un corps simple. Les phénomènes qu'il présente s'expliquent assez bien dans cette hypothèse; nous ne chercherons point cependant à la défendre, parce qu'il nous semble, qu'ils s'expliquent *encore mieux*, en regardant l'acide muriatique oxigéné, comme un corps composé." But Sir H. Davy chose to defend that opinion, and succeeded in convincing the world that it was the just one; and that the hypothesis which the French chemists regarded as *still better*, was destitute of proof, and untenable.

The experiments by which he effected this great revolution

of sentiment, are among the finest ever made; and it is discreditable to Dr. Thomson, and detrimental to his readers, that he has entirely suppressed them. In his meagre description of chlorine, his usual inaccuracy accompanies him: "whenever any vegetable blue colour is exposed to the action of chlorine, it is immediately destroyed, and cannot afterwards be restored by any method whatever." The gas itself cannot effect this change on *dry* colours; the agency of moisture is required.

"The deutoxide of chlorine was discovered about the same time by Sir H. Davy and Count Von Stadion, of Vienna; but Davy's account of it was published sooner than that of Count Von Stadion." The account of the former was published in *Thomson's Annals*, eight months before that of the latter appeared. Surely, some qualm of conscience must have smitten our compiler, in writing his next page. "But the properties of the substance described by the Count, differ so much from those of the gas examined by Davy, that it is probable they are distinct substances."

"A volume of chlorine may be considered as equivalent to an atom, while a volume of oxygen gas is equivalent to two atoms. Hence, if a body be a compound of two volumes chlorine and one volume oxygen, we know that it consists of one atom chlorine and one atom oxygen*." Into such atomical inconsistencies does he fall, in following Mr. Dalton's hypothesis, instead of the experimental system of Sir H. Davy.

The third section discusses iodine. It is word for word the same as in the 5th edition, and it is badly extracted from the memoirs of Sir H. Davy and M. Gay-Lussac. He introduces here, among simple substances, his whole account of iodic and chloriodic acids, in violation of his own systematic arrangement; and perverts Sir H. Davy's results on chloriodic acid, to suit his own atomic notions.

His fourth section on fluorine is a misnomer. It is occupied almost wholly with fluoric acid, which being, according to him, a compound, has no business among simple substances. His account of it is remarkable, as usual, for distorting Sir H. Davy's results. "From an experiment of Sir H. Davy," says he, "it would appear, that the number which represents an atom of this acid is 1.0095; supposing an atom of oxygen to be 1†." It is really too much for the Doctor to commit such errors, and then transfer them to that accurate philosopher. To make the blunder more comical, he quotes the experiment in a note to the same page, from which it appears, that the atom of fluoric acid, on the Doctor's own

* *System*, I. p. 189. † Ibid. I. p. 198.

principles, computed by the plainest arithmetic is 1.3015. "Hence," says he, "fluate of lime is composed of

Fluoric acid,	26.418	1.0095.
Lime,	73.582	3.625."

This being a simple question in the rule of three, he might surely have contrived to solve it, in the course of two consecutive editions of his complete system. For, 73.582 : 3.625 : : 26.418 : 1.3015, and not 1.0095, as he has it. But he is not content with forging this infinitesimal atom; he must turn it to good account. The following is a rather favourable specimen of Doctor Thomson's style of philosophizing. "If we suppose fluate of lime to be a compound of fluoric acid and lime, its composition will be, fluoric acid, 1.0095,

lime, . 3.625."

"From this we see that the weight of an integrant particle of fluoric acid must be 1.0095. If it be supposed to be a compound of one atom of oxygen, and one atom of an unknown inflammable basis, then, as the weight of an atom of oxygen is one, the weight of an atom of the inflammable base can be only 0.0095, which is only the thirteenth part of the weight of an atom of hydrogen. On that supposition, fluoric acid would be composed of inflammable basis, 1.00,

oxygen, . 105.67.

"So very light a body, being contrary to all analogy, cannot be admitted to exist without stronger proofs than have hitherto been adduced. On the other hand, if fluor spar be in reality a *fluoride of calcium*, then its composition will be,

Fluorine 2.0095
Calcium 2.625

So that the weight of an atom of fluorine would be 2.0095, or almost exactly t vice the weight of an atom of oxygen. This is surely a much more probable supposition than the former. But the question cannot yet be considered as fully decided." What a heap of tautology, piled upon his own arithmetical blunder! He gives no intelligible account of Sir H. Davy's last elaborate train of researches on fluoric acid.

His second chapter is entitled, "Of simple incombustibles." "By incombustible," says he, "I mean a body, neither capable of *undergoing* combustion, nor of *supporting* combustion." It unites to all the supporters; but the union is never attended with the evolution of heat and light." Now we apprehend, that if the mutual action of two bodies, in any circumstances, be attended with the evolution of heat and light, one of them, at least, must be a combustible. But azote forms with chlorine and iodine, compounds capable of exploding with heat and light. A considerable portion of the article azote is taken up with its acids, in order, as it were, to enlarge the book,

without giving the author the trouble of new composition. The same matter is reprinted in the second volume.

The third chapter treats of simple combustibles. Of these, his first genus contains "bodies forming *acids*, by uniting with the supporters of combustion, or with hydrogen." We meet here with nine substances, among which are silicon, arsenic, and tellurium; but he excludes chromium and tungsten, though one might imagine they had rather a better claim than silicon, to be regarded as *acidifiable combustibles*. "The present method," says he, "of confounding every thing under the name of metal, has introduced much confusion into the science." We know of no chemical writer, who has introduced such confusion as Dr. Thomson, who has clashed together, silicon, sulphur, arsenic, tellurium, and osmium, in one group.

Under carbon, the only novelty is *hydrocarbonic oxide*, which Dr. Thomson has ventured to embody in a system of chemistry, as, we fear, upon insufficient grounds; for more than one-third of the weight of ferro-chyazic acid is, by his own account, azote; yet when he decomposes that acid in the ferro-chyazate of potash, by sulphuric acid and heat, he gets a gas absolutely destitute of azote.

In treating of chloric ether, he says, "I examined this compound in 1810, and *ascertained* that it is a compound of olefiant gas and chlorine." He refers to the first volume of the *Wernerian Memoirs*, p. 516. On looking over this paper on olefiant gas, we find the only thing said about chloric ether, to be, "It is a substance, of a nature quite peculiar, and *seems* to consist of the two gases simply combined together." This is, obviously, a mere guess, for he offers no analysis of it, yet assumes the credit of determining its nature, after the researches of M. M. Colin and Robiquet had developed it. In that paper, we find him using, for another analysis, an olefiant gas, which, by his own account, "contained 16 *per cent.* of common air, and the oxygen gas was mixed with 11 *per cent.* of common air." We should like to know how he ascertained so precisely, the proportion of common air, when he was in the habit of operating with such impure materials.

In a note at the end of carbon, the Doctor says, "Mr. Brande, in a paper just read to the Royal Society, has advanced the opinion, that this gas (carburetted hydrogen) is only a mixture of olefiant gas and hydrogen gas. But the specific gravity of the gas is quite inconsistent with such an opinion. A mixture of one volume olefiant gas and two volumes hydrogen, would possess the chemical properties of carburetted hydrogen. But the specific gravity of such a mixture, instead of 0.555, would be only 0.36987." On the preceding page, we find the properties of carburetted hydrogen to be such, that "for complete combus-

tion, it requires twice its volume of oxygen gas, and produces exactly its own volume of carbonic acid*." But one volume of olefiant gas requires three volumes of oxygen, and two volumes of hydrogen require one of oxygen, constituting four volumes of oxygen to three volumes of the gases, mixed in his proportion; instead of four to two, as his own statement requires. So that "a mixture of one volume olefiant gas and two volumes hydrogen would *not* possess the chemical properties of carburetted hydrogen." Again, when we turn to the *Annals of Philosophy*, for November, 1820, where this criticism lies stretched at full length, we find him floundering in a quagmire of figures, and employing, as usual, algebraic symbols, to perplex one of the plainest cases of arithmetic. "Suppose now," says he, "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." Here we have two-thirds of a volume of hydrogen, which, like the rogues in buckram, suddenly become three times that proportion in his system, or two whole volumes. But, waiving this contradiction, let us examine his elaborate criticism in the Annals. The specific gravity of a gaseous mixture is found at once, by dividing the sum of the weights by the sum of the volumes. Therefore, when one volume of olefiant gas, and two-thirds of a volume of hydrogen are mixed, their joint specific gravity will become,

$$\frac{0.9722 + (0.0694 \times \frac{2}{3})}{1\frac{2}{3}} = 0.611$$

Let us now see, how the Doctor goes to work, to solve the same problem, and what result he obtains. "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†." Inimitable

* Vol. I. p. 30.
† *Annals of Philosophy*, November 1820, p. 381, 382.

algebraist; excellent computer! we are apt to think, that,

$$x = \frac{(0.66 \times 0.0694) + 0.972\dot{2}}{1.66} = 0.611.$$

precisely as we gave above, for both rules are identical. The Doctor, we are told, has hired an ingenious mechanic, for a short time, to make *experiments* for him; let him next hire an arithmetician to compute their results, and a person, versant in English composition, to clothe them in words; and he may then, probably, publish a better system of chemistry. But the most amusing detection remains. On turning our eyes to the Doctor's paper, so often appealed to by himself, in the *Wernerian Memoirs*, we find, page 507, the following statement about his true carburetted hydrogen: "after depriving it of its carbonic acid, I found its specific gravity 0.611, that of air being 1 000." What a marvellous coincidence! absolutely the same specific gravity by experiment, as the above theoretic mixture of olefiant gas and hydrogen, which possesses the chemical properties of his carburetted hydrogen! Doctor Thomson is here caught in his own trap. To be sure he will try to get out of it by saying, as he does in the *Wernerian Memoirs*, that the ditch gas contained 12 5 *per cent.* of air; but as he gives us no experimental evidence for that assumption, we beg leave to withhold our assent. This 12.5 *per cent.* is necessary for his atomic hypothesis of the density of the gas, but we find no proof or probability of its presence. When we figure to ourselves, the Doctor poking the black slime at "Restalrig" for fetid gas, and wishing us to believe, that he caught in his funnel of "greasy paper," 12½ *per cent.* of pure atmospheric air, from putrid mud, we must say, "*Credat Judæus Apella*," *haud nos*. We see that the experimental density corresponds minutely with that deduced from calculation. The reason that the specific gravity of this gaseous mixture, was so accurately obtained by him, is its having the same density nearly as aqueous vapour. Hence moisture introduced no error into the result, as it has done in his specific gravities of the denser gases, of which there is so pompous an account in his September and October *annals*. The atomic theory shews us, that if two atoms of carbon act chemically on two atoms of water, there ought to result, one atom of carbonic acid (= 1 carbon + 2 oxygen) + 1 atom of olefiant gas (= 1 carbon + 1 hydrogen) + 1 atom of hydrogen. Of the latter gas, which is so very light, a small proportion escapes, leaving the mixture of 1 volume of olefiant + ⅔ of a volume of hydrogen. We lay little stress on this atomic representation of the process, but merely throw it out to accommodate Dr. Thomson, in return for the numerous *ignes fatui*, of a similar kind, that he has conjured up before us

Boron and silicon occupy the third and fourth sections. They

are reprinted without alteration, and contain nothing remarkable any way. To phosphorus in the fifth section he devotes nearly twenty-four pages, of which about five are atomic alterations. The quantity of sulphuric acid, 83½, which he prescribes for decomposing 100 parts of calcined bones, is much too great, and will spoil the subsequent process for procuring phosphorus. And with regard to forming, as he directs, a phosphate of lead, with nitrate of lead, as a step in the operation, no practical chemist could afford to practise it. In his view of the acids of phosphorus, which is reprinted in the second volume, *secundum artem*, he has contrived to make himself appear the hero of the scene, though it is one in which he has played a very subaltern part. He has suppressed entirely the long succession of blunders into which he plunged, and from which he was extricated by Sir H. Davy's researches published in the *Philosophical Transactions* for 1818. Well may the Doctor say, "The weight of an atom of phosphoric acid, has cost me first and last, a good deal of trouble*."

In 1816, he presented a paper on the subject to the Royal Society, which after being read, was withdrawn. Full of its importance, however, at the time, he hastened to publish in his *Annals* for April, a detail of its contents, among which we find the atom of phosphoric acid, declared to be 3.634. Meanwhile he sets to work on phosphuretted hydrogen, and deduces from it, the atomic weight of the acid to be 3.5, which new decision, he publishes in his *Annals* for August 1816. But lo! in January following, we find him affirming that 4.5 is the true atomic weight of phosphoric acid, and that 3.5 is undoubtedly wrong, and must be abandoned. In the subsequent October, the 5th edition of his System comes forth, with a long array of proofs, shewing that 4.5 is the true atom of phosphoric acid. Sir H. Davy's decisive experiments, on the subject, being read, however, before the Royal Society in 1818, made him reflect a little on his contradictions; subsequent to which time, he seizes on the number 3.5 fixed by Sir H. Davy, and finding it, among his former guesses, claims it as his own prior discovery.

The following sentence shews the Doctor's incompetency as an experimentalist. "Unless the flask," says he, "be very completely exhausted, indeed, of common air, combustion takes place when the phosphuretted hydrogen gas is let into it." This confession betrays poverty of invention, and ignorance of the methods previously practised in such cases. If the Doctor will consult Sir H. Davy's *Bakerian Lecture on the Alkaline Metals*, he will find this philosopher filling his flask with hydrogen, and then exhausting that gas, in order to get entirely rid of the atmospherical oxygen.

* *Annals of Philosophy*, for January 1821, p. 9.

In the edition of the System published in the autumn of 1817, Dr. Thomson says, "It will appear, in a subsequent part of this section, that phosphorous acid is composed of 1.5 phosphorus, and 2 oxygen. Hence its constituents are,

Phosphorus 100
Oxygen 133⅓"

The subsequent part of the section to which he alludes is the following; "So that the phosphorus in phosphuretted hydrogen, combines with either ½ volume, or with 1 volume of oxygen gas. In the first place, I suppose that the hypophosphorous acid is formed. In the second case, phosphorous acid*." Now as he makes the volume of phosphorous vapour 0.8328, we have the proportion of 0.8328 to 1.1111, or 100 to 133.4 as above. Observe, this is the Doctor's last decision, prior to Sir H. Davy's paper in the *Transactions* for 1818. Here, however, we find the following statement: "If it be supposed a simple compound of oxygen and phosphorus, the series of proportions in the acids of phosphorus will be

	Phosp.	Oxyg.	Phosp.	Oxyg.
Hypophosphorous acid,	45	15	3	: 1
Phosphorous acid,	45	30	1.5	: 1
Phosphoric acid,	45	60	1.5	: 2"

Compare with the above, the following table in Dr. Thomson's 5th edition: "Thus the three acids of phosphorus are composed as follows:

	Phosp	Oxyg.
Hypophosphorous acid,	1.5	1
Phosphorous acid,	1.5	2
Phosphoric acid,	1.5	3† "

In his 6th edition, we find the following bold statement: "The analysis of phosphorous acid given by Davy, in his paper published in the *Philosophical Transactions* for 1818, exactly agrees with mine, and serves, of course, to confirm it‡." "And the weight of an atom of phosphorous acid is 2.5." We suppose there must be an error of the press, and for "*confirm*," we should read "*confute*."

One would imagine the atom of phosphoric acid fixed in the Doctor's mind; yet he devotes a whole page of his present edition to shew, that the analyses by Berzelius of the phosphates of lead, barytes, soda, and lime, make the atomic weight of the acid 4.5! And from the same authority, he deduces the atomic weight of phosphorous acid to be 3.5. In such contradiction with itself, is his article on phosphorus. "It must be allowed," says he, "that the evidence advanced by Berzelius has a very imposing aspect. But I think MY mode of determining the composition of phosphorous acid is so much more

* *System*, 5th edition, I, 73; † Ibid, p. 266.
‡ Ibid., 6th edition, I, p. 263.

simple than his, that the chance of accuracy is much increased. Yet as Dulong's analysis of phosphorous acid, coincides almost with that of Berzelius, the subject must not be considered as finally settled*." Dr. Thomson's mode has more pliancy, than precision, when it allows him to make the atom of the same body, alternately 2.5 and 3.5, at pleasure. "Therefore," adds he, "according to this view, hypophosphorous acid is a compound of one atom of phosphorus, and $\frac{1}{2}$ of an atom of oxygen. Now this splitting of an atom of oxygen, not merely into halves, but into quarters, which the hypothesis of Berzelius and Dulong render (renders) necessary, is, to say the least of it, very unsatisfactory†," *pauvre atome, corps qu'on vainement regarde comme indivisible!* Upon the whole, the section on phosphorus is the most confused piece of writing that we ever saw.

Under sulphur in his sixth section, we again find Dr. Thomson setting up claims to discoveries, and referring to original memoirs for proofs. Thus, for the composition of sulphurous acid, he refers to his paper in the 6th volume of *Nicholson's 8vo. Journal*; and, on turning to it, we observe the following determination, "Hence sulphurous acid is composed of

Sulphur.	68
Oxygen,	32
	100.

Page 97.

"The phenomena which attend the acidification of sulphur, and the decomposition of sulphurous acid, render it probable that sulphurous acid is rather a compound of sulphuric acid and sulphur, than of sulphur and oxygen." Such are his chemical proportions and chemical philosophy in the paper he refers to. "*Hence* it follows," he *now* says, "that sulphurous acid is composed of 100 sulphur + 100 oxygen." And he actually places to his own credit, and from the above paper, this late determination, that sulphurous acid consists of *equal* weights of sulphur and oxygen. The first person who demonstrated this truth, was Sir H. Davy. "If the specific gravities of sulphurous acid gas and oxygen be compared, and the last subtracted from the first, it will appear that sulphurous acid consists nearly of equal parts of oxygen and sulphur by weight. In several experiments in which I burned sulphur, procured from iron pyrites out of the contact of air or moisture, in dry oxygen gas over mercury, I found that the volume of the oxygen was very little altered‡." These are experiments in which the world may confide. Dr. Thomson sinks them entirely, and refers merely to his own, on which nobody can rely.

The eighth section contains a transcript of Berzelius's re-

* *System*, 6th edition, 1, p. 261. † Ibid., 1, 266.
‡ *Elements of Chemical Philosophy*, p. 273, 274.

searches on selenium. In the ninth section, on arsenic, he deduces the atomic weight of arsenic acid, from the experiments of Berzelius and Laugier, to be 7.25; and, in the next page, converts that number into 14.5, without trying to reconcile the two statements. The atom of the metal itself, is, in the one page, 4.75, and in the other, 9. In the first, it is said to take $1\frac{1}{2}$ and $2\frac{1}{2}$ atoms of oxygen, and in the second 3 and 5 to produce its two acids. Tellurium and osmium, which are classed with sulphur and arsenic, are re-printed with the addition of only one sentence to the first, on its seleniuret.

This second *genus* of simple combustibles, the *alkalifiable*, contains thirty-two bodies, which he subdivides into five families. His long articles potassium and sodium are re-printed *verbatim*, with the addition to the first, of a few lines from Berzelius on the seleniuret. Lithia, is necessarily new; but the Doctor, as usual, accommodates its atomic weight to a multiple of hydrogen. M. Vauquelin's estimate of its composition as a metallic oxide; M. Arvedson's analysis of its chloride, and M. Gmelin's analysis of its carbonate, all concur in shewing its atom to be 2.3, and not 2.25, as Doctor T. decides. In the fourth section, which treats of calcium, he makes the atomic weight of lime 3.625, by suppressing the most accurate analyses of the carbonate by different chemists, and trusting to his own, which was certainly incorrect, as the sequel of his system shews. The sections on calcium, barium, strontium, and magnesium, are literally re printed. His prescription for preparing chloride of barium (muriate of barytes), is marked with his usual awkwardness. By pouring muriatic acid on the sulphuret obtained from the decomposition of the sulphate with charcoal, he takes up all the iron, and contaminates the product unnecessarily; whereas, by adding the acid to the filtered solution of the sulphuret in hot water, this evil is avoided.

His second family, composed of yttrium, glucinum, aluminum, zirconium, and thorinum, are merely re-printed, and deserve no particular notice. The long article, iron, occupying twenty-two pages, is re-printed with all its errors, on the formation of steel, &c., which have long been the ridicule of practical men. The only thing new, is a short paragraph on the seleniuret. Nickel and cobalt are also served up, as before. The absurdities exposed in our former critique *, on his article manganese, remain unaltered in the present edition. He has a new page on the hydrate of Arvedson, and the manganesic acid of Chevillot and Edouards. Cerium, uranium, and zinc, are re-printed *verbatim*. Cadmium is a transcript from Gilbert's annals. His sections on tin, lead, bismuth, copper,

* *Journal of Science*, VIII. 319.

and mercury, are all mere affairs of the printer, except short notices about their seleniurets. We were not a little surprised to find, that he took no notice of Mr. Donovan's new determination of the proportion of oxygen in the mercurial oxides, after the numerous panegyrics which he has pronounced on his paper. "He finds," says the Doctor, "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 s veral 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

	Merc.	Oxyg.
Protoxide	100	+ 3.98
Peroxide	100	+ 7.96

numbers which differ very little from those determined by former experiments*."

Here are committed two distinct and independent errors, in two different compounds, one in excess, the other in defect, and by taking the mean, truth is to be obtained! Has the Doctor any notion of logic?

The articles gold, platinum, palladium, rhodium, iridium, antimony, chromium, molybdenum, tungsten, columbium, and titanium (with which the first volume concludes) are copied *verbatim* from the old edition. We perceive merely a new notice of the ignition produced by exposing platinum, in contact with tin or antimony, to a moderate heat.

Having devoted so much attention to his first volume, which in fact contains the substance of his system, we shall take but a cursory view of the remainder. The Doctor commences his second volume with the following paragraph: "In the present state of the science of Chemistry, I have thought it better to describe several of the compound substances, while treating in the last book, of the simple bodies, by the union of which they are constituted, than to place all the compounds under distinct heads. A contrary plan has been followed by some modern writers, but I think the result has been such, as ought to deter others from imitating their example. The unity of the subject has been destroyed, and the facts have been exhibited in so unconnected a manner, as must considerably retard the progress of the student, while it fatigues and disgusts those who are already acquainted, with the subject." Is Dr. Thomson aware what a graphical picture he has been drawing of his *own* work?

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* *Annals of Philosophy*, p. 17, 18, 1820.

Nobody ever "destroyed the unity of Chemistry to such a degree, as we see he has done, in his first volume; nobody has "exhibited the science in so unconnected a manner;" and nobody has done so much to "retard the progress of the student," and to "fatigue and disgust those, who are already acquainted with the subject." His tautologies are endless, and his self-contradictions intolerable.

Azote, which had figured by itself as an *incombustible*, in the first volume, becomes a *combustible* in the second. Book II. is entitled, "of compound bodies." Its first division contains "*Primary Compounds.*" Of these, the first subdivision is called "compounds of oxygen with simple combustibles." It has three chapters; the first treats of unsalifiable oxides; the second of salifiable bases; the third, of acids. Of the last, he has two genera; acids with a simple basis, and combustible acids. Before he reaches the combustible acids, *one hundred and thirty-three pages* are occupied with details, which, to a very great extent had been already given in the first volume. His unsalifiable oxides, are the two oxides of azote, the two of hydrogen, and carbonic oxide, which occupy five sections. These are, for the most part, mere repetitions of what he had given us before. The salifiable bases are described in his second chapter, which is divided into two sections, the combustible bases, and the metallic oxides; beginning, as usual, with the most intricate subjects, ammonia, morphia, *&c.*

The quantity of quicklime, which he prescribes for the decomposition of sal-ammoniac, is extravagantly great; three of the former to one of the latter; whereas equal weights are quite sufficient in practice. The equivalent proportions are nearly 1 of lime, to 2 of the salt. His account of water of ammonia is a mass of confusion, joined to a suppression, or perversion of known facts. "Water, by my trials," says he, "is capable of absorbing 780 times its bulk of this gas; while in the mean time, the bulk of the liquid increases from 6 to 10. The specific gravity of this solution, is 0.900, which just accords with the increase of bulk."

Now, as 780 cubic inches of this gas weigh $7.80 \times 18 = 140.4$ grains, which combine with one cubic inch of water weighing 252.5 grains, we have their sum = 392.9 grains. Now $392.9 : 140.4 :: 100 : 35.74$ = ammonia in 100. And $252.5 : 392.9 :: 6 : 9.33$ = increase of weight on six parts. But the weight, divided by the specific gravity, will give the volume. Hence

$$\frac{9.33}{0.90} = 10.366$$

instead of his 10; so that his arithmetic is as erroneous as we shall shew his experiments to be.

From the first proportion above, we see that such water as "he made in his trials," contained 35.74 *per cent.* of ammonia.

But in the same page he says, "it follows from the experiments of Davy, that a *saturated* solution of ammonia is composed of 74.63 water + 25.37 ammonia*." But the Doctor's water of 0.900, which he presently shews was far *under* saturation, must have contained, by his own statement, 35.74 *per cent.* of ammonia. Not content with this absurdity, he plunges into another. "The following table, for which we are indebted to Mr. Dalton, exhibits the quantity of ammonia contained in ammoniacal solutions of different specific gravities. We there observe the following numbers:

Specific gravity of liquid.	Ammonia per cent. by weight.
0.85	35.3
0.87	29.9
0.90	22.2"

Now we ask Dr. Thomson, how does 22.2 tally with 35.74, the quantity deduced from his "trials," at the very same specific gravity, 0.90? But what right had he to take up a very old estimate of Sir H. Davy's, and neglect the table given by this philosopher, in his Elements published in 1812? Was it that he might shew by the comparison of Mr. Dalton's table, that Sir H. Davy was egregiously wrong, in speaking of a *saturated* solution, with 25.37 ammonia, when Mr. Dalton's table exhibited solutions containing 35.3 *per cent.*? Why does he not hint at the existence of Sir H. Davy's table, of which he was unquestionably not ignorant? No confidence whatever can be placed in the Doctor's favourite table of the water of ammonia, one of the most important re-agents of the chemist, and one of the most useful preparations of the apothecary. The *per centage* of ammonia in the water of specific gravity 0.90, is, by Sir H. Davy's table, 26; a quantity very nearly, if not absolutely, exact.

Potash, soda, and the earths, are merely reprinted from his former edition.

In his introductory remarks on acids, we meet with an assertion, in the last of the following sentences, which would do discredit to the incipient tyro. "All the acids, at present known, with the exception of three, namely, sulphuretted hydrogen, telluretted hydrogen, and seleniuretted hydrogen, contain a supporter of combustion. By far the greater number of known acids contain oxygen. *All acids, therefore, (with the exception of those above named) are combinations of supporters and combustibles.*" Pray, Dr. Thomson, what is iodic acid; what is chloriodic acid; what is chloric acid; what is *Count Stadion's* perchloric acid? Where is their combustible element? We do not find it among any, or all, of *your* combustibles. These acids result from the union of your mere supporters; and in their formation combustion takes place, without one of your combustibles. The Doctor's remarks on acidity, indeed, like

* *System*, II., p. 28.

all his specimens of generalization, are feeble, and at least ten years behind. As samples of composition, they are intolerably heavy.

Under sulphuric acid, we have first of all a most lame account of its manufacture, and secondly, a very defective and incorrect table from Mr. Dalton, of its specific gravities at successive stages of dilution; though he knows, that a complete and accurate table of this kind is indispensable to the practical chemist. His inconsistencies are here particularly glaring. "Various statements," says he, "are to be met with in books, of the specific gravity of the sulphuric acid of this country, which is a compound of 1 atom acid + 1 atom water," (81.6 + 18.4)." From my own experiments, I conclude that when quite pure, its specific gravity is 1.8447*." Yet in the table, immediately preceding these remarks, opposite to 1.845, we find 77 *per cent.* by weight of real acid, instead of 81.6, an error of no less than 4.6 on the hundred of the liquid acid. The whole table participates in this inaccuracy, and never, by any accident, approaches within two *per cent.* of the truth, so that it is worse than useless to the chemist.

Among his combustible acids, we miss three old acquaintances, which he formerly introduced to our notice, the rheumic, sorbic, and zumic, the latter of which was christened by himself. But he has replaced them by three others of apparently equal identity and importance; the isaguric, krameric, and ellagic, which might as well have been left in their native nests, till they were a little fuller formed. We are well pleased, however, to see the lampic, meconic, and purpuric, though we presume that he is wrong in his guesses respecting the first of these.

We are glad to find that oxalic acid is finally freed of an awkward twelfth part of an atom of oxygen, which vexed him in a former edition. He makes it now a compound, merely of one atom of carbonic acid and one atom of carbonic oxide, whose joint atomic weight is 4.5 = 2.75 + 1.75.

Upon this subject we were surprised that Dr. Thomson does not refer to his own elaborate paper on the subject, in the *Philosophical Transactions* for 1808; particularly as this is his only memoir received into that distinguished collection. But, on looking into the paper itself our surprise took another direction. "The committee of the Royal Society *ought* to be impartial," says the Doctor, in his late diatribe against them: in our humble opinion, they have here shewn that they *are* impartial. It little becomes Dr. Thomson, therefore, to accuse the council of the Royal Society of partiality and oppression against rising genius, "while all the papers written by another favoured individual, however numerous, however expensive, however trifling,

* *System*, II. 115.

or however absurd, are sure to find a place in the Transactions of that learned body*." This assertion forces upon us the trouble of shewing that the two latter epithets are not altogether inapplicable to his own paper on oxalic acid.

Vauquelin mentioned in his dissertation on cinchona, that the crystallized oxalic acid contains about half its weight of water. "But this ingenious chemist does not seem to have been aware of the real composition of oxalate of lime†." The Doctor, from a vast heap of experiments, made out this to be, 62.5 acid, and 37.5 base; while the crystals of acid contained, according to him, 77 real acid, and 23 water. Let us see what he declares in his system, to be the truth. "Berzelius has shewn," says he, "that they (the crystals of oxalic acid) are composed of

Real acid 52
Water 48

Hence, they seem to be a compound of 1 atom acid + 4 atoms water‡." And oxalate of lime is now stated by him to consist of

Acid 55.44
Base 44.56

a very different proportion, truly, from that which he has given above, "of which," he justly observes, "M. Vauquelin was not aware." Bergman's old proportion of 48 acid to 46 lime; or in 100 parts of 51 and a fraction, to 48 and a fraction, is prodigiously more correct. Yet, of this excellent chemist, Dr. Thomson says, "there must have been some mistake in his experiment." His mistake was precision itself, compared to the Doctor's error. As all his subsequent statements, in this paper on oxalic acid, are founded upon this erroneous analysis of oxalate of lime, it is needless to examine the superstructure. Oxalate of barytes is now made, in his system, to consist of

Oxalic acid	31.62	Barytes	68.38
Or,	100.00		216.00
In his memoir, it is	100.00		142.86

being of base a proportion too little by *more than one-half* of the whole quantity.

Though he does not refer us to the paper itself, yet he says, "I found the composition of oxalic acid may be stated as follows:

Oxygen 64
Carbon 32
Hydrogen 4
100§"

But that acid of his consisted of 32.5 water + 67.5 real acid in

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* *Annals of Philosophy*, October, 1820, p. 296.
† *Philosophical Transactions* for 1808.
‡ *System* II. 169. § Ibid., II. 169.

100. Hence, if we deduct 28.9 oxygen + 3.6 hydrogen (= 32.5, water), from the above proportions, we have

35.1	oxygen, or	52.00	
32.0	carbon, ..	47.4	
0.4	hydrogen	0.6	
67.5		100.0	

which is the composition of the real acid by his experiments. Berzelius's analysis, which he now considers as nearly exact, gives us,

Oxygen	66.534
Carbon	33.222
Hydrogen	0.244
	100.000*

The above statement exposes, as we think, an attempt to substitute an absurd analysis for one nearly true; for our author compares the delusive constitution of an acid, containing, confessedly, 32.5 *per cent.* of water, with Berzelius's real analysis of the real acid, and from gross errors in his former estimate, which cause an accidental and spurious resemblance, claims anticipation of the truth.

The second subdivision of "primary compounds, formed by the combination of two or more simple substances with each other," comprehends "compounds of chlorine with supporters and combustibles." "Secondary compounds," says Dr. Thomson, "are formed by the combination of two or more compound bodies with each other. Thus phosphate of ammonia is composed of phosphoric acid and ammonia†." In downright violation of his own arrangement, he introduces chlorate of ammonia, and all the chlorates, under primary compounds, such as acids and oxides; and the reader seeks them in vain, among the salts, his *secondary* compounds, to which they clearly belong. Under the chapter "Chlorides" we have only chloride of lime; and here we were surprised to find a commendatory repetition of his former plan of analysis, after the confutation it received in a late volume of the *Annales de Chimie et de Physique*. "The oxygen gas," says the Doctor, "given out, (by heat,) enables us to determine very exactly, the quantity of chlorine contained in the powder‡." This method is worse than nugatory to the bleacher; it is perfectly deceptive. For a mixture of chlorate of lime and muriate of lime, will give exactly the same proportion of oxygen by heat, as a genuine chloride of lime, but the former mixture has no bleaching power. Moreover, as the chlorine in the bleaching powder is but loosely united to the lime, a portion of it comes off by

* *System*, II. 169 † Ibid. II. 1 ‡ Ibid., I. 241

the heat, either alone, or in the state of an oxide, soluble in water. His preceding scheme* of analyzing this powder by nitrate of silver, was still worse: it was a method which would lead the bleacher to believe, that his powder was strongest, when in reality it was weakest, or altogether inert. For when the chloride of lime passes into the common muriate, as by keeping, &c., the precipitate by nitrate of silver attains a *maximum*.

When we read his next article, on muriatic acid, we are tempted to exclaim with the old statesman, Nescis, mi fili, quam parvâ scientiâ, libri quidam conficiuntur. "A cubic inch of water," says he, "at the temperature of 60°, barometer 29.4, absorbs 515 cubic inches of muriatic acid gas, which is equivalent to 308 grains nearly. Hence water, thus impregnated, contains 0.548, or more than one half its weight of muriatic acid, in the same state of purity as when gaseous. I caused a current of gas to pass through water, till it refused to absorb any more. The specific gravity of the acid thus obtained was, 1.203. If we suppose that the water, in this experiment, absorbed as much gas as in the last, it will follow from it, that 6 parts of water, by being saturated with this gas, expanded so as to occupy very nearly the bulk of 11 parts, but in all my trials, the expansion was only to 9 parts. This would indicate a specific gravity of 1.477; yet upon actually trying water thus saturated, its specific gravity was only 1.203. Is this difference owing to the gas that escapes during the taking of the specific gravity†?" This nonsense is quite deliberate. It is copied *verbatim* from his fifth edition. Yet the whole is an affair of simple proportion, level to the capacity of the youngest school boy. 100 cubic inches of muriatic gas weigh, according to him, 39.162 grains; hence

(1.) 100 : 39.162 :: 515 : 201.6843, and *not* "308 grains nearly," as he states.

(2.) (25.252 + 201.68) : 201.68 : : 100 : 0.444, and *not* "0.548, or more than half its weight."

(3.) 252.52 : (252.52 + 201.68) : : 1 : 1.798 = the increased *weight*, which a volume of water acquires by combining with 515 volumes of muriatic acid gas. And the volume being inversely as the specific gravity, if we divide that weight by the given specific gravity, we shall have the *volume* of the liquid. Hence

(4.) $$\frac{1.798}{1.203} = 1.4946$$

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* *Annals of Philosophy*, March, 1819, p. 182.

† *System*, II. 215.

which is very nearly 1.5. Thus 6 parts become 9, and not 11, as he *would* have them to be. What becomes now of the question, with which he concludes?

Muriate of ammonia, and the other muriates, are placed among *primary* compounds, in utter disregard of his own distinctions. By thus separating them from the other saline bodies, or secondary compounds, to which they strictly belong, he destroys all appearance of system. Nay, further, under the title *muriatic acid*, we have his prolix and not very edifying account of the several processes for extracting subcarbonate of soda from sea salt. Here, we perceive nothing new in this edition, except brief interpolations concerning the muriates of cadmium, and of the vegeto-alkalies, the latter of which, ought by his own notions, to have been placed among the salts of these bases. Much perplexity, and teazing references to the first volume, are occasioned by his jumbling together the chlorides and muriates. In like manner, the title, *iodic acid*, is a misnomer; the article consists wholly of a misplaced detail of the iodates. Of the acid itself he gives no account. The hydriodates and the iodides, share the same confusion with the muriates. Under *fluoric acid*, we are told nothing of this substance itself, but we have an unsatisfactory detail of its salts. These are vitiated thoroughly, as to their proportions, by his extravagant error of "considering the weight of an atom of fluorine to be 2;" whereas it is, by simple arithmetic, in his own view of its nature, 2.25, and the fluoric acid 2.375, from Berzelius's latest researches on fluate of lime. This makes its atom, 0.125 × 19, when regarded with Dr. Prout, as a multiple of hydrogen, by a whole number.

The formula which he gives, as from M. Gay-Lussac, for preparing chlorocyanic acid, evidently furnishes a mixture of that acid, and the muriatic. M. Thenard, who probably comprehends the language and process of his distinguished colleague, gives the following directions for preparing his chlorocyanic acid, in the second edition of his *Traité*: "On peut le préparer, en faisant passer un courant de chlore, dans une dissolution d'acide hydrocyanique jusqu'à ce qu'elle décolore l'indigo dissous dans l'acide sulfurique, le privant de l'excès de chlore qu'elle contient, par le mercure, *et la soumettant ensuite à une chaleure modérée.* On obtient ainsi un fluide *elastique*, qui possède toutes les propriétés attribués à *l'acide prussique* oxigéné. Cependant ce fluide n'est point de l'acide chloro-cyanique pur; car celui-ci ne peut exister que liquide sous la pression de l'atmosphère, à la temperature de 15 à 20 degres; c'est un melange d'acide carbonique et d'acide chloro-cyanique dans des proportions variables qu'il est difficile de determiner. L'acide chloro-cyanique ainsi obtenu est incoloré; son odeur est si vive, qu'à une très-petite dose, il

irrite la membrane pituitaire, et determine le larmoiement; sa densité determinée par le calcul est de 2.111; il rougit le tournesol, n'est point inflammable, *&c.*"*

M. Gay-Lussac, indeed, proved that it was not naturally gaseous, by placing the liquid, before applying heat to it, in a jar, inverted over mercury, under the receiver of an air-pump; and exhausting the air, till the vapour had expelled the liquid. On re-admitting the atmospheric pressure, the whole vapour condensed again. Now Dr. Thomson mistakes this experiment of *probation*, for the process of *production*, and omits the latter entirely. "In my experiments," says M Gay-Lussac "it was mixed with carbonic acid gas. It would have been more advantageous, if instead of this gas, an insoluble gas had been present; but after finishing my analysis of the *mixture* of chloro-cyanic acid and carbonic acid, I did not think that I should add to its accuracy, by repeating it with another mixture: .... It is colourless, its smell is very strong. A very small quantity of it irritates the pituitary membrane, and occasions tears. It reddens litmus, is not inflammable, and does not detonate when mixed with twice its bulk of oxygen or hydrogen. Its density, determined by calculation, is 2.111, *&c.*†"

Let us see how our Doctor travesties the French philosopher. "Chloro-cyanic acid, thus obtained, possesses the following properties. It is a colourless *liquid*, having a very strong and peculiar odour, so that a very small quantity of it irritates the pituitary membrane, and occasions tears. It reddens infusion of litmus, is not inflammable, and does not detonate when mixed with twice its *weight* of oxygen, or with hydrogen‡." The acid which M. Gay-Lussac examined was not liquid, it was rendered gaseous by admixture with carbonic acid, and thereby separated from muriatic acid, which must exist abundantly in the Doctor's acid, in consequence of the union of chlorine with the hydrogen of the hydro-cyanic acid. "This liquid mixture does not detonate when mixed with twice its weight of hydrogen." Who would expect that it should? Its density is undoubtedly much greater than that of water; but twice its *weight* of hydrogen will be eleven thousand eight hundred times its bulk, supposing its density equal only to water. "It reddens litmus." Who can doubt it? It consists of an atom of muriatic acid, mixed with an atom of the chloro-cyanic acid, dissolved in water. It would be much wiser for the Doctor to stick to literal transcription. He never alters the language of the original author, without impairing its force, or changing its mean-

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* *Traité*. Tome II, 540.

† *Annales de Chimie*, Tom. 95. The above is Dr. Thomson's translation, in his *Annals* for July 1816, p. 48; which renders the mistake in his system more ridiculous.

‡ *System*, II. 290.

ing. This reminds us of the late Mr. Heron, a translator of Fourcroy, who, meeting the expression, "precipite per se," imagined that the two last words were one, (perse) and produced under mercury, a Persian precipitate, to the no small astonishment of the English chemists.

The sulpho-chyazic acid of Mr. Porrett, which the Doctor, in his 5th edition, by atomic juggling, converted into "a compound of 1 atom of cyanogen + 3 atoms sulphur," in defiance of Mr. Porrett's experiments, has now become a compound of 2 atoms sulphur + 1 atom hydrocyanic acid. We have no patience to follow him through these atomic tortuosities.

The Doctor's dilemma between hypothesis and experiment on the ferrochyazic, or ferroprussic acid, is quite comical. "I consider it as proved that the acid is composed as follows:

2 atoms carbon	=1.500
1 atom azote	1.750
1 atom hydrogen	0.125
½ atom iron.....	1.750
	5.125

"But as the equivalent number 5.125 cannot be reconciled to the composition of the salt, I see no other alternative than to suppose that the iron, in reality, amounts to a whole atom, although I have only been able to obtain half an atom. On that supposition, ferro-chyazic acid must be composed as follows:

2 atoms carbon	=1.500
1 atom azote	1.750
1 atom hydrogen.....	0.125
1 atom iron.....	3.500
	6.875

"This would make the weight of the equivalent number for the acid 6.875. I am disposed to suspect that it will ultimately turn out to be 6 75, which would be the weight of an atom of cyanogen, united to an atom of iron*." What are we to believe, amid these threefold contradictions? and what becomes of his "considering it as proved" that the first proportion is correct? His first atomic proportion gives 34.186 *per cent.* of iron; his second, 50.9 in the same acid. Can his analytical methods not furnish him a better approximation than these two numbers? Under ferrochyazate of potash, he says not one word on its composition, though it is, undoubtedly, the most interesting salt to the practical chemist in his whole system. The ferrochyazates, too, are all misplaced; they are in no sense *primary compounds*.

* Syst., II 311

In treating of sulphuric ether, he gives the atomic view of its constitution, as if deduced by himself, from Mr. Dalton's experiments; but M. Gay-Lussac is the sole author of that important demonstration. If we turn to the *Annales de Chimie*, Tom. 95, for July, 1815, we find the following statement, "M. Gay-Lussac, pense que l'analyse de l'ether sulfurique, par M. de Saussure, n'est point exacte. Il croit que cet ether est composé de deux volumes de gas hydrogène percarboné (olefiant gas) et d'un volume de vapeur d'eau ou en poids de 100 d'hydrogène percarboné, et de 31.95 d'eau, parce qu'en effet, en ajoutant deux fois la densité de gas hydrogène percarboné a celle de la vapeur d'eau, c'est à dire 0.978 *plus* 0.978 à 0.625, on obtient 2.581, qui ne diffère que de 5 millièmes de 2.586, densité de la vapeur d'ether. Cette vapeur resulterait donc de deux volumes de gas hydrogène percarboné, et d'un volume de vapeur d'eau condensées en un seul*." Yet our author claims for himself, in 1817, the merit of a research published by M Gay-Lussac in 1815, and reprinted with just commendations by M. Thenard in 1816? "The experiments,' says the Doctor, 'which Mr. Dalton has made on the analysis of ether, shew in a very satisfactory manner, that the notion which *I* threw out in my *System of Chemistry* (of 1817), 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." "Hence

2 volumes olefiant gas weigh	1 9416
1 volume vapour of water	0.625
Total	2.5666
Specific gravity of ether vapour is	2 5860†

In treating of muriatic ether, he quotes M. Thenard's experiments from the *Memoires d'Arcueil*, Tome I, published in 1807; but entirely conceals the more recent researches of M. M. Colin and Robiquet in the *Annales de Chimie et de Physique*, I., p. 348, published in April 1816, where they shew that, this ether passed through an ignited tube is converted into muriatic acid and carburetted hydrogen, and that its constitution may be represented by 1 volume of olefiant gas + 1 volume of muriatic acid, condensed into one volume; for the sum of the densities of these gases, is nearly the same with the density of muriatic ether vapour. The Doctor sinks all this from abroad, and brings the view forward, as if this idea of its constitution, were a suggestion of his own. "I have very little doubt," says he, "that this ether is a compound of 1 volume of olefiant gas + 1 volume muriatic acid gas, condensed into one volume. On this suppo-

* We print the above passage, as it is quoted by M. Thenard, in his *Traité*, Tom. IV, p 289, published in 1816, to shew the general promulgation of this view of ether, long prior to the Doctor's 5th Edition.

† THOMSON, in *Annals of Philosophy*, August, 1820, p. 81.

sition, the specific gravity will be that of olefiant gas and that of muriatic acid gas added together *."

On his salts, we shall waste few words. Atomical conceits perpetually vitiate his descriptions of them. Thus in treating of the carbonate of ammonia of the shops, he says, "When in its perfect state, this subspecies is composed of 1 atom carbonic acid, and 1 atom of ammonia, or by weight of

Carbonic acid,	2.75	56.41	100
Ammonia,	2.125	43.59	77.27

But the longer it is kept, the greater is the proportion of carbonic acid, and the smaller the proportion of ammonia, which it contains, because the alkali gradually makes its escape into the atmosphere. I have obtained it from shops in London composed as follows:

Carbonic acid,	55.70	100
Ammonia,	21.16	46.98
Water,	18.13†	

How is the water introduced into his "perfect" salt; for it is not deliquescent? In truth, the Doctor knows very well, that this is not the constitution of the solid subcarbonate of the shops. The commercial salt is *never* "composed of 1 atom carbonic acid and 1 atom of ammonia." It consists of 3 atoms carbonic acid + 2 atoms ammonia + 2 atoms water; which form the pungent smelling compound. By exposure to air, this becomes a scentless salt, consisting of 2 atoms carbonic acid + 1 atom ammonia + 2 atoms water; so that it loses an atom of carbonic acid and an atom of ammonia = one atom of M. Gay-Lussac's carbonate, in the transition‡. On sulphate of ammonia we observe one of his quotations to be corruptly given. On turning to his reference (*Annals of Philosophy*, X. 294), we find the sulphate deduced by experiment as consisting of

Sulphuric acid,	61.00	1 atom is	60.60
Ammonia,	25.96	1	25.76
Water,	13.04	1	13.64

The Doctor chooses to make it, acid 60, base 40 in 100 parts. His own *theoretical* composition "is acid 70.17 + base 29.83 = 100." We do not profess to know what Dr. Thomson means by theory; whether it be an explanation *founded* in *facts*, and *adequate* to account for the phenomena; or an explanation *unconnected* with facts, and *inconsistent* with the phenomena.

The salts, generally speaking, remain with all the errors and imperfections of the old edition. We shall give one example, among many, of the vitiation of this part of chemistry, by atomical conceits. "According to Proust, the acetate of copper is composed of,

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* System, II, 359. † Ibid., II, 413,

‡ *Annals of Philosophy*, X. 206; and this *Journal*, VII. 294.

61 acid and water
39 oxide

If we suppose it a compound of 1 atom acid, 1 atom oxide, and 3 atoms water, its constituents will be,

Acetic acid	25.12
Peroxide of copper	39.41
Water	35.47
	100.00

"*I consider these to be its true constituents**." But there is not a shadow of evidence for this random decision. If for 25 per cent of acid, he had guessed 50; and for 35½ nearly of water, one fourth of the quantity, he would have come pretty near to the truth. Such temerity of error destroys all confidence in the statements; nor can practical men derive any benefit from them, in conducting their operations.

Of the third volume, the first 160 pages, are taken up with Dr. Thomson's *notions* of the philosophy of chemistry; or, "an account of the nature of the power which produces combinations." The following is the incipient sentence. "All the great bodies which constitute the solar system, are urged towards each other by a force which preserves them in their orbits and regulates their motions." The force which *urges them together, preserves them in their orbits!* Every school-boy knows that the system of the world is sustained in order, by the "blended powers of gravitation and projection," acting on its revolving spheres. "Conantur ea omnia a centris orbium recedere; et nisi adsit vis aliqua *conatui isti* contraria, qua cohibeantur, et in orbibus retineantur, quamque ideo centripetam appello, abibunt in rectis lineis, uniformi cum motu†." The venerable words of Newton will atone to our readers, for wasting a moment of their time with so plain a matter.

In the section on gaseous constitution, to which no praise, either for ingenuity or research, can be given, Dr. Thomson enumerates the solids, sulphur and iodine, among the simple gases. Now surely, phosphorus, carbon, boron, mercury and all the metals, have as good a claim to admission among the simple gases, as the above two solids; and indeed, had he been desirous to give us a systematic view of *combining volumes*, he should have done so. "Of these gaseous bodies," says he, "there is one whose specific gravity is equal to the weight of its atom. This is oxygen.

Specific gravity oxygen being 1	Weight of an atom
Oxygen 1.000	1.000

Sixteen of them have their specific gravity equal to half the weight of their atoms. These are chlorocarbonic acid, chlo-

* *System* II. 644.
† *Newtoni Principia*, 4to, editio tertia. Definitio quinta.

rine," &c. "Five of them, have their specific gravity, equal to one fourth the weight of their atoms. These are hydriodic acid, muriatic acid, deutoxide of azote, hydrocyanic acid, and ammonia. It follows as a consequence from the preceding facts, that the number of atoms in a given volume of these 3 sets of gases, are to each other, as the following numbers:

First set.....4
Second set.....2
Third set.....1*"

We ask Dr. Thomson, "Do you think Aristotle right, when he says that relatives are related? Do you judge the analytical investigation of the first part of your enthymen, deficient, *secundum quoad*, or *quoad minus?*" Against his first grand proposition, that if we assume the specific gravity of oxygen to be 1, and the weight of its atom to be 1, then "there is one body whose specific gravity is equal to the weight of its atom," we have nothing very forcible to urge, but only to congratulate him on this instance of invention. The enigmatic empiricism of his following propositions, may deserve a little developement. As in the Daltonian hypothesis, which Dr. Thomson has long laboured to expound, the atomic unit is half a volume of oxygen; so, in order to convert the atomic relations of other bodies, into relations by volume, we must multiply their atomic weight, by that of half a volume of oxygen gas, which is 0.5555, when atmospheric air is called 1.; but 0.5 of course, if oxygen gas be assumed as the standard of gaseous density, or 1. Specific gravity is merely the relative weight of equal volumes of matter. If oxygen be, therefore, taken as the standard to which both atomic weight, and weight of volume is referred, other volumes will become = one half of their atomic weights, or = atom × 0.5, instead of = atom × 0.5555. In this hypothesis, 2 volumes of hydrogen form unity. But if *one* volume of hydrogen come to be accounted unity in any case, as we must consider it to be with regard to muriatic, hydriodic, and hydrocyanic acids, as well as ammonia, then the volumes, or specific gravities, will become = atom × 0.25, instead of atom × 0.5 as in the second case, or atom × 0.555, as in the common reduction to atmospheric air. And as to deutoxide of azote, one volume of it contains only half a volume of oxygen, and is therefore equivalent to a *single* volume of hydrogen, or half the Daltonian unit of volume, $= \frac{0.5}{2}$, in the above case. So that Dr. Thomson's tabular distinctions are merely different aspects of his atomic Proteus, which he mistakes for the different aspects of nature, and holds out to his readers as essential distinctions in the constitution of things.

* *System*, III. 25.

His sections on the combination of gases (and indeed the whole of this part of his work) are, with trivial alterations, reprinted from the former edition, and are for the most part, a repetition of what is given, with sufficient prolixity, in the first volume. We should like to know the use of reprinting, at the present day, Mr. Kirwan's table as one that "exhibits the increase of density which takes place, when sulphuric acid of the specific gravity 2.000 is mixed with various proportions of water by weight?" He is well aware, that this ingenious, but multifarious chemist, must have either operated on an acid excessively contaminated with saline matter, or that his numbers are hypothetical; and his results are very inaccurate. Dr. Thomson could have found, if he had chosen to look into the pages of this Journal, modern tables, from which it appears, that 50 of sulphuric acid + 50 of water give 0.107, for the increase of density by combination, and not 0.1333, as he takes it from Mr. Kirwan.

In like manner, he occupies a great many pages with Hassenfratz's tables of saline solutions, "which exhibit," he says, "the specific gravity of saline solutions, differently impregnated, at the temperature of 55° *." Here we find the specific gravity of the saturated solution of sulphate of soda, the first in the list, to be 1.000, a density which we know to be much too small. The Doctor's fondness to involve questions of plain arithmetic, in algebraic symbols and formulæ, appears in the following passage: "Let D be the weight of a saturated solution which we wish to dilute, S the quantity of salt which it contains; x, the quantity of water to be added; S' the quantity of salt contained in 100 parts of the new mixture; then we have

$$\frac{D + x}{S} = \frac{D}{S'} \text{ hence } x = \frac{SD - S'D}{S'}$$

Suppose the solution which we have, to be nitre, and D = 100. From the table we see that a saturated solution of nitre, contains 24.88 per cent of salt; therefore S = 24.88. Let it be required to reduce it, so that it shall only contain 10 per cent. of salt, here S' = 10. We have therefore,

$$x = \frac{2488 - 1000}{10} = 148.8$$

So that to 100 parts of the saturated solution, if we add, 148.8 parts of water by weight, we shall form a new solution, containing only 10 per cent of salt†." The formula is taken from Hassenfratz, but the tautology is his own. The Professor should observe the Horatian precept, *Nec Deus intersit*. A simple statement of proportion would place the thing much more clearly before the student, and bring him, as

* *Syst.* II., 93. † Ibid., III. 100.

expeditiously to his purpose. For 10 : 90 : : 24.88 : 223.92; of which water, 75.12 are already present in the saturated solution. The difference 148.8, therefore, is the quantity of water, that should be added to 100 of the above saturated solution, to produce the desired degree of dilution $= \frac{1}{10}$

And this rule is immediately convertible into the following general form. Let W : S, be the proportion of water to the salt in 100 of the saturated solution; w : s, the proportion required in the dilute; then the quantity of water to be added to 100 of the former, or $x = \frac{Sw}{s} - W$.

We come at length to his title "Part II. *Chemical Examination of Nature*," as if the preceding 1500 pages were not a Chemical Examination of Nature. In the first two books, on the Atmosphere and Waters, we can find nothing new, except a Table compiled from "Bladh, Horner, John Davy, and Marcet," of the specific gravity of sea water, in different parts of the ocean.

Mineralogy, which begins now to assume the systematic aspect of the other parts of Natural History, by the labours of Werner, Hauy, Mohs, and Jameson, is here exhibited in a truly chaotic state. He has no allusion whatever to the natural history method of Mohs, which promises to do for the study of minerals, what the sexual system did for plants; enabling a person, on taking up a specimen, to refer it to its peculiar class, order, genus, and species, till he discover its name and various relations. His first chapter, "On the Description of Minerals," is copied from Professor Jameson's *Treatise on the External Characters*. We find the same chapter, in the same words, in the former edition, but with a reference to Mr. Jameson, which is now suppressed. The only observable alteration, indeed, in his present article on Mineralogy, is the erasure of Professor Jameson's name, wherever it formerly occurred.

If the mineralogy be a useless heap of typography, one might have expected some compensation in the chapter "On the Analysis of Minerals." But greater disappointment met us here. The descriptions of the processes transcribed from celebrated analysts, are so meagre and incorrect, as to be most delusive guides to the practical investigator. Under the analysis of sulphuret of silver, of iron, and of molybdenum, we are told that "100 parts of the dried precipitate, (sulphate of barytes,) indicate about 14.5 of sulphur." Now they indicate, at utmost, on his own *data*, only 13.56, a serious difference in modern analysis. In narrating Klaproth's analysis of antimoniated silver, he says, "Common salt occasioned a precipitate which weighed 87.75 (muriate,) equivalent to 65.81

of pure silver*." But Klaproth gives here 88.75 of muriate of silver, which are equivalent to 66.87 of metal. The Doctor's erroneous number 87.75, is *not* equivalent to 65.81, as he asserts, but to 66.11, computed from his own atomic weights.

Under analysis of red copper ore, he says, "88 parts of the precipitated copper being equivalent to 100 of the orange oxide of which the ore is composed." Now 88 are equivalent to 99, not 100. "The analysis of the oxides and carbonates of copper," says he, "scarcely requires any remarks. The water and carbonic acid, must be estimated by distillation in close vessels, and collecting the products. The ore may then be dissolved in nitric acid, and its copper ascertained as above†." This is all he says; and what can a student make of it? The quantity of carbonic acid is to be estimated by the loss of weight, which 100 grains of the ore sustain during their solution in dilute nitric acid; and the proportion of water is found by subtracting that quantity from the total loss of weight, which another 100 grains suffer by ignition. But if arsenic, or other volatile matter, be suspected, the water may be estimated at first, by a regulated desiccation. Under the Analysis of Arseniate of Copper and Iron, he says, "Nitrate of lead was mixed with the solution; 100 parts of the precipitate indicated 33 of arsenic acid." Now by Berzelius's estimate, which he adopts in his first volume, 100 are equivalent to 33.52.

"Tin-stone," says he, "was thus analyzed by the same celebrated chemist, (Klaproth); 100 parts of the ore were heated to redness with 600 parts of pot-ash in a silver crucible‡." Klaproth says, "One hundred grains of tin-stone from Alternon in Cornwall, previously ground to a subtile powder, were mixed in a silver vessel with a *lixivium*, containing 600 grains of caustic potash. This mixture was evaporated to dryness in a sand-heat, and then moderately ignited for half an hour§." By following Dr. Thomson's misdirection, a satisfactory analysis cannot be made.

Of three different ores of antimony, he gives three plans of analysis, two of which are copied from Klaproth, and the third *must be* his own. The whole are, however, erroneous as given by Dr. Thomson. "Native antimony," says he, "was thus analyzed by Klaproth; 100 grains were digested in nitric acid, till the whole was converted into a white powder; when the acid emitted no longer any nitrous gas, the mixture was diluted with water, and thrown upon a filter. The solution was then treated with nitrate of silver. The precipitate yielded by re-

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* *System*, 6th Edition, III. 605. † Ibid, III. 608.
‡ *System*, III. 609. § *Analytical Essays*, I. 53. English translation.

duction one grain of silver *." Klaproth never wrote such nonsense as this. "Upon one hundred grains," says this genuine chemist, "of pulverized and pure native antimony from Andreasberg, I affused nitric acid, which, on the application of heat, attacked it with vehemence, and soon converted it into a white oxide. When, on the addition of a fresh quantity of acid, no more red vapours arose, I diluted the mixture with water, filtered, and combined it with *muriatic* acid. Muriated silver fell down, which, upon reduction, gave one grain of metallic silver †." This is intelligible procedure; the addition of nitrate of silver, which he imputes to Klaproth, would have been irrational.

"Sulphuret of antimony is to be treated," says the Doctor, "with nitro-muriatic acid. The sulphur and the muriate of silver, (if any silver be present,) will remain. Water precipitates the antimony; sulphuric acid, the lead; and ammonia the iron ‡." Compare with this, the following: "Sulphuret of copper may be dissolved in muriatic acid, by the help of nitric acid. Part of the sulphur separates, part is acidified §." In like manner, when sulphuret of antimony is treated with nitro-muriatic acid, a portion of the sulphur is acidified, which instantly falls down in an insoluble sulphate of lead, along with the insoluble muriate of silver. And water will *not* precipitate the whole antimony, as we shall presently see. So much for his own formula.

"Klaproth," says the Doctor, "analyzed the red ore of antimony as follows: 100 grains were digested in muriatic acid, till the whole dissolved, except 1½ grains of sulphur. A little sulphuret of antimony rose with the sulphuretted hydrogen gas exhaled, and was deposited in the beak of the retort. The solution was diluted with water. The *whole* precipitated in the state of a white powder; *for potash threw nothing from the liquid* ‖." Klaproth says something very different. "The antimony contained in the solution was precipitated in the state of a white oxide, by diluting it with water, and the small portion of the metal still remaining in that fluid, was afterwards *entirely thrown down by means of potash*. The oxide thus procured, was *re-dissolved* in muriatic acid, the solution diluted with six times its quantity of water, and once more combined with such a proportion of the same solvent, as was necessary in order to re-dissolve entirely that *portion* of the oxide which the affused water had precipitated. After the dilute solution had in this manner again been rendered clear, its ingredient antimony was reproduced as *metallic antimony*, by immersing polished iron in the liquor ¶.'" We see, there-

* *System*, III. 613. † *Analytical Essays*, II. 136. English translation.
‡ *System*, III. 613. § Ibid., 607. ‖ Ibid., 613.
¶ *Analytical Essays*, II. 143.

fore, that muriatic acid is the appropriate solvent of the oxide of antimony; a fact of which Dr. Thomson seems ignorant, though he transcribes the process, of which this fact is the ground-work.

His directions for procuring pure antimony, are of a piece with the above. "Antimony may be dissolved in nitro-muriatic acid, and precipitated by the affusion of water. The precipitate is to be mixed with twice its weight of tartar, and fused in a crucible. A button of pure antimony is obtained*." If bismuth be present in the antimony, the two metallic oxides will go down together, and a button of pure antimony will *not* be obtained. Nay, further, suppose the antimony associated with tin, it is impossible to separate the two metals, by solution in nitro-muriatic acid, and affusion of water; for the oxide of antimony carries down with it, in a state of combination, a large quantity of oxide of tin. See *Annales de Chimie*, Tome 55, p. 276.

On his fourth volume we need not enter into details. It is the same abstract from books of natural history, mixed up with a little chemistry, as we found in his former editions. The carelessness with which it is reprinted is conspicuous in the very first paragraph. "Vegetables," says he, "are too well known to require any definition. They are, perhaps, the most numerous class of bodies belonging to this globe of ours; the species already known, amounting to no less than 30,000, and very considerable additions are daily making to the number†." If we look into his analysis of Bonpland and Humboldt's "Nova genera et species plantarum," in the Annals of Philosophy for May 1816, we find him stating, that "Botanists at present are acquainted altogether with 44,000 species of plants; while the whole number, mentioned by the Greeks, Romans, and Arabians, does not exceed 1,400‡."

A considerable part of the fourth volume is professedly devoted to physiology, or an examination of the physical functions of living beings, vegetable and animal. How mawkish the composition is, the following average specimen will shew. "Why do plants die? This question can only be answered by examining, with some care, what it is which constitutes the *life* of plants; for it is evident, that if we can discover what that is which constitutes the life of a plant, it cannot be difficult to discover whatever constitutes its death. Now the phenomena of vegetable life are, in general, vegetation. As long as a plant continues to vegetate, we say that it lives; when it ceases to vegetate, we conclude that it is dead§." This is after all untrue, as he immediately begins to recollect; for vitality exists in a seed or root, without active vegetation.

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* *System*, III. 621.
† Ibid., IV. 1.
‡ Ibid., page 374.
§ Ibid., IV. 361.

The chemical changes which accompany or constitute fermentation, form a very interesting department of the science; and have derived such illustration from modern inquiry, that we expected his account to be somewhat clear and consistent, at least, if neither ingenious nor profound. But here, alas! the most luminous emanations of chemical philosophy, in passing through the doubly refracting medium of his pages are depolarized and dissipated.

The general result of vinous fermentation, is the conversion of sugar into carbonic acid and alcohol. M. Gay Lussac, in 1815, elegantly deduced from the experiments of Saussure, that a volume of alcohol vapour, consists of a volume of olefiant gas, and a volume of vapour of water, condensed into a single volume. In determining the density of the vapour of the absolute alcohol of Richter, he discovered that when that liquid is diluted with water, the density of the vapour of the mixture is exactly the mean between the density of the absolute alcoholic vapour, and that of the aqueous vapour; the affinity which condenses the liquid compound, not operating in the gaseous state. Hence it is evident, that absolute alcohol contains no independent aqueous matter. We may therefore state the composition of alcohol in numbers thus:

	Weight of volume.	Per cent. by theory.	Per cent. by Saussure.
Olefiant gas	0.9722	60.87	61.13
Aqueous vapour	0.625	39.13	38.87
Density of alcoholic vapour =	1.5972	100 00	100.00

Hence these 39 parts of water are essential to its constitution; which may be represented atomically by,

2 atoms carbon	= 1.500	(2 atoms carbon	= 2 atoms olefiant gas.
3 atoms hydrogen	= 0.375	+ 2 hydrogen)	
1 atom oxygen	= 1.000	= 1 oxygen	= 1 atom water.
		+ 1 hydrogen)	

Without this atom of water, therefore, the peculiar compound, alcohol, could not exist. It would in that case become olefiant gas. Let us see what our author says on the subject. "Now alcohol of the specific gravity 0.822, contains one-tenth of its weight of water, which *can* be separated from it; and if we *suppose* with Saussure, that *absolute* alcohol contains 8.3 per cent. of water, then the products of sugar decomposed by fermentation, according to the preceding experiment, are as follows:

Alcohol	47.70
Carbonic acid	35.34
	83.04

Or in 100 parts,	Alcohol	57.44
	Carbonic acid	42.56

This result approaches so nearly that of Lavoisier, that there is reason to suspect that the coincidence is more than accidental*." This imputation against the honour of M. Thenard, whose experiments he is canvassing, is unwarranted. But as Dr. Thomson not merely *adopts* the atomic theory of M. Gay-Lussac, but gives it as his own, we should beg him to tell us, what absolute alcohol will become, when deprived of 8.3 per cent. of its constituent water. We see plainly that 60.87 : 39.13 :: 1.75 : 1.125; but take away 8.3 per cent. of water from alcohol, and we shall have a remainder of 60.87 of olefiant gas + 30.83 water; now 60.87 : 30.83 : : 1.75 : 0.888. Here, therefore, we have the atomic weight 1.75 of olefiant gas, associated with 0.888, instead of 1.125 of water; or *his* atom of the former, with about 8-10ths of an atom of the latter.

His subsequent atomic view of the conversion of sugar into carbonic acid and alcohol, is copied closely from M. Gay-Lussac, *Annales de Chimie*, Tome 95 (for July, 1815).

We have now fatigued ourselves, and we fear many of our kind readers, with the length and minuteness of our remarks.

Besides the errors and defects which we have noticed, there are others in every page, occasioned chiefly by the incessant twisting, stretching, and curtailing, of experimental results, to suit some fantastic atomical dress.

We have animadverted on the style *passim*. It is feeble, discontinuous, and ungrammatical. But it is the spirit of the book which we most dislike, and which we think calculated to awaken jealousy and misunderstanding, where the most cordial unanimity should subsist. We have endeavoured, from the purest motives, to apply the corrective powers of criticism, to this spirit. But the effectual mode of laying it, would be for some person of judgment, temper, and taste, to execute the laborious task, of uniting into one comprehensive and systematic body, the well authenticated results of chemical investigation.

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* *System*, IV. 377.

ASTRONOMICAL AND NAUTICAL COLLECTIONS. No. V.

i. *M.* DELAMBRE'S *direct Method of computing the Latitude from Two Observations of the Sun's Altitude, and the Time elapsed between them. From the* Connaissance des Tems *for* 1822; *with Remarks.*

THERE is scarcely any problem in Nautical Astronomy so important, or of so frequent occurrence, as the determination of the latitude and the time from two observations of altitudes: and the future improvements, which may be anticipated, in the construction and general employment of timekeepers, will probably render this computation almost the only one that will be required for determining a ship's place in all common cases.

The approximative method of Douwes has been recommended in the Requisite Tables, and generally practised in the British Navy: but, like most other contrivances of the kind, it generally gives more trouble than it is intended to save. Dr Brinkley has proposed two much simpler and more convenient methods, which, however, agree with it in requiring a supposed latitude, as an element of the computation; and it seems to be at least more elegant to assume nothing that is not immediately derived from the observations themselves.

M. Delambre, in an essay published in the *Connaissance des Tems* for 1822, has made the very important remark, that the method of Douwes is not only as long, if once repeated, as the direct method, but that it is wholly void of any convergence, since, when conducted with rigid accuracy, it leads back precisely to the supposed latitude, at least when that latitude happens to be very near the mark. He has given examples, in this elaborate paper, of the strict trigonometrical mode of computation, and of an improved formula invented by M. Querret of St. Maloes. M. Dubourguet, of Dieppe, has also very lately

communicated a similar improvement to the respectable veteran Von Zach, who has inserted it in the number of his valuable Correspondence, bearing the nominal date of March, 1820. But Mr. Querret's formulæ, though more geometrically accurate than Mr. Delambre's, possess no practical advantage over them, and Mr. Dubourguet's method appears to be almost exactly the same as one of those which Mr. Delambre has employed.

Rule for double Altitudes.

1. Having corrected one of the observations for the change of the ship's place during the interval, take the logarithmic sine of the mean polar distance $\frac{1}{2}$ (PA+PB), and the sine of half the interval converted into space, that is, $\frac{1}{2}$ t; add them together, and the sum will be the sine of half the distance AB.

2. Then as the sine of AB is to the sine of the opposite angle APB, so is the sine of one of the polar distances; for instance, the *second* PB to the opposite angle PAB.

3. Having thus the three sides ZA, ZB, and AB, we have next to find the angle BAZ. For this purpose, add together the two polar distances ZA, ZB, and the distance AB, and from the half sum subtract the *first* polar distance ZA and the distance AB: add together the sines of the remainders, and the arithmetical complements of the sines of the sides last mentioned, half the sum will be the sine of half the required angle BAZ.

4. The difference of BAZ and PAB, or sometimes the sum, between the tropics, will be the angle PAZ, subtended by the colatitude PZ from the sun's place A. To find this colatitude, take out the logarithmic cosines and sines of the first polar distance and zenith distance, and with the sines set down the cosine of the included angle PAZ: add them separately together, and find the corresponding natural numbers, the sum of which will be the natural sine of the latitude, and its logarithm of course the logarithmic sine. But if the angle PAB lies without BAZ, and their sum exceeds a right angle, the cosine becomes negative, and the difference of the natural numbers must be taken: and if there is any doubt in the computer's mind, it will be easy to try both suppositions.

Note. If the declination is very small, it may sometimes be more convenient to find the angle PAB from the three sides of the triangle, as in the third precept, instead of the second.

EXAMPLE.

Let the two zenith distances corrected be ZA = 73° 54′ 13″, and ZB 47° 45′ 51″, the declinations 8° 18′ and 8° 15′, the interval of time three hours, or PA = 81° 42′, PB = 81° 45′, and APB = 45°, whence ½ (PA + PB) = 81° 43′ 30″. The operation will be thus:

1. L sin ½ (PA+PB)	=81° 43′ 30″	9.9954547	(1)
sin ½ t =	22 30 0	9.5828397	(2)
sin ½ AB =	22 15 11.3	9.5782944	(3)
AB =	44 30 22.6		
2. L. sin AB		A. C. 0.1542889[8]	(4)
sin APB =	45 0	9.8494850	(5)
sin PB	81 45	9.9954822	(6)
sin PAB =	86 38 5[8]	9.99925[70]	(7)
3. ZA = 73° 54′ 13″	L. sin ZA	A. C. 0.0173696	(8)
ZB = 47 45 51	sin AB	A. C. 0.1512898	
AB = 44 30 23	sin (S — ZA)	9.2030232	(9)
2S = 166 10 27	sin (S — AB)	9.7949174	(10)
S = 83 5 13.5		2) 19.1695990	
	sin ½ BAZ = 22° 36′ 26″.6	9.5847995	(11)
S — ZA = 9 11 0.5	BAZ = 45 12 53		
S — AB = 38 34 50.5	PAB = 86 38 58		
(ZB = 47 45 51)	PAZ = 41 26 5		
4. L. cos PA 9.1594354	L. sin PA	9.9954271	(12)
cos ZA 9.4428780	sin ZA	1.9826314	(13)
.040023 8.6023134 (15)	cos PAZ	9.8748934	(14)
.712774	.....	9.8529519	(16)
.752797 N. sin 48° 50′ 0″, the Latitude required.			(17)

ii. *Computation of the Effect of terrestrial Refraction, in the actual Condition of the Atmosphere.*

A. It is well known that a projectile, thrown in any oblique direction, will acquire a height equal to the verse sine of twice

the angle of elevation, in the circle, of which the diameter is the height due to the velocity; and that its horizontal range will be four times the corresponding sine. Hence it is obvious, that when the direction is nearly horizontal, the radius of curvature of the path of the projectiles will be equal to twice the diameter of that circle, or to twice the height due to the velocity, since the chord is twice as great, and the verse sine the same as in the circle, so that the radius must be quadruple.

B. It is also well known, that the tangent of a parabola intersects its diameter at a distance above the vertex, equal to the length of the abscis below it; so that the portion of the abscis below the vertex is half of the part cut off by the tangent.

C. The horizontal ordinate of the parabola, flowing uniformly with the time, is always proportional to the vertical velocity, and the difference of any two proximate ordinates, compared with their length, and the evanescent interval between them, will always give the distance of the intersection of the tangent, according to the common method of finding the tangents of curves; that is, as the difference of the velocities is to the whole velocity, so is the difference of the abscis to the part cut off by the tangent, or to twice the abscis reckoned from the vertex.

D. Now the velocity of light, considered as a projectile, must be supposed to vary directly as the refractive density; so that we have only to determine what proportion the variation of the refractive density of the atmosphere, in the height of a foot or a yard, bears to the whole refractive density, and to increase the foot or the yard in the same proportion, and we shall obtain the measure of twice the height due to the velocity, or of the radius of the circle of curvature of the ray of light moving horizontally through such an atmosphere.

E. The velocity of light in a vacuum, and in the atmosphere at 50°, with the barometer at 30, varies in the ratio of 3540 to 3541; the height of a homogeneous atmosphere, under these circumstances, is 27,000 feet; and the temperature descends about 1° for every 300 feet that we ascend. Consequently the velocity varies $\frac{1}{3540} \cdot \frac{1}{27000}$ in every foot, as far as the diminution of pressure is concerned, and $\frac{1}{3540} \cdot \frac{1}{300} \cdot \frac{1}{491}$ is to be deducted

for every foot, on account of the diminution of temperature, or as much more or less as this diminution is more or less rapid; so that if the change were 1° in $\frac{27000}{494} = 55$ feet, the refraction would be annihilated, and, if still more sudden, there would be a depression or looming, instead of an elevation. But in ordinary circumstances, supposing Professor Leslie's estimate of 1° in 300 to be correct, we have $\frac{1}{3300}\left(\frac{1}{27000} - \frac{1}{118200}\right) = \frac{1}{116873000}$ for the variation in a foot, and consequently, 116873000 feet for the radius of curvature of the ray; which is to the earth's radius, or 20900000 feet, as 5.6 to 1; consequently, the elevation of a distant object must be $\frac{1}{11.2}$ of the angle subtended at the earth's centre, since the angle contained between an arc and its chord is always equal to half the angular extent of the arc.

F. The general temperature of the atmosphere will affect this refraction in so slight a degree, that it may safely be neglected; but it would be always of importance to ascertain, if possible, the comparative temperature at different heights; and whenever it is practicable to find the height h, corresponding to a depression of 1°, supposing it to be different from 300, we may employ as a divisor, instead of 11.2, the reciprocal of $\frac{1}{3300}\cdot\left(\frac{1}{27000} - \frac{1}{494\,h}\right)$ divided by 10450000; or the reciprocal of $\frac{10450000}{3540}\left(\frac{1}{27000} - \frac{1}{494\,h}\right) = \frac{10450000}{3540}\cdot\frac{h - 54.7}{27000\,h} = \frac{.1093\,(h - 54.7)}{h} = .1093 - \frac{5.98}{h}$, which, when $h = 300$, becomes $.1093 - .0199 = .0894 = \frac{1}{11.2}$, as before.

iii. *Note respecting the Connaissance des Tems.*

It is right that the possessors of the *Connaissance des Tems* should be informed, that a cancel and two pages of errata for 1822 were received in London after the delivery of the volume for 1823, to which they belong. It ought also to be generally known to practical astronomers, that the well-intended and well-contrived tables for the correction of the places of the stars,

published in the *Connaissance des Temps* for 1812, have been rendered, by some omission in the calculation, completely erroneous throughout. The volume for 1823 appears to be incomparably more accurate than those of the preceding years, and the editors appear to have profited very laudably by the example of diligence, which has been set them in this country.

iv. *An Essay on the easiest and most convenient Method of calculating the Orbit of a Comet from Observations. By* William Olbers, M. D. 8vo. *Weimar*, 1797.

Section II.

On some Equations of the First and Second Order, which have been proposed for determining the Equations of Comets.

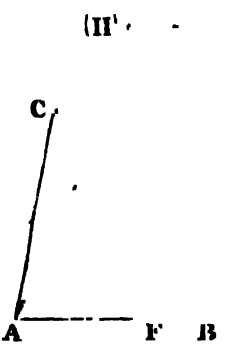
[Continued from Vol. X. p. 426]

§ 35.

Upon this supposition, it will be easy to determine, what would have been the apparent place of the comet at the time of the middle observation, if the earth had been in *d*, and the comet in D. For first, all the apparent places in ADC, viewed from *a d c*, lie in a great circle of the sphere: and, secondly, the points *b b* SDB are all in one plane, so that all the points of the line BS, seen from any part of *b* S, are in the same great circle. We have therefore only to determine the place of these two circles on the sphere, in order to find the position of the line *d* D. The first great circle will pass through the first and third places of the comet; the second through the middle place and that of the sun. Hence, if we make $\frac{\text{tang } \beta'''}{\sin (\alpha''' - \alpha') \text{ tang } \beta' - \cot (\alpha''' - \alpha')} = \cot \pi$, the longitude of the point of intersection of the former circle with the ecliptic will be $\alpha'' - \pi$; and the angle of intersection will be η, to be found by the equation $\text{tang } \eta = \frac{\text{tang } \beta'}{\sin \pi}$. The longitude of the point of intersection of the other circle with the ecliptic must obviously be Λ'', or that of the sun at the time of the middle observation, and its inclination ϑ will be determined by the

equation tang $\vartheta = \frac{\text{tang } \beta''}{\sin (A'' - \varpi'')}$. Hence we may easily find the position of the intersection of these two circles with respect to the ecliptic; for if we put $\cot \sigma = \frac{\text{tang } \varepsilon}{\text{tang } \vartheta} \sin (A'' + \varpi - \varpi') + \cot (A'' + \varpi - \varpi')$, we have $\varpi' - \varpi + \sigma$ for the longitude of this point, which may be called c'', and its latitude γ'' will be such that tang $\gamma'' =$ tang $\varepsilon \sin \sigma$.

[NOTE 5. In order to demonstrate the general proposition, that the relative apparent places of two bodies describing right

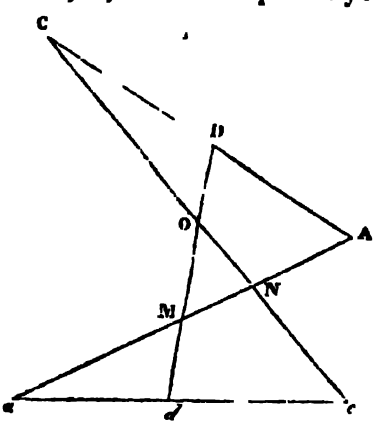


lines with proportional velocities, will be found in a great circle, we must consider, that if the line AB be in the plane of the figure, and CD meet it in C, the great circle, to which both AC and BD will be directed, will be found by drawing AE parallel to BD, and determining the plane which passes through AC and AE. Now, if we take, instead of BD, any other position of the line of direction, as FG, dividing AB and CD in the same ratio, and draw AH parallel to FG, it may be shown, that AH will be in the same plane with AC and AE; for if AE = BD and AH = FG, the points C, H, and E will be in the same right line, their heights above the plane of the figure being in the same proportion, as their distances from C, since these heights are equal to the heights of G and D, which are to each other as CG to CD: and since GH is equal and parallel to AF, it will be to DE as CG to

CD; so that the lines AC, AH, and AE, must always be in one plane.—Tr.]

§ 38.

Since, according to our supposition, the chord of the orbit of the comet, and that of the earth's orbit, are divided by the lines of direction a A, d D, c C, in proportion to the times, the same proportion must also hold good for all orthographical projections of these chords and lines of direction. Supposing now CDA to represent the chord of the orbit of the comet projected on the ecliptic, a c d, as before, the chord of the earth's orbit, and a A, d D, c C, lines determined in their angular directions by the longitudes α', c'', and α''' respectively:



we have then $CO : AM = \frac{CD}{\sin COD} : \frac{AD}{\sin DMA}$ and $cO : aM = \frac{cd}{\sin COD} : \frac{ad}{\sin DMA}$ but since $cd : da = CD : AD = t'' : t'$, and $Cc = CO + cO$, and $Aa = AM + aM$, we have $Aa : Cc = \frac{t'}{\sin DMA} : \frac{t'}{\sin COD}$. Now DMA is the difference of the first and second longitudes, $c'' - \alpha'$; and COD, that of the second and third, $\alpha''' - c''$; and Aa and Cc are the curtate distances of the comet from the earth in the first and third observation, which we have before called ϱ' and ϱ'''; consequently,

$$\varrho' : \varrho''' = \frac{t'}{\sin(c'' - \alpha')} : \frac{t'}{\sin(\alpha''' - c'')}, \text{ and } \varrho''' = \varrho'.$$

$\frac{t'' \sin (c' - a')}{t' \sin (a''' - c')} = M \frac{\varrho''}{\varrho'}$; which expresses the proportion of the curtate distances in the first and third observations.

§ 37.

This mode of finding the proportion of the curtate distances is, however, neither universally applicable, nor always the most convenient. There is one case in which it is quite useless, that is, where the apparent motion is nearly perpendicular to the ecliptic, or the change of longitude very slow, and that of latitude considerable: for in this case the angles $c' - a'$ and $a''' - c''$ would be too small for the determination of M with sufficient security. In another case it must be employed exclusively, when comets are near their quadrature, and have very little motion, especially in latitude; so that the following method is rendered inconvenient. There is also another case, in which it is particularly advantageous; that is, when the intervals are very small, or the observations not very accurate; for in this instance we may, without hesitation, employ the longitude a' of the second observation, instead of the corrected longitude c', so as to supersede the calculation of § 35. This is equivalent to the supposition, that the lines B b and D d, § 34, are parallel to each other; which will not be far from the truth when the arcs $a\,c$ and AC are small, and the lines $b\,d$, BD, still smaller in proportion. In this case we may assume at once

$$M = \frac{t'' \sin (a' - a')}{t' \sin (a - a'')}.$$

§ 38.

In other cases it will be generally more convenient to employ a plane of projection perpendicular to the ecliptic, and to the middle position of the revolving radius belonging to the earth, as LAMBERT has already done with advantage. If we then make $\text{tang } b' = \frac{\text{tang } \beta'}{\sin (A' - a')}$, $\text{tang } b' = \frac{\text{tang } \gamma'}{\sin (A'' - c'')}$ and $\text{tang } b'' = \frac{\text{tang } \beta'''}{\sin (A'' - a''')}$, the angles b, b', and b''' will be those which the projections of the lines of direction will form with the earth's orbit. But since $\frac{\text{tang } \gamma''}{\sin (A'' - c'')} = \frac{\text{tang } \beta}{\sin (A' - a'')}$, the cal-

culation of the values of c'' and γ'' becomes unnecessary. [For in this case the projection of the revolving radius of the comet will coincide with that of the line of the direction in the second observation, the same point representing both the earth and the sun.] If we now call the projected distance in the first observation δ, and in the third N δ, we shall have, since the projected chords are here also divided in the proportion of the times, $N = \frac{t'' \sin(b''-b')}{t' \sin(b''-b'')}$. Now

$$\varrho' = \frac{\delta \cos b'}{\sin(A''-a')}, \text{ and } \varrho''' = M\varrho' = \frac{N\delta \cos b'''}{\sin(A''-a''')}; \text{ consequently}$$

$$M = \frac{\cos b''' \sin(A''-a') \sin(b''-b') t'}{\cos b' \sin(A''-a''') \sin(b'''-b) t'} =$$

$$\frac{\sin(A''-a') (\text{tang } b''-\text{tang } b') t'}{\sin(A''-a'') (\text{tang } b'''-\text{tang } b'') t'} =$$

$$\frac{(\text{tang } \beta' \sin(A''-a') - \text{tang } \beta' \sin[A''-a'']) t''}{(\text{tang } \beta'' \sin(A''-a'') - \text{tang } \beta'' \sin[A''-a''']) t'}$$

a very convenient expression for M; which, however, may be rendered more immediately applicable to calculation in the form

$$M = \frac{(m \sin(A''-a') - \text{tang } \beta') t''}{(\text{tang } \beta' - m \sin[A''-a'']) t'} \text{ making } m = \frac{\text{tang } \beta''}{\sin(A''-a'')}.$$

[*Note communicated by the Author.*

It must here be observed, that the two expressions $M = \frac{\sin(c''-a') t''}{\sin(a'-c') t'}$ and $M = \frac{(m \sin(A''-a') - \text{tang } \beta') t'}{(\text{tang } \beta''' - m \sin(A''-a''')) t''}$ are identical, and may be derived from each other. For since $m = \frac{\text{tang } \beta''}{\sin(A''-a'')}$ is the tangent of the angle formed with the ecliptic, by a great circle drawn through the place of the comet and of the sun in the middle observation, we obtain, by means of a well-known property of great circles, already mentioned in § 30. the two equations

$$\text{tang } \gamma \sin(a''-a') - \text{tang } \beta' \sin(a'''-c') - \beta''' \sin(c''-a') = 0$$

$$\text{tang } \gamma \sin(a'''-a') - m \sin(A''-a') \sin(a'''-c'') - m \sin(A''-a''') \sin(c''-a') = 0$$

consequently $\frac{\sin(c''-a')}{\sin(a'''-c'')} = \frac{m \sin(A''-a') - \text{tang } \beta'}{\text{tang } \beta''' - m \sin(A''-a''')}$.]

§ 39.

Such, therefore, is the proportion of the curtate distances of the comet from the earth in the first and third observations. In order to find the distances themselves, we must determine from them the chord and the two extreme distances AC, SA, SC, §.34, and compare the area of the sector with the time intervening. Now the two distances of the earth from the sun, S α, Sc, being R′ and R″, and the distances of the comet from the sun, SA, SC, r and r'', we have $r'^2 = R^2 - 2R'\varrho' \cos(A' - \alpha') + \varrho'^2 \sec^2\beta'$, and $r'''^2 = R'''^2 - 2R'''M\varrho' \cos(A''' - \alpha''') + M^2\varrho'^2 \sec^2\beta'''$.

[*To be continued.*]

v. *Further Remarks on the Transit of the Comet of* 1819 *over the Sun. By* Dr. Olbers.—Bode's *Jahrb.*, 1823.

The authority of the observation of General Von Lindener, in favour of the invisibility of the comet in its transit, is considerably diminished by the testimony of other observers, particularly Professor Schumacher and Professor Brandes, who agree in declaring, that the sun was by no means free from spots on the day of the transit, as it appeared to General Von Lindener: and on the other hand, Dr. Gruithuisen and Professor Wildt agree in describing a small spot near the middle of the sun's disc, which might possibly have been the comet, though certainly not so distinctly defined as a planet would have been.

vi. *Errors of the Tables of the Planets, with other Notes, from* Bode *and* Zach.

The German observations of Jupiter and Saturn, as recorded by Bode, do not agree quite well enough to settle the question of the accuracy of the tables of their motions, without a reference to the Greenwich Observations. They appear, however, to prove, that Bouvard's tables of both planets are considerably more accurate than Delambre's. The mean error of Bouvard in the H. longitude of ♃, about the time of opposition in 1819,

was + 5″.7 or — 10″, in the latitude + 3″.2 or — 1″, according to Sniadecki and Derfflinger: in the 4th longitude of ♀ — 6″.8 or + 23″; and in the latitude + 7″ or + 6″. Delambre's tables of ♃ gave the longitude — 21″.1, — 19″, — 26″ or + 12″, and the latitude + 1″.7, + 2,″ — 2″, or + 4, according to Sniadecki, Bittner, Bürg, and Derfflinger. For Saturn's longitude, + 63″.1, + 87″, and + 87″, latitude, 0″, + 12″, and + 14″, according to Sniadecki, Bittner, and Derfflinger respectively.—*Bode*, 1823, p. 119, 120, 131, 132, 144, 174, 175.

We find in the *Correspondance Astronomique, for February*, 1820, above thirty observations of the lunar distances from Venus, made at Toulon, for the purpose of ascertaining the accuracy of Inghirami's tables published in that work, and partly copied into these Collections: the greatest error does not exceed 11′ of longitude; and the mean error is much less. There are also thirteen observations of the distance of Jupiter, in which the mean error is still less, and the greatest about 9′.

With respect to the comparative facility of observing lunar occultations, it is remarkable, that of thirty-five conjunctions of fixed stars with the moon, announced in the Berlin almanack for 1819, nine only were occultations visible at Berlin, and Professor Bode was unable, on account of the weather, to observe any one of these.

Professor Hansteen of Christiania, so well known for the accuracy of his magnetical researches, has announced to the Baron von Zach as an important discovery, that polarity is by no means confined to iron; but that the wall of a house, a tree, and the mast of a ship, are capable of producing the effects of a north pole below, and a south pole above. It is well known, that the late M. Coulomb once fancied that he had discovered some magnetic properties in various substances, independently of any iron contained in them: but his experiments were repeated at the Royal Institution without success, and he was afterwards obliged to abandon the opinion. It is said that Professor Hansteen was once a believer in animal magnetism: a circumstance which does not give much weight to his evidence

on this occasion. The subject deserves, however, to be carefully re-examined with respect to this induced polarity, which, if its existence were confirmed, would tend to remove some difficulties in the theory of a ship's attraction.—See *Astronomical Collections*, No. III.

The planet *Vesta* was in opposition, 13th January, 1821. For 3d April, midnight at Paris, M. T., her AR. will be

	111°. 9′	Decl. 27°. 9′ N.	Log. Dist. fr. ☉ .33
April 7.	112°. 5′	26°. 3′	.34
11.	113°. 7′	25°. 56′	.35

Encke, in Bode, p. 225.

Juno will be in ♌ 24th July, 1821. Her place at midnight, M.T., at Manheim, will be

	AR.	Decl.	Log. Dist ☉
May 5.	20ʰ 20ᴹ 34ˢ	5° 46′ S.	.421
June 2.	20 27 57	3 47	.347
July 4.	20 15 48	3 20	.274
Aug. 1.	19 52 23	5 17	.247
Sept. 2.	19 33 10	9 9	.274
Oct. 4.	19 39 17	12 30	.334

Nicolai, in Bode, p. 226.

Place of Pallas.

Midnight at Gottingen, M. T.

	AR	Decl.	Log Dist. ☉
April 1.	252° 8′	16° 21′ N.	.360
May 3.	249 32	23 22	.338
June 4.	243 3	26 11	.354
July 2.	238 46	24 16	.394
30.	238 22	19 54	.446

Von Staudt, in Bode, p. 227.

vii. *Danish Standard of Length. Communicated by Professor* SCHUMACHER.

The length of a pendulum vibrating seconds of mean solar

time in 45° N. latitude on the meridian of Skagen, on the level of the sea, and in a vacuum, is to be divided, according to a new Royal decree, into 38 equal parts, each of which is to be a Danish inch, and 12 inches a foot. All other standards are hereafter to be considered as merely subsidiary to this determination, and to have no authority any further than as they agree with it.

The weight of a cubic foot of water is to be hereafter determined by Professor Oerstedt. The Senate of Hamburg has also adopted the same standard.

Correction.

The example of a lunar distance, copied into the Third Number of these Collections, from the Appendix to the Requisite Tables, contains an error in the tabular logarithmic difference, which was not suspected, and which therefore pervades the other computations, in which that logarithm is employed. This is the true cause of the apparent inaccuracy of the great tables, which, as well as the scales depending on them, are thus vindicated from a groundless imputation.

There is also an inaccuracy in some of the numbers of the example of all the minute corrections, which has tended to exaggerate, in some degree, the importance of these corrections, since they really amount, in the case computed, to about half a minute only, instead of a minute, or to fifteen miles of longitude, instead of thirty: the general tendency of the example is, however, not affected by this error.

Art. XIV. *Corrections in Right Ascension of Thirty-Six principal Fixed Stars to every Day of the Year.* By James South, F.R.S., F.L.S., *Honorary Member of the Cambridge Philosophical Society, and Member of the Astronomical Society of London.*

[Concluded from Vol. X. p. 444.]

A Mr. James Grooby having published in the *Philosophical Magazine* of February last, the Apparent Right Ascension of Dr. Maskelyne's thirty-six Stars, for every day of the months of March and April, curiosity has naturally induced me to examine how far the corrections given by me in the last Number of this Journal, would afford similar results; and, upon mature consideration, I would recommend Mr. Grooby, in his next communication, to revise the preface to his labours of the 12th of February last; substituting *incautiously* for "carefully," *purloined* for "calculated," and *Mr. James South's* for "Dr. Maskelyne's;" *perhaps*, too, the sentence might be improved, were he to add "found by me in Mr. Brande's Journal of January last."

The paragraph will then run thus. "The mean places, (*Right Ascensions*, Mr. Grooby, I presume, means,) were deduced from Mr. Pond's table, annexed to the *Nautical Almanac* for 1823, and the corrections incautiously purloined from Mr. James South's own tables, found by me in Mr. Brande's Journal of January last." Signed (James Grooby.)

To be serious, however, should the proposed alteration sound unmusical in Mr. Grooby's ears, I can assure him it will afford me great pleasure to retract the sentiments it conveys, on his proving that they are unfounded; and for this purpose, all that is necessary will be for Mr. Grooby to do again, but in the presence of mutual friends, what he would have the world believe he has already done, a thing; which he must acknowledge to be *nec asperum, nec difficile.*

James South.

Blackman-Street, March 24th, 1821.

	γ Pegas.	α Arietis.	α Ceti.	Aldebaran.	Capella.	Rigel.	β Tauri.	α Orionis.	Sirius.
	″	″	″	″	″	″	″	″	″
July 1	+2,02	+1,46	+1,28	+0,99	+0,96	+0,65	+0,96	+0,78	+0,81
2	06	49	26	1,01	99	67	99	80	33
3	09	52	29	04	+1,02	69	+1,01	82	34
4	12	55	32	06	05	71	04	84	35
5	15	59	35	09	08	73	06	86	36
6	18	62	38	11	11	75	09	88	37
7	22	66	41	14	14	77	11	90	39
8	25	69	44	17	17	79	14	92	40
9	28	73	47	19	20	81	16	94	42
10	31	76	50	22	23	83	19	96	43
11	34	80	53	24	26	85	21	98	45
12	37	83	56	27	30	88	24	+1,00	46
13	40	87	59	30	33	90	26	02	48
14	43	90	62	32	36	92	29	04	49
15	46	94	65	35	39	95	32	06	50
16	50	97	68	38	42	97	34	08	52
17	53	+2,00	71	41	46	+1,00	37	10	53
18	56	04	74	44	49	02	39	12	55
19	59	07	77	47	52	04	42	14	56
20	62	10	80	50	55	06	45	16	58
21	65	13	83	53	59	09	48	19	59
22	68	17	86	56	62	11	51	21	61
23	71	20	90	59	66	14	54	24	63
24	74	23	93	62	69	16	57	26	65
25	77	27	96	65	73	18	60	29	67
26	80	30	99	68	76	21	63	31	69
27	83	34	+2,03	71	80	24	66	33	71
28	86	37	06	74	84	27	69	36	73
29	89	40	09	77	88	29	72	38	74
30	91	44	12	80	92	32	75	41	76
31	94	47	15	83	96	35	78	44	79
Aug. 1	96	51	18	86	+2,00	38	81	46	81
2	+3,00	54	21	89	04	41	84	49	83
3	02	57	24	92	08	43	87	51	85
4	05	60	27	95	12	46	90	54	88
5	07	63	30	98	16	49	93	56	90
6	09	67	34	+2,02	20	52	97	59	92
7	11	70	37	05	24	54	+2,00	61	94
8	14	73	40	08	28	57	03	64	96
9	16	77	43	11	32	60	06	67	98
10	18	80	46	14	36	62	09	69	+1,00
11	20	83	49	17	40	65	13	72	03
12	22	86	52	20	44	68	16	75	05
13	24	89	55	23	48	70	19	78	07
14	27	92	58	27	53	73	22	81	09
15	29	95	61	30	57	76	25	84	11
16	31	98	64	33	61	79	29	87	14
17	33	+3,01	67	36	66	82	32	90	16
18	35	04	70	39	70	85	36	93	19
19	37	07	73	42	74	88	39	96	21
20	39	10	76	45	79	91	43	99	24
21	41	13	79	48	83	94	46	+2,01	26
22	43	16	82	51	87	97	50	04	28
23	45	19	85	54	91	+2,00	53	06	30
24	47	22	88	57	96	03	57	09	33
25	49	25	91	61	+3,01	06	60	12	35
26	51	27	93	64	05	08	63	15	37
27	53	30	96	67	09	11	67	17	40
28	55	33	99	70	13	14	70	20	42
29	57	35	+3,02	74	17	17	74	23	45
30	59	38	04	77	22	20	77	26	48
31	61	[illegible]	07	80	26	23	81	29	50

	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Virg.	Arcturus
July 1	+1,07	+0,75	+1,05	+0,94	+1,34	+1,82	+1,74	+2,18	+2,52
2	08	76	05	84	34	81	74	17	51
3	09	77	06	84	34	81	73	16	50
4	09	77	07	84	34	80	72	16	49
5	10	78	08	84	34	79	72	15	47
6	11	79	09	84	33	79	71	14	46
7	12	80	10	84	33	78	71	13	45
8	13	81	11	84	32	77	70	12	44
9	14	82	12	84	32	76	70	11	43
10	15	83	13	84	32	76	69	10	42
11	17	84	14	84	31	75	68	09	41
12	18	86	16	84	31	74	68	08	40
13	20	87	17	84	31	73	67	07	39
14	21	89	18	84	31	73	66	06	38
15	23	90	20	84	31	72	65	05	37
16	25	91	21	84	31	71	64	04	36
17	27	93	23	84	31	70	64	03	35
18	29	94	24	84	30	69	63	02	34
19	31	95	25	84	30	68	62	01	33
20	33	96	27	84	30	67	61	00	32
21	35	97	28	85	30	66	61	+1,99	30
22	37	99	30	85	30	66	60	98	29
23	39	+1,00	31	86	30	65	60	97	27
24	40	01	33	86	31	64	59	96	26
25	42	03	35	87	31	63	59	95	25
26	44	04	37	87	31	63	58	94	23
27	46	06	39	87	32	62	58	93	22
28	48	08	40	88	32	62	57	92	20
29	50	09	42	88	32	61	57	91	19
30	52	11	44	89	33	61	56	90	18
31	54	12	46	89	33	60	56	89	16
Aug. 1	57	14	48	90	33	60	56	88	15
2	59	16	50	91	34	59	55	87	13
3	61	18	52	92	34	59	55	86	12
4	64	20	54	93	35	58	54	85	11
5	66	22	56	94	35	57	54	84	10
6	68	24	58	95	35	56	53	83	09
7	70	25	00	96	36	56	53	82	07
8	72	27	02	96	36	55	52	81	06
9	74	29	04	97	36	55	52	80	05
10	77	31	07	98	37	54	51	79	03
11	79	33	09	99	37	54	51	78	02
12	82	35	12	+1,00	38	54	51	78	00
13	84	38	14	01	38	54	51	77	+1,99
14	87	40	16	02	39	53	50	76	97
15	89	42	18	03	39	53	50	75	96
16	92	44	21	04	40	53	50	74	94
17	95	46	23	05	41	53	49	73	93
18	97	48	26	06	42	53	49	72	91
19	+2,00	51	28	07	43	53	49	71	90
20	03	53	31	08	44	53	48	71	88
21	06	55	33	10	45	53	47	70	87
22	08	57	36	11	46	53	46	69	85
23	11	59	38	12	47	53	45	69	84
24	14	62	+2,00	13	48	52	43	68	83
25	16	64	03	14	49	52	42	67	82
26	19	66	05	16	50	52	41	66	81
27	22	68	08	17	51	52	40	65	79
28	25	70	10	18	52	52	40	64	78
29	28	72	13	19	53	52	41	63	77
30	31	75	16	21	54	52	42	62	76
31	34	77	19	22	56	53	43	60	75

	α Lyræ.	α Cor. Bor	α Serpent.	Antares	α Hercules	α Ophiuchi.	α Lyræ	γ Aquilæ.	α Aquilæ.
July 1	+2,73	+2,85	+2,89	+3,57	+3,08	+3,19	+2,95	+3,10	+3,19
2	73	85	89	57	08	18	95	11	14
3	72	84	88	57	08	18	96	12	16
4	71	83	88	57	08	18	96	13	18
5	71	82	87	57	08	18	97	15	19
6	70	81	87	57	08	18	97	16	21
7	70	81	86	57	08	14	98	17	22
8	69	80	86	56	08	14	98	19	24
9	69	79	85	56	08	14	99	20	25
10	68	78	85	56	08	14	99	21	27
11	67	77	84	56	08	14	99	23	28
12	66	76	84	56	08	14	99	24	29
13	65	75	83	55	08	14	99	25	30
14	64	74	82	55	08	14	99	26	31
15	63	73	81	54	07	14	99	27	33
16	62	72	80	54	07	14	99	28	34
17	61	70	80	53	07	13	99	29	35
18	60	69	79	53	06	13	99	30	36
19	59	68	78	52	06	13	99	31	37
20	58	67	77	51	05	13	98	31	38
21	57	65	76	50	04	12	98	32	38
22	56	64	75	50	04	12	97	32	39
23	55	62	74	49	03	12	97	33	39
24	54	61	73	49	02	12	96	33	40
25	53	60	72	48	01	11	95	34	41
26	52	59	71	47	01	11	95	34	41
27	51	57	70	46	00	10	94	35	42
28	50	56	69	45	00	10	94	35	43
29	49	54	68	44	+2,99	09	93	36	44
30	48	53	67	43	98	09	93	36	45
31	47	51	66	42	97	08	92	37	45
Aug. 1	46	50	65	41	97	07	91	37	46
2	45	49	64	40	96	06	90	38	46
3	44	48	63	39	95	05	89	38	46
4	43	46	62	38	94	04	88	39	46
5	42	45	61	37	93	03	87	39	46
6	40	43	59	35	92	02	86	40	47
7	39	42	58	34	91	01	85	40	47
8	38	40	57	33	90	00	84	40	47
9	37	38	56	32	89	+2,99	83	40	47
10	35	36	54	30	87	98	81	40	46
11	34	35	53	29	86	96	80	40	46
12	32	33	51	27	84	95	78	39	46
13	31	31	50	26	83	94	77	39	46
14	30	29	49	25	82	93	76	39	45
15	28	27	48	24	81	92	75	38	45
16	27	26	47	23	80	91	74	38	45
17	26	24	45	21	78	90	72	38	44
18	25	23	44	20	77	89	71	37	44
19	24	21	43	19	76	88	69	37	43
20	22	19	42	18	74	86	67	37	43
21	21	17	41	17	73	85	66	36	43
22	20	15	39	15	71	83	64	36	42
23	19	14	38	14	70	82	62	36	42
24	17	12	36	12	68	80	60	35	41
25	16	10	35	11	67	79	58	35	41
26	15	08	33	09	65	77	57	34	40
27	13	06	32	08	64	76	55	34	40
28	12	04	30	06	62	74	53	33	39
29	11	02	29	05	60	73	51	32	38
30	10	00	27	04	59	72	49	31	37
31	09	+1,99	26	03	57	70	48	30	37

	β Aquilæ.	α Capricor.	α Cygni.	α Aquarii	Fomalhaut	α Pegasi	α Androm.	Polaris H M. S
July 1	+3,14	+3,47	+2,59	+3,85	+3,18	+2,36	+1,91	0.57.3,11
2	15	49	61	87	21	39	94	3,79
3	17	51	63	90	25	42	98	4,46
4	18	52	65	92	28	45	+2,01	5,13
5	20	54	67	95	31	48	05	5,85
6	21	56	69	97	34	51	08	6,60
7	23	58	71	+3,00	37	54	12	7,40
8	24	60	73	02	41	57	16	8,23
9	26	62	75	05	44	60	19	9,08
10	27	63	77	07	47	63	22	9,91
11	29	65	79	09	51	66	26	10,74
12	30	66	80	12	55	69	29	11,52
13	32	68	82	14	58	71	32	12,28
14	33	69	84	17	61	74	35	13,07
15	35	71	85	19	65	77	39	13,84
16	36	73	87	22	68	79	42	14 29
17	37	74	88	24	71	82	45	14,94
18	38	76	89	27	75	84	48	15,62
19	39	77	90	29	78	87	51	16,31
20	40	78	91	31	80	89	53	17 04
21	41	79	92	33	[illegible]	91	57	17,81
22	42	80	92	34	85	91	60	18 30
23	43	81	93	36	87	90	63	19,11
24	43	82	94	38	89	0	66	20,20
25	44	83	95	40	91	+3,00	00	20 93
26	44	84	96	42	94	02	[illegible]	21.71
27	45	85	96	44	96	05	[illegible]	22,40
28	45	86	97	46	98	07	[illegible]	23.04
29	46	87	98	48	+4,00	09	90	23,67
30	46	88	98	49	02	12	93	24,26
31	47	89	99	51	05	14	96	24,84
Aug. 1	47	90	99	53	07	16	99	25,46
2	48	90	+3,00	55	09	18	02	26,10
3	48	91	00	56	11	21	04	26,79
4	49	92	01	58	13	23	07	27,50
5	49	92	01	59	15	25	+3,00	28,23
6	50	93	01	61	17	27	03	28,96
7	50	93	02	62	19	29	05	29,68
8	50	91	02	64	21	31	08	30,38
9	50	94	02	65	23	33	10	31,02
10	50	94	01	66	24	35	12	31,62
11	49	94	01	67	26	37	15	32,16
12	49	95	01	68	28	38	17	32,69
13	49	95	01	69	30	40	20	33,20
14	49	95	00	70	32	42	22	33,71
15	49	95	00	71	34	43	24	34,24
16	48	96	00	72	36	45	27	34,81
17	48	96	+2,99	73	37	46	29	35,42
18	48	96	99	74	39	48	31	36,07
19	48	96	98	75	40	50	33	36,71
20	48	96	98	76	42	51	35	37,33
21	47	96	98	77	43	52	37	37,95
22	47	95	97	77	45	53	39	38,54
23	46	95	96	78	46	54	41	39,09
24	45	95	95	79	47	56	43	39,56
25	45	94	94	79	48	57	45	40,00
26	44	94	94	80	50	58	47	40,41
27	44	93	93	80	51	59	49	40,83
28	43	93	92	81	52	60	51	41,26
29	42	92	91	81	53	61	52	41,68
30	41	92	90	81	54	62	54	42,14
31	41	91	89	82	54	63	56	42,67

	γ Pegas.	α Arietis	α Cet.	Algebaran	Capella.	Regul.	β Tauri.	α Orionis	Sirius.
	′ ″	″	″	″	″	″	″	″	″
Sep. 1	+ 3,61	+ 3,43	+ 3,10	+ 2,83	+ 3,30	+ 2,96	+ 2,85	+ 2,32	+ 1,83
2	62	45	13	87	34	28	88	35	56
3	65	48	16	91	39	31	91	38	59
4	67	50	19	94	43	34	95	41	62
5	68	53	22	97	47	37	98	44	65
6	70	55	24	+ 3,00	52	40	+ 3,02	47	68
7	71	58	27	03	56	43	06	50	71
8	72	60	30	06	61	46	09	53	74
9	73	63	32	09	65	49	13	56	76
10	74	65	34	12	70	52	16	59	79
11	75	68	36	15	74	55	20	62	82
12	76	71	38	18	79	58	23	65	85
13	77	73	41	21	83	61	26	68	88
14	78	75	43	24	87	64	30	71	91
15	79	78	45	27	92	66	33	74	94
16	80	80	47	30	96	69	37	77	96
17	81	82	49	33	+ 4,00	72	40	80	99
18	82	84	1	36	05	75	44	83	+ 2,02
19	83	86	54	39	09	78	47	86	05
20	84	88	56	42	14	80	51	89	07
21	85	90	59	45	18	83	54	92	10
22	86	92	61	48	22	86	57	95	13
23	87	94	64	51	27	89	61	98	16
24	88	97	66	54	31	92	64	+ 3,01	19
25	88	99	69	57	35	95	68	04	22
26	89	1 01	71	60	40	98	71	07	25
27	90	03	74	63	44	+ 3,00	75	10	28
28	91	05	76	66	48	04	78	13	31
29	92	07	79	69	52	07	81	16	34
30	92	09	81	72	56	10	85	19	37
Oct. 1	93	11	83	75	60	12	88	23	40
2	94	12	84	77	64	15	91	25	42
3	95	14	86	80	68	18	94	28	45
4	95	15	88	83	72	20	97	31	48
5	95	16	90	86	76	23	+ 4,01	34	51
6	95	18	92	88	80	25	04	37	54
7	95	19	94	91	84	28	07	40	57
8	95	20	96	94	88	31	11	43	60
9	95	22	97	96	92	34	14	46	63
10	95	23	99	99	96	36	17	49	66
11	95	25	+ 4,00	+ 4,01	99	39	21	52	69
12	95	26	03	04	+ 5,03	42	24	55	72
13	95	28	04	07	07	45	27	59	75
14	95	29	05	09	11	47	31	61	78
15	95	31	07	12	15	50	34	64	81
16	95	32	08	15	19	52	37	67	84
17	95	33	10	18	23	55	40	70	87
18	95	34	12	20	27	57	43	73	90
19	95	35	14	23	31	60	46	75	93
20	95	36	16	26	34	62	49	78	96
21	95	37	18	28	38	65	52	81	99
22	95	38	19	30	41	67	55	83	+ 3,01
23	92	39	20	32	45	69	58	86	04
24	92	39	22	34	48	72	61	89	07
25	92	40	23	37	52	74	64	92	10
26	91	41	25	39	55	77	67	95	13
27	91	42	26	41	59	79	70	98	16
28	91	43	28	44	62	82	73	+ 4,01	19
29	90	43	29	46	66	84	76	03	21
30	90.	44	31	48	69	87	79	06	24
31	90	45	32	51	73	89	82	08	27

		Castor.	Procyon	Pollux.	α Hydræ.	Regulus.	β Leonis	β Virgin	Spica Vir	Arcturus.
		"	"	"	"	"	"	"	"	"
Sep.	1	+ 2,37	+ 1,80	+ 2,22	+ 1,84	+ 1,57	+ 1,53	+ 1,44	+ 1,58	+ 1,78
	2	40	82	24	85	59	53	45	57	73
	3	43	85	27	27	60	53	46	55	72
	4	46	88	30	28	61	54	47	53	71
	5	49	90	33	30	63	54	49	52	70
	6	53	93	36	32	64	54	50	51	69
	7	56	95	39	34	66	55	51	50	68
	8	59	98	42	36	67	55	52	49	67
	9	62	+ 2,01	45	38	69	56	53	50	66
	10	65	04	48	39	70	56	54	50	65
	11	66	06	51	41	72	57	55	51	64
	12	71	09	54	43	73	57	55	51	63
	13	75	12	57	45	75	58	55	52	61
	14	78	14	60	47	76	58	56	52	60
	15	81	17	63	49	78	59	56	53	59
	16	84	19	66	51	79	59	57	53	58
	17	87	22	70	53	81	60	57	54	57
	18	91	25	73	55	83	61	58	54	56
	19	94	27	76	57	85	62	59	54	55
	20	97	30	79	59	87	63	59	54	55
	21	+ 3,01	33	82	61	89	64	60	54	54
	22	04	36	85	63	91	64	61	54	53
	23	08	39	89	65	93	65	62	54	52
	24	11	42	92	67	95	66	63	54	51
	25	14	45	95	70	97	67	63	54	51
	26	18	47	98	72	99	68	64	54	50
	27	21	50	+ 3,01	74	+ 2,01	69	65	54	49
	28	25	53	04	77	03	70	66	54	49
	29	28	56	08	79	05	71	67	54	48
	30	32	58	11	81	07	73	68	54	48
Oct.	1	36	61	15	84	09	74	70	54	48
	2	39	64	18	86	11	75	71	54	48
	3	43	67	22	89	13	76	73	55	47
	4	46	70	25	91	15	77	74	55	47
	5	50	73	29	93	17	79	75	55	47
	6	54	76	32	96	19	80	77	55	47
	7	57	79	35	98	21	81	78	56	46
	8	61	82	38	+ 2,00	23	82	80	56	46
	9	64	85	42	03	25	83	81	57	46
	10	68	86	45	05	28	84	82	58	46
	11	71	91	49	08	31	85	84	58	46
	12	74	94	52	10	33	86	86	59	46
	13	78	97	56	13	36	87	88	60	47
	14	82	+ 3,00	59	15	39	88	89	61	47
	15	86	04	63	18	42	89	91	62	47
	16	90	07	66	20	44	90	93	63	47
	17	94	10	70	22	47	92	95	64	47
	18	98	13	73	26	50	+ 2,00			47
	19	+ 4,01	16	77	29	52	02	99	66	48
	20	05	19	80	32	55	04	+ 2,01	67	48
	21	08	22	84	35	58	06	03	68	49
	22	12	25	87	38	61	07	05	69	49
	23	16	28	91	41	64	09	07	71	49
	24	19	31	94	44	67	11	09	72	50
	25	23	35	98	47	70	13	11	74	50
	26	27	38	+ 4,01	50	73	15	13	75	51
	27	31	41	05	53	76	17	15	76	51
	28	34	44	08	56	79	19	17	77	52
	29	38	47	12	59	82	21	20	79	53
	30	41	50	15	62	85	23	22	80	54
	31	45	53	19	65	88	25	24	82	55

	α Libræ.	α Cor. Bor.	α Serpent.	Arcturus.	Rasalhague.	α Ophiuchi.	α Lyræ.	γ Aquilæ.	α Aquilæ.
Sep. 1	+2,08	+1,97	+1,85	+2,00	+1,86	+2,09	+2,44	+1,99	+2,[illegible]
2	07	95	84	+1,99	85	07	42	98	3[illegible]
3	06	93	82	97	83	06	40	97	3[illegible]
4	05	91	81	95	81	04	38	95	2[illegible]
5	04	90	79	93	80	02	35	94	2[illegible]
6	03	88	78	92	78	02	33	93	3[illegible]
7	02	86	76	91	77	00	31	92	3[illegible]
8	01	84	75	89	75	2,00	29	91	2[illegible]
9	00	82	73	88	73	1,98	27	90	2[illegible]
10	+1,99	81	72	86	72	97	25	88	2[illegible]
11	98	79	70	85	70	95	23	87	2[illegible]
12	97	78	69	83	68	93	21	86	2[illegible]
13	96	76	67	81	66	91	18	84	2[illegible]
14	95	74	66	80	64	89	16	83	2[illegible]
15	94	73	64	79	63	88	14	82	2[illegible]
16	93	71	63	78	61	86	11	80	1[illegible]
17	92	69	61	76	59	84	09	09	1[illegible]
18	91	67	60	74	57	82	07	08	1[illegible]
19	90	66	+1,99	73	55	81	04	07	1[illegible]
20	89	64	98	71	54	79	02	05	1[illegible]
21	88	63	97	70	52	78	+1,99	04	1[illegible]
22	88	62	96	68	50	76	97	02	1[illegible]
23	87	60	95	67	48	74	94	00	1[illegible]
24	86	59	94	66	46	72	92	+2,99	0[illegible]
25	85	57	93	64	45	71	90	97	0[illegible]
26	85	56	91	63	43	69	88	96	0[illegible]
27	84	54	90	62	41	67	86	94	0[illegible]
28	83	53	89	61	39	66	84	93	0[illegible]
29	83	51	88	59	38	64	81	92	0[illegible]
30	82	50	87	58	36	62	79	90	0[illegible]
Oct. 1	82	49	86	57	35	61	76	88	+2,9[illegible]
2	81	48	85	56	34	59	74	87	9[illegible]
3	80	47	84	54	32	58	71	85	9[illegible]
4	80	46	83	53	31	56	69	83	9[illegible]
5	79	45	82	52	+1,98	55	66	81	9[illegible]
6	79	44	81	50	97	53	64	80	9[illegible]
7	78	43	80	49	95	52	61	78	8[illegible]
8	78	[illegible]	[illegible]	48	94	50	59	76	8[illegible]
9	78	[illegible]	[illegible]	47	92	49	56	75	8[illegible]
10	78	[illegible]	78	46	91	48	54	73	8[illegible]
11	78	39	77	45	89	46	51	72	8[illegible]
12	78	38	77	44	88	45	49	70	8[illegible]
13	78	37	76	43	87	+1,99	47	69	7[illegible]
14	78	36	75	42	86	98	44	68	7[illegible]
15	77	35	74	41	84	96	42	66	7[illegible]
16	77	34	74	40	83	95	39	65	7[illegible]
17	77	33	73	39	81	93	37	63	7[illegible]
18	[illegible]	33	73	39	80	92	35	61	7[illegible]
19	77	32	72	38	79	91	33	60	7[illegible]
20	78	32	72	38	78	90	31	58	6[illegible]
21	78	31	71	37	77	89	29	56	6[illegible]
22	78	31	71	37	76	88	27	54	6[illegible]
23	78	30	71	36	75	87	25	52	6[illegible]
24	78	30	70	36	74	86	23	50	6[illegible]
25	79	29	70	35	73	85	21	49	5[illegible]
26	79	29	70	35	72	84	19	47	5[illegible]
27	80	28	70	34	71	83	17	46	5[illegible]
28	81	28	70	34	70	82	15	45	5[illegible]
29	82	28	70	34	69	81	13	43	5[illegible]
30	82	28	70	34	69	80	11	42	5[illegible]
31	83	28	70	34	68	79	09	40	5[illegible]

	β Aquilæ	α Capricorni	α Cygni	α Aquarii	Fomalhaut	α Pegasi	α Andromedæ	Polaris
	"	"	"	"	"	"	"	H M. S
Sep. 1	+ 3,40	+ 3,90	+ 2,88	+ 2,82	+ 4,55	+ 3,64	+ 3,58	0.57.43 18
2	39	90	87	83	56	65	59	43,73
3	39	89	86	83	57	66	61	44 24
4	38	88	85	83	58	67	62	44,72
5	37	88	84	84	59	68	64	45,18
6	36	87	83	84	60	69	65	45,58
7	35	87	81	85	61	70	67	45,94
8	34	87	79	85	61	70	68	46 24
9	33	86	78	85	62	71	70	46,52
10	32	85	76	85	62	71	71	46,91
11	31	84	74	84	62	71	73	47,12
12	30	83	73	84	62	71	73	47,46
13	28	82	71	84	63	74	74	47,80
14	27	81	69	84	63	73	75	48,19
15	26	80	67	83	63	74	76	48,00
16	24	79	66	83	64	73	77	49,00
17	23	78	64	83	64	71	78	49,10
18	22	77	62	83	64	73	79	49 71
19	21	76	60	82	64	73	80	50 03
20	20	74	58	82	64	73	81	0 7
21	19	73	56	82	64	73	81	0,15
22	17	72	54	81	64	73	82	50 00
23	15	70	52	81	64	73	83	50 79
24	14	69	49	80	64	73	83	0 4
25	12	68	47	80	64	73	4	1 10
26	11	66	45	80	64	73	85	1 0
27	10	65	43	79	64	73	85	51, -
28	08	64	41	79	63	73	86	51 70
29	07	62	38	78	63	72	86	51,01
30	05	61	36	77	63	7	87	52 -7
Oct. 1	04	60	34	76	62	71	87	52 50
2	02	59	32	76	62	73	87	52 71
3	00	57	30	75	61	71	87	52 81
4	+ 2,99	56	27	74	61	70	87	52 0-
5	97	54	25	73	60	71	88	52,90
6	96	53	22	72	60	69	88	52 98
7	94	51	20	71	59	69	88	53 00
8	92	50	18	70	58	68	88	53 05
9	91	48	15	69	57	68	89	52,99
10	89	47	13	68	57	68	90	53,03
11	88	46	1	67	56	67	89	53 12
12	86	45	09	67	56	67	89	53,25
13	84	43	07	66	55	66	90	53,34
14	83	42	05	65	54	66	89	53,41
15	81	40	03	64	54	65	90	53,48
16	80	39	00	63	53	65	89	53 51
17	78	37	+ 1,98	62	52	64	89	53,51
18	76	36	95	61	51	63	89	53,40
19	75	35	93	60	50	63	88	53,26
20	74	33	90	59	49	62	88	53,11
21	73	32	88	58	48	61	87	52,97
22	71	30	85	57	47	60	87	52,93
23	69	28	82	55	45	60	86	52,68
24	68	27	80	54	44	59	86	52,57
25	66	25	77	53	43	58	85	52,51
26	65	24	75	51	42	58	85	52,46
27	63	22	72	50	41	57	84	52,42
28	62	21	69	49	40	56	84	52,33
29	60	19	67	48	39	55	83	52,22
30	59	18	64	47	38	54	83	52,07
31	57	16	62	46	37	53	82	51,87

	γ Pegas.	α Arietis	δ Ceti	Aldebaran	Capella.	Rigel	β Tauri	α Orionis	Sirius.
	"	"	"	"	"	"	"	"	"
Nov. 1	+3,90	+4,15	+4,33	+4,53	+5,76	+3,92	+4,85	+4,11	+3,39
2	89	46	34	55	80	94	88	14	38
3	89	47	35	57	83	96	91	17	35
4	88	48	36	59	87	99	94	20	38
5	88	49	37	61	90	+4,01	96	23	41
6	87	50	38	63	94	03	99	25	44
7	86	50	39	65	97	05	+5,01	27	47
8	86	51	40	67	+6,00	07	04	30	49
9	85	51	40	69	02	09	06	32	52
10	85	51	41	71	05	11	09	35	55
11	84	51	42	73	08	13	11	37	57
12	84	52	43	75	11	15	14	39	60
13	83	52	44	77	14	17	16	42	63
14	83	52	44	79	17	19	19	44	66
15	82	53	45	81	20	21	22	47	68
16	81	53	46	83	23	23	25	49	71
17	80	53	46	85	25	25	27	52	73
18	79	54	47	86	28	27	29	54	76
19	78	54	48	88	31	29	31	56	78
20	77	54	48	89	34	31	33	58	81
21	77	54	49	91	37	32	35	60	83
22	76	55	50	93	39	34	38	63	85
23	75	55	50	95	41	36	40	65	88
24	74	55	51	96	44	38	42	68	90
25	73	55	52	97	46	39	44	70	93
26	73	55	52	99	49	41	46	72	95
27	72	55	53	+5,00	51	42	48	74	98
28	71	54	53	01	52	43	49	75	+4,00
29	70	54	53	02	54	45	51	77	02
30	69	53	53	03	56	46	52	79	04
Dec 1	68	53	53	04	58	47	54	80	06
2	67	52	51	05	60	48	56	82	08
3	66	52	54	06	62	49	58	84	10
4	65	51	54	07	64	51	60	86	12
5	64	51	54	08	65	52	61	87	14
6	63	50	54	09	67	54	63	89	16
7	62	50	54	10	68	55	64	90	18
8	61	50	54	10	70	56	66	92	20
9	60	49	54	11	71	57	68	93	22
10	59	49	54	12	73	58	69	95	24
11	58	48	53	13	75	59	70	96	26
12	56	48	53	14	77	60	72	98	28
13	55	47	53	15	79	61	73	+5,00	30
14	54	47	53	16	81	62	74	01	32
15	53	46	53	16	82	63	76	03	34
16	52	46	53	17	83	64	77	04	36
17	51	45	53	17	83	65	78	05	37
18	50	44	52	18	84	65	79	06	38
19	49	43	52	18	84	66	80	07	40
20	48	42	51	19	85	66	81	08	42
21	47	42	51	19	85	67	82	09	43
22	46	41	50	20	86	68	83	10	45
23	45	40	50	20	86	69	84	11	46
24	44	39	49	20	87	70	84	12	47
25	43	39	49	21	87	70	85	13	49
26	42	38	48	21	88	71	86	14	50
27	41	37	47	21	90	71	87	15	51
28	39	36	47	21	92	72	87	16	52
29	37	36	46	21	94	72	88	16	54
30	36	35	46	21	96	73	88	17	55
31	35	34	45	21	98	73	89	18	56

	Castor	Procyon	Pollux	α Hydræ	Regulus	β Leonis	β Virginis	Spica Virg	Arcturus
	″	″	″	″	″	″	″	″	″
Nov. 1	+4,48	+3,56	+4,22	+3,68	+3,91	+3,30	+3,26	+1,83	+1,56
2	52	59	26	71	94	38	29	85	57
3	56	62	30	75	97	35	31	87	58
4	60	65	33	78	+4,00	37	34	89	59
5	63	68	37	81	03	40	36	90	60
6	67	71	40	84	06	43	39	02	61
7	70	74	44	87	09	46	42	04	62
8	74	77	48	90	12	48	44	06	64
9	77	80	51	93	15	51	47	93	65
10	81	83	55	96	18	54	50	+2,00	67
11	84	86	58	99	21	57	52	02	68
12	88	89	62	+3,03	25	60	55	04	69
13	91	92	65	06	28	63	58	06	71
14	95	95	69	09	31	66	61	09	72
15	98	98	73	13	34	68	64	11	74
16	+5,02	+4,01	76	16	37	71	67	13	75
17	05	04	80	19	40	74	70	15	77
18	09	07	83	22	44	77	73	18	79
19	12	10	87	25	47	80	76	20	81
20	16	13	90	29	51	83	79	23	83
21	19	16	93	32	53	86	82	25	85
22	23	19	96	36	58	89	85	28	87
23	26	22	+5,00	39	62	92	88	31	89
24	30	25	03	43	65	95	91	33	91
25	34	28	06	46	69	98	94	36	93
26	37	31	09	49	72	+4,01	97	38	95
27	40	34	12	52	75	04	+4,00	41	98
28	43	37	15	55	79	07	03	43	+2,00
29	46	39	19	58	82	1	06	46	03
30	49	42	21	61	85	13	09	49	05
Dec. 1	52	44	24	64	89	16	12	52	07
2	55	47	27	67	92	20	16	55	09
3	58	49	30	70	95	23	19	58	11
4	61	52	33	73	99	26	22	61	14
5	64	54	36	76	+4,02	30	25	63	16
6	67	57	39	79	05	33	28	66	18
7	70	59	42	82	09	36	31	69	21
8	73	62	45	85	12	40	35	72	23
9	76	64	48	88	15	43	38	75	26
10	79	67	51	91	19	47	42	78	29
11	82	69	54	94	22	50	45	81	31
12	85	72	57	97	25	54	49	84	34
13	87	74	60	+4,00	29	57	52	87	37
14	90	76	63	03	32	61	55	90	40
15	93	79	65	06	35	65	59	93	43
16	95	81	68	09	38	68	62	96	46
17	97	83	70	12	41	71	65	99	49
18	99	85	72	14	44	75	68	+3,02	52
19	+6,02	87	75	17	47	78	72	05	55
20	04	89	77	20	51	82	75	08	58
21	07	91	80	22	54	85	78	11	60
22	09	93	82	25	57	89	81	15	63
23	11	95	84	28	60	92	84	18	66
24	14	97	87	31	63	95	88	22	69
25	16	99	89	34	66	98	91	25	72
26	18	+5,01	91	37	69	+4,02	94	28	75
27	20	03	93	40	72	06	97	31	78
28	22	05	95	42	75	09	+4,01	35	81
29	24	06	97	45	78	13	05	38	85
30	26	08	99	47	81	16	08	42	88
31	28	10	+6,01	50	84	19	11	45	91

	α Libræ	α Cor. Bor.	α Serpent.	Antares	α Hercules	α Ophiuch	α Lyræ	γ Aquilæ	α Aquilæ
	′	″	″	″	″	″	″	″	″
Nov. 1	+1,84	+1,29	+1,71	+2,34	+1,67	+1,78	+1,07	+2,89	+2,49
2	85	29	71	34	66	77	05	87	48
3	86	29	71	34	65	76	03	86	46
4	87	29	71	34	65	75	01	35	45
5	88	29	71	34	64	74	+0,99	34	44
6	89	29	71	34	63	73	97	33	43
7	90	30	72	34	63	73	95	31	42
8	92	30	72	34	62	72	94	30	41
9	93	31	73	34	62	72	92	29	40
10	94	31	73	34	62	71	90	28	39
11	95	32	74	34	62	71	89	27	38
12	97	33	75	35	61	70	87	26	37
13	98	33	75	35	61	70	85	25	36
14	99	34	76	36	61	69	83	24	35
15	+2,01	34	77	36	60	69	82	23	34
16	02	35	78	37	60	69	80	22	33
17	04	36	79	38	60	69	79	21	32
18	05	37	80	39	60	69	78	20	30
19	07	38	82	39	61	69	76	19	29
20	09	39	83	40	61	69	75	18	28
21	11	40	84	41	61	69	74	17	28
22	13	42	85	42	61	69	73	16	27
23	15	43	86	43	61	69	72	15	26
24	17	44	88	44	62	69	71	14	25
25	19	45	90	45	62	69	70	13	24
26	21	47	91	46	62	69	69	12	23
27	23	48	92	47	62	69	68	11	23
28	25	50	94	49	63	70	67	11	22
29	27	51	95	50	63	70	67	10	22
30	29	53	97	51	64	70	66	09	21
Dec. 1	31	54	98	52	64	70	65	09	20
2	34	56	99	54	65	71	64	08	19
3	36	57	+2,01	55	65	71	63	07	18
4	39	59	02	56	66	72	63	06	18
5	41	60	04	58	67	73	62	06	17
6	43	62	06	60	68	73	61	05	17
7	45	64	08	62	69	74	61	05	17
8	48	66	10	64	70	75	61	05	16
9	50	68	12	66	71	75	61	05	16
10	53	70	14	68	72	76	61	05	16
11	56	72	16	70	73	77	61	05	16
12	59	74	18	72	75	78	61	04	15
13	61	76	20	74	76	79	61	04	15
14	64	78	22	76	77	80	60	04	15
15	67	80	24	78	78	81	60	03	14
16	69	83	26	80	80	82	60	03	14
17	72	85	28	82	81	83	60	03	14
18	75	88	31	84	83	84	60	03	14
19	78	90	33	87	84	86	60	03	14
20	81	93	35	89	86	87	60	03	14
21	84	95	37	91	87	89	60	03	14
22	87	97	40	94	89	90	61	04	15
23	90	+2,00	42	96	90	92	61	04	15
24	93	02	44	99	92	93	61	04	15
25	96	05	47	+3,01	93	95	62	05	15
26	99	07	49	03	95	96	62	05	15
27	+3,02	10	52	06	97	98	62	05	15
28	05	13	54	08	99	99	63	06	16
29	08	15	57	11	+2,01	+2,01	63	06	16
30	11	18	59	13	02	02	64	07	17
31	14	21	62	16	05	04	64	07	17

	β Aquilæ.	ε Capricor.	α Cygni.	α Aquarii.	Fomalhaut	α Pegasi.	α Androm.	Polaris. H. M. S.
	"	"	"	"	"	"	"	
Nov. 1	+2,56	+3,15	+1,59	+3,45	+4,36	+3,52	+3,82	0.57.51,63
2	55	14	56	43	35	51	81	51,31
3	54	13	54	42	34	50	80	50,98
4	52	12	51	41	33	49	80	50,66
5	51	10	49	39	32	48	79	50,37
6	49	09	46	38	31	47	79	50,11
7	48	08	44	37	30	46	77	49,84
8	47	07	42	36	28	45	76	49,62
9	46	06	40	35	27	43	75	49,41
10	45	05	37	34	25	42	75	49 21
11	44	04	35	33	24	41	74	49,00
12	43	03	33	31	23	40	73	48,60
13	42	02	30	30	21	39	73	48,36
14	41	01	28	29	20	38	72	47,98
15	40	00	25	28	19	37	71	47,58
16	39	+2,00	23	27	18	36	70	47,13
17	38	98	21	26	17	35	69	46,64
18	37	97	19	25	15	34	68	46,17
19	36	96	17	23	14	33	67	45,72
20	35	95	15	22	12	32	66	45,21
21	35	95	13	21	11	31	65	44,94
22	34	94	11	20	10	30	64	44,56
23	33	93	09	19	08	29	63	44,20
24	32	92	08	18	07	27	62	43,84
25	31	91	04	17	05	26	61	43,46
26	30	90	02	16	04	25	60	43,03
27	29	89	00	15	03	24	59	42,54
28	28	88	+0,99	14	01	22	57	41,99
29	28	87	96	12	00	21	56	41,41
30	27	86	94	11	+3,98	20	55	40,81
Dec. 1	26	86	92	10	97	19	54	40,22
2	25	85	89	09	95	17	53	39 60
3	24	84	88	08	94	16	52	39,01
4	24	83	86	06	92	15	51	38,47
5	24	82	84	05	91	13	49	37,97
6	23	82	82	04	90	12	48	37,40
7	23	82	80	03	89	11	47	36,90
8	22	81	79	02	88	10	46	36,40
9	22	81	77	01	87	09	44	35,95
10	22	81	76	00	86	08	43	35,48
11	22	81	74	+2,00	85	07	42	34,77
12	21	80	72	00	84	06	41	34,09
13	21	80	71	00	83	05	40	33,30
14	21	80	69	07	81	03	39	32,67
15	20	79	68	06	80	02	37	31,95
16	20	79	67	06	79	01	36	31,27
17	20	79	66	05	78	00	35	30,58
18	20	79	65	04	76	+2,00	33	29,91
19	20	79	64	04	75	00	32	29,34
20	20	79	63	03	74	97	30	28,76
21	20	79	61	02	73	96	29	28,18
22	21	79	60	01	72	94	28	27,57
23	21	79	59	00	71	93	27	26,94
24	21	79	58	00	70	92	26	26,26
25	21	79	57	00	68	91	24	25,55
26	21	79	56	00	67	90	23	24,79
27	21	79	55	00	66	89	22	24,01
28	21	79	54	00	65	88	20	23,20
29	22	80	53	00	64	87	19	22,42
30	22	80	52	00	63	86	17	21,66
31	22	80	51	00	62	85	16	20 94

Art. XVI. *Miscellaneous Intelligence.*

I. Mechanical Science.

§ 1. Agriculture, Optics, Astronomy, &c.

1. *Apparatus for shewing the double Refraction of Minerals.*—In the *Journal of Science*. Vol. X., p. 168, two methods of finding the double refraction of minerals have been quoted from M. Soret, in the *Journal de Physique*, Tom. XC., p. 353; and lest the public should be led by that notice to ascribe the invention of them to him, M. Soret has thought it of sufficient importance to declare, in a letter to the editor, "That the apparatus are not of his invention, but BELONG to M. Biot."

M. Soret must certainly have misunderstood M. Biot, for he has undoubtedly no claim whatever to *the invention of these two kinds of apparatus*. Dr. Brewster was the first person who employed the apparatus of two plates of a singly refracting crystal, placed transversely: the crystal which he used was *Agate*. A long time afterwards M. Biot discovered an analogous property in the *Tourmaline*, and substituted it in place of the agate, but the apparatus did not on this account become of his invention. Dr. Brewster was also the first who used Agate Microscopes, consisting chiefly of thin plates cemented on plano-convex lenses, and he has since constructed similar apparatus by converting calcareous spar and artificial salts into singly-refracting plates, (See *Philosophical Transactions*, 1819, p. 149,) and has also repeatedly used analogous apparatus, consisting of transverse parcels of films of glass blown to extreme thinness, and films of mica arranged in a particular manner. The merit which belongs to M. Biot is that of having discovered that *Tourmaline* has the singly refracting and polarising property of *Agate*.

M. Soret must have ascribed the *second* apparatus to M. Biot, solely because he had exhibited to him the experiment. It belongs exclusively to Dr. Brewster, who shewed the experiments to Major Petersen in 1816 and 1817, and to Count Breunner, and Professor Mohs in 1818. (See the *Philosophical Transactions*, 1819, p. 11., and the *Journal de Physique*, Mars, 1820, Tom. XC., p. 177, the *same volume* in which M. Soret ascribes the invention to M. Biot.)

2. *Diving Machine.*—A new diving machine, called a Dolphin, has been invented by M. F. Farkas, an Hungarian. The continental papers have described some of the advantages of the instrument, but not its construction. An experiment was made with it at Vienna in the military swimming-school at the Prater. Count Joseph Esterhazy de Galanthy, Count Fergas de Ghymes, the acting Chamberlain Nemes Slagod, and several Englishmen and persons of distinction were present. The servant of the inventor plunged with the Dolphin in twenty-four feet water, and walked upon the bottom over the whole square of the swimming-school. To prove that there could be no want of light, the inventor sent down a lantern, and when it was taken up again the light was still burning. After the man had remained one hour under water, he returned to the surface without assistance; not because he wanted air, but because all who were present were satisfied with the success of the experiment, and directed that the man might ascend.

3. *Astronomical Prize Question.*—The Astronomical Society of London have offered their gold medal and twenty guineas "For the best paper on the theory of the motions and perturbation of the satellites of Saturn. The investigation to be so conducted as to take expressly into consideration the influence of the rings and the figure of the planet as modified by the attraction of the rings on the motions of the satellites: to furnish formula adapted to the determination of the elements of their orbits and the constant co-efficients of their periodical and secular equations from observation: likewise to point out the observations best adapted to lead to a knowledge of such determination. The papers to be sent to the Society on or before February 1, 1823."

Each memoir is to bear a motto and be accompanied by a sealed paper with the same motto and the author's name. The successful paper is to be left with the society, and published as they may direct.

II. Chemical Science.

§ Chemistry, Electricity, &c.

1. *Oxides of Manganese*,—Dr. Forchhammer, in addition to his remarks on the acids of manganese, has published an account and analysis of the different oxides, the preparation and composition of which will be briefly noticed below.

The manganese was obtained free from other metals, by heating the black oxide with sulphuric acid till all excess of acid was driven off, by making a solution, and then by precipitating the copper and iron from that solution by hydro-sulphuret of ammonia, they fall down of a black colour; when the precipitate becomes grey, the solution is to be heated to the boiling point, and, if sufficient hydro-sulphuret has been added, will precipitate white with a farther addition of it. From the solution thus precipitated the carbonate is obtained, and from that the other preparations of manganese.

Another process for preparing pure manganese may be found at page 358, vol. VI. of this *Journal*.

Dr. F. obtained protoxide of manganese by heating the deutoxide very gently in a glass tube, and at the same time passing a current of hydrogen gas over it. The brown powder became of a light yellow colour, and whilst cooling, white: the cold oxide was of a beautiful light green colour; by mere exposure to the air it absorbed oxygen, and began to turn grey.

Several analyses of this oxide were made, one from among the rest gives its composition as 100 manganese + 30.24 oxygen, and Dr. F. thinks that the true quantity of oxygen, combined with 100 of manganese, is between 30.18 and 31.29.

The deutoxide of manganese is prepared by heating pure protoxide in the air, at a temperature between the boiling point of water and of mercury, it takes fire and burns slowly with a reddish light, into deutoxide. The composition of this oxide is 100 manganese with 42 04 oxygen.

When this deutoxide is boiled in dilute nitric acid a part is dissolved, and an insoluble peroxide remains. It is black, and insoluble in acids or alkalies. The latter when slightly heated with it form deutoxide and manganeseous acid, the latter being dissolved. It is a conductor of electricity. It may be formed, also, by exposing carbonate of manganese to air, at a temperature of 500° Fahr., and, washing it with weak cold muriatic acid, its composition is 63.749 manganese + 36.351 oxygen when dry, but it is when prepared as above, always a hydrate, and contains manganese 30 + oxygen, 16 + water 9.

The oxide obtained, by exposing the nitrate to moderate heat,

and which Berzelius considers as the deutoxide, is, according to Dr. F. a compound of 1 atom of peroxide = 22.323 and 1 atom of deutoxide = 77.677.—*Annals of Phil.* I., p. 50.

2. *Dissection of Crystals.*—Those specimens of sulphuret of antimony which are crystallized in large crystals, crossing each other, admirably illustrate Mr. Daniell's mode of displaying crystalline texture by dissection. On introducing such a piece of sulphuret into a portion of fused sulphuret and continuing the heat, it begins to melt down; but so far from this taking place uniformly at the surface, crystals will sometimes be left more than half an inch long projecting from it; and in other places the cavities left by fused crystals will be so large and have such perfect surfaces, that the angles they form with each other may be readily ascertained. In order to observe these effects it is only necessary to remove the half-fused piece of sulphuret from its hot bath, and allow it to cool. M. F.

3. *Solution of Lime.*—Mr. Dalton formerly shewed that lime was more soluble in cold water than in hot water, and gave a table of quantities, from which he concluded, that the quantity held in solution by water of 32° Fahr., was nearly twice that retained by water of 212°. Mr. Phillips has lately taken up the subject, and after ascertaining the accuracy of Mr. Dalton's experiments and conclusions, proceeds to experiment and remark upon the cause of the phænomenon, and considers it as resulting "from the effect which heat sometimes produces of increasing instead of diminishing the attraction of cohesion. The affinities which are brought into play, are the attraction of aggregation of the particles of lime for each other, the attraction of the lime to form a hydrate with a small portion of water, and the mutual affinity existing between that hydrate and the water of solution," and at the high temperature, Mr. Phillips thinks that the two former affinities may be heightened so as to overpower the latter.

Mr. P. found, that by heating cold saturated lime-water a crystalline deposition of hydrate of lime was thrown down, but the crystals were so minute that their form could not be ascertained.

10.000 gr. of water at 212° dissolve 7.8 gr. of lime.
10.000 gr. of water at 32° dissolve 15.2 gr. of lime.

Annals of Phil. I. p. 107.

4. *Lithia in Lepidolite.*—Professor Gmelin has detected lithia in two specimens of lepidolite; one being Swedish, and the other from Moravia. He endeavoured, without success, to form alum with this alkali and the super-sulphate of alumine.

5. *Spontaneous Combustions.*—The following case of spontaneous combustion has been described by Mr. James Gullan, of Glasgow, see *Edin. Phil. Journal,* vol. vii. p. 219. Having sold a respectable spirit-dealer a parcel of sample-bottles, I sent them to him packed in an old basket, the bottom of which was much broken; to prevent the bottles from falling through, I put across the bottom of the basket a piece of old packing-sheet, which had lain long about an oil and colour warehouse, and was besmeared with different kinds of vegetable oil. About six or eight weeks after, the gentleman informed me that my oily-cloth and basket had almost set his warehouse on fire. The basket and cloth had been thrown behind some spirit casks pretty much confined from the air, and about mid-day he was alarmed by a smell of fire. Having moved away the casks in the direction where the smoke issued, he saw the basket and cloth in a blaze. This fact may give a useful hint to persons in public works, where galipoli, rapeseed, or linseed oils are used in their manufactures; as it is an established fact (though not generally known), that these vegetable oils used in cloths, yarn, or wool, in the process of dyeing, and confined for a time from the open air, are very apt to occasion spontaneous fire.

6. *Polishing Powder from Charcoal*—Mr. J. Thomson, of Glasgow, has lately turned his attention to the property possessed by charcoal of giving a fine polish when rubbed on metals. This property is not possessed by charcoal in general, but has been found to belong only to particular pieces; no means were known by which such charcoal could be distinguished, except actual trial, nor was the cause of the superiority of some pieces over others at all understood. Mr Thomson, in consequence of information he received from Messrs. Harts that the Dutch rush used in polishing wood owed its powers to silex, was induced to suppose that charcoal made from wood growing on sandy soils would have the property required, and on trial this was found to be the case. It frequently happens that turners meet with wood which very rapidly destroys the edges of their tools. Mr. Thomson procured some of this wood, and having converted it into charcoal, tried its polishing powers. They gave great satisfaction; and hence Mr. T. recommends, that turners, cabinet-makers, &c., should lay aside such wood when they meet with it, as a source of charcoal for the copper-plate workers, &c., to whom it is of more value than to the former, and who are constantly in want of polishing charcoal-powder.

7. *On the colouring Matter of the Lobster.*—M. Lassaigne has lately examined the colouring matter of the lobster. He obtained it by separating the shell of the animal from all other

substances, and digesting it in alcohol, using the same portion to different quantities of the shell. The pieces thus treated gradually parted with their colouring matter, and were incapable of becoming red when boiled. The solution collected and evaporated afforded a red matter, having the appearance of fat. This substance is insipid and inodorous; is insoluble in water, but is easily dissolved in sulphuric acid, or concentrated alcohol. Its solution is of a scarlet colour, and does not become turbid by the addition of water, so that it is not analogous to fat. Potash, soda, or ammonia, do not alter its colour. Dilute mineral acids have no effect upon it; but, when concentrated, they destroy and change it into a dull yellow substance. Salts of tin, lead, iron and copper, do not precipitate this substance from a solution of alcohol. M. Lassaigne states that this substance is contained in a membrane which adheres strongly to the calcareous envelope when the animal is young; but that it is easily separated from those at the full growth. The membrane is very thin, and is of a violet colour in reflected light; but of a purple hue in transmitted light.—*Journal de Pharmacie*, vi. p. 174.

8. *Vegetable Alcali: Daturium.*—A substance, supposed to be a new vegetable alkali, has been obtained from the seeds of the *daturium stramonium* by M. R. Brandes, and distinguished by the name *Daturium*. It is combined in the seeds with malic acid, and is obtained in the usual way. It is nearly insoluble in water and cold alcohol, but is soluble in hot alcohol from which it precipitates on cooling in flocculi. It has been obtained with difficulty in crystals, which were quadrangular needles. It neutralizes acids, but requires to be added in large quantity. Its sulphate is crystallizable, soluble in water, efflorescent, and decomposed by fixed alkalies. Its muriate forms square plates, readily soluble in water. Its nitrate is crystalline and soluble. Its acetate is deliquescent. It acts on iodine as other alkalies do, though feebly.—*Journal de Physique*, xci. p. 144.

9. *Atropia.*—Another of these substances found by the same philosopher in the *Bella donna Atropia*, and which gives to that plant its particular properties, is *atropia*; it is white, shining, crystallizable in long needles, insipid, and little soluble in water or alcohol; it forms regular salts with the acids, and is capable of neutralizing a considerable quantity of them. Its sulphate contains

Atropia.....	38.93
Sulphuric acid..	36.52
Water	24.55
	100

When atropia and potassa are mixed and raised to a red heat, the *ashes* (solution?) mingled with muriate of iron, produces a brilliant red colour.

Hyoscyamia is extracted from the *hyoscyamus niger*, and is not easily altered even at a red heat. It crystallizes in long prisms, and when saturate with sulphuric acid or nitric acid, forms very characteristic salts.

In examining the constituent alkaline principles of narcotic plants, much care must be taken, as the venomous properties of the plants are concentrated in them. The vapour is very injurious to the eyes, and the smallest fragment placed on the tongue is extremely dangerous—*Jour. de Phys.* XCI. p. 239.

10. *Lupulin, or the active Principle of the Hop.*—Dr. A.W. Ives, of New York, has lately made experiments on the hop, which prove that its characteristic properties reside in a substance forming not more than one-sixth part of the weight of the hop, and easily separable from it. It was observed, that on removing some hops from a bag in which they had been preserved for three years, an impalpable yellow powder was left behind which, when sifted, appeared quite pure; this has been called *lupulin*, it is peculiar to the female plant, and is probably secreted by the nectaria.

From various experiments made on it, Dr. Ives inferred that lupulin contains a very subtle aroma which is yielded to water and to alcohol, and which is rapidly dissipated at a high heat; that no essential oil can be detected by distillation in any portion of the hop; that the lupulin contains an extractive matter which is soluble only in water; that it contains tannin, gallic acid, and a bitter principle which are soluble in alcohol and water; that it contains resin which is soluble in alcohol and ether, and wax which is soluble only in alkalies and boiling ether; that it contains neither mucilage, gum, nor gum resin; that the aromatic and bitter properties of the lupulin are more readily and completely imbibed by alcohol than by water, and much sooner by both when hot than when cold; that about five-eighths of the whole substance is soluble in water, alcohol, and ether, there being about three-eighths of it vegetable fibrous matter; 120 grains of lupulin contain about

Tannin	5 gr.
Extractive matter....	10
Bitter principle.....	11
Wax	12
Resin.....	36
Lignin.....	46

Hops from which all the lupulin had been separated when acted upon by water, alcohol, &c. gave a portion of extract

which; however, possessed none of the characteristic properties of the hop.

Having ascertained that the lupulin was the only important part of the hop as regarded brewing, Dr. Ives next endeavoured to ascertain the quantity afforded by a given weight of hops: 6lbs. of hops from the centre of a bag were put into a light bag, and by thrashing, rubbing, and sifting, 14 ounces of lupulin were separated. It is supposed, therefore, that dry hops would yield about a sixth part of their weight of this substance.

Two barrels of beer were then made, in which 9 oz. of lupulin were substituted for 5 lbs. (the ordinary quantity) of hops. The result confirmed every expectation. Though the quantity of lupulin was less than usually enters into the same quantity of wort, and though the weather during June was unusually warm, and therefore unfavourable to the beer, still, at the end of five weeks, it was very fine. As a further experiment,—equal quantities of the beer were exposed in open phials to the sun, and a scruple of lupulin was added to one of them; this was unchanged at the end of fifteen days; the other became mouldy and sour in ten days.

The advantages which promise to result from the discovery that lupulin may replace the white hop in brewing, are, the diminished expenses of conveyance and storage, the facility of preserving it from the air, the non-absorption of wort by the hops, and the absence of an useless nauseous extractive matter which remains in the leaves. It remains to be seen, whether practice will establish the truth of the foregoing deductions and advantages.—*Annals of Philosophy*, p. 194

11. *Analysis of Indian Corn*.—Indian corn, either alone or mixed with the flour of wheat or of rye, constitutes a considerable article in the food of the inhabitants of the United States. In consequence of the importance which thus belonged to it, Dr. John Gorham of Harvard University, Cambridge, U.S., was induced to examine it chemically, with great attention. His experiments were made upon two varieties of maize, that producing small yellow grain, and the large, flat and white kind, commonly known by the name of Virginian corn; but the results were so similar, that those only belonging to the former kind have been given.

One hundred grains powdered, when macerated and triturated with great precaution in water, gave a clear filtered solution, which, on evaporation, afforded 4 grains of greyish semi-transparent substance, disposed in lamnæ. Of this, when acted upon by alcohol, 1.75 grains were insoluble, and resembled gum; the 2.25 grains that were soluble, were separated from the alcohol by evaporation, and dissolved in

water, then being acted on by acetate of lead and sulphuretted hydrogen, .8 of a grain of extractive matter was obtained, and 1.45 grains of a saccharine matter remained.

Another portion of the mixed gummy and saccharine matter was obtained; a drop of sulphuric acid was added to a part of it and liberated acetic acid, and quick-lime being added to another part, a small quantity of ammonia was liberated. Hence it appears to contain acetate of ammonia. It also afforded a portion of phosphate of lime.

The portion unacted on by water, and left on the filter, was digested for twenty-four hours in alcohol, and the clear solution evaporated; a yellow substance was then obtained, resembling bees'-wax in appearance. It was soft, ductile, tenacious, elastic, insipid, nearly inodorous, and heavier than water. When heated, it swelled, became brown, exaled the odour of burning bread, melted with the smell of animal matter, and left a voluminous charcoal. It burnt in the flame of a lamp, but not rapidly. When distilled, no ammonia seemed formed It was insoluble in water, but soluble in alcohol, oil of turpentine, and sulphuric ether, and sparingly in mineral acids, and caustic alkalies. It was insoluble in fixed oils, but mixed with resin. The quantity obtained from 100 grains, was 3 grains.

This substance appears to differ from all known vegetable bodies, and has been called zeine by Dr. Gorham. It resembles gluten in some circumstances, but differs from it in containing no azote, in its great solubility in alcohol, and in its permanency, not undergoing any obvious change in six weeks. On the other hand, it is analogous to the resins in its solubility in alcohol, essential oils, alkalies, and partial solubility in acids It is inflammable, and probably composed of oxygen, hydrogen, and carbon. It may easily be obtained by digesting a few ounces of the meal from the yellow corn in a flask with warm alcohol, allowing it to rest for some hours, then filtering and evaporating.

After the action of alcohol on the 100 grains it was boiled in successive portions of water, a large quantity of starch was thus dissolved, leaving 14.25 grains of a substance, which, when boiled with weak sulphuric acid, was reduced to 3.75 grains. The acid solution, when concentrated, deposited 2.25 grains of what was considered albumen, and it appeared that about 8 grains of starch had also been taken up by the acid. The 3.75 grains of solid matter were then heated with potassa, and reduced to 3 grains of ligneous matter and cuticle containing a little phosphate of lime; the portion dissolved appeared to be albumen.

According to this analysis the constituents of yellow Indian corn, in the common and the dry state, will be as follow:

	Common state.	Dry state.
Water	9.0	
Starch	77.0	84.599
Zeine	3.0	3.296
Albumen	2.5	2.747
Gummy matter	1.75	1.922
Saccharine matter	1.45	1.593
Extractic matter	.8	.879
Cuticle and ligneous fibre	3.0	3.296
Phosp. carb. sul. of lime, and loss	1.5	1.648
	100.	99.980

The powder of the corn is hygrometric, and the quantity of water in it varies with the state of the atmosphere. Sometimes it would lose 12 *per cent.* on drying, at other times not more than half that quantity.

In some experiments on the colouring matter of the different coloured varieties of Indian corn, it was found to be soluble in both water and alcohol, and to become green by alcalies, and red by acids.

A spirituous liquor may be obtained from Indian corn, in consequence of the changes which take place in its saccharine matter.

12. *Bohnenbergen's Electrometer.*—This instrument is intended to indicate at once the nature, as well as presence, of electricity. The exterior is formed of a cylinder of glass, about two inches and a half wide, and three inches and a half high: it is closed at top by a brass plate, from which descend two of De Luc's electric columns, each containing about 400 discs of gilt and silvered paper about three lines in diameter, and terminated below by brass rings; these tubes are one inch and a half distant from each other, and between them is placed a tube of glass, which, passing through the cover in the manner of Singer's insulation, supports a wire terminated below by two gold leaves, and above by a metallic plate. It is easy, from this disposition, to perceive that when the leaves are unelectrified they will hang midway between the tubes; but when affected by the approach of electrified bodies, they will diverge and indicate by the attraction of the leaf on the one side, on the other the nature of the charge.

13. *On the Composition of the Prussiates or Ferrugmous Hydrates.*—These compounds which have drawn the attention of a great number of chemists to their examination, frequently without much success, have lately been investigated with great ability by M. Berzelius, and a number of very interesting points with regard to them established. Without tracing what had

previously been done by others, and which is well known to the scientific world, an attempt will be made in the following lines to present to view the result of M Berzelius' labours.

The first object was, to ascertain the proportion of the iron to the other base in the ferro-prussiates The salt with base of potash was first examined; it was purified by fusion, solution, and crystallization, after which it lost nothing by exposure to air for two days, but at a temperature of 140° Fahr. effloresced and diminished between 12 9 and 12 1 *per cent*, it did not then lose weight by a heat above that of boiling water: two grammes (30 89 gr.) of this salt thus dried, were mixed with sulphuric acid, it heated a little, but suffered no further change till its temperature was raised by a spirit-lamp, when sulphurous acid and hydrocyanic acid were liberated The heat was continued till all excess of sulphuric acid was driven off and the mixture then dissolved in warm water containing a little muriatic acid, the solution was precipitated by ammonia and the oxide of iron, collected, washed, and dried; it weighed in different experiments between 4 and 43 of a grammie (6 41 gr.) The solution was then evaporated, and the sulphate of ammonia sublimated by heat in which operation it was found advantageous to introduce a small piece of carbonate of ammonia in a spoon into the covered crucible in this way 1.894 grammes (29 20 gr.) of sulphate of potassa were obtained. The mean result of several experiments similar to the above, gave the following proportions for some of the elements of the ferro-prussiate of potassa:

Potassa.....	44.62 containing	7 58 = 2	of oxygen.
Protoxid of iron	16 64 ——	5 79 = 1	——
Water	12 7 ——	11.3 = 3	——
Loss.....	26 04		

from which it results that the potassa contains twice as much, and the water thrice as much oxygen as the iron in the state of protoxide

The ferro-prussiate of baryta was prepared from prussian blue and the hydrate of baryta. When heated, and the residue analyzed, it gave

Baryta.....	51.273 containing	5 38 = 2	of oxygen.
Protoxide of iron	11.865 ——	2 7 = 1	——
Water.....	16 56 ——	14.72 = 5.5	——
Loss..	20 302		

Here also the proportion between the oxygen of the baryta and that of the protoxide of iron is as 2 to 1.

The ferro-prussiate of lime, prepared in the same manner as the former salt, was obtained in crystals, of which 100 parts

Lime.....	22.45 containing	6.20 =	2 of oxygen,
Protoxide of iron	13.69 ——	3.12 =	1 ——
Water.....	39.61 ——	35.21 =	11.5 ——

The ferro-prussiate of lead was prepared, by adding solution of nitrate of lead to ferro-prussiate of potassa, the latter being in excess, the precipitate was then washed and dried. In consequence of the vicinity of the point of perfect dryness to that at which the salt began to effloresce, it was difficult to ascertain the quantity of water, but Mr. Berzelius is inclined to consider that, as with the ferro-prussiate of potassa, so also the water in this salt contains as much oxygen as is contained in both the bases together. On analysis 100 parts gave,

Oxide of lead ..	70 containing	5.09 =	2 of oxygen,
Protoxide of iron	11.9 ——	2.57 =	1 ——
Loss.....	17.7		

These analyses of compounds taken from the three classes of bases, suffice to prove, that whatever be the state of the iron in those salts, *it requires in the state of protoxide half as much oxygen as the radical of the other base.*

The second section of M. Berzelius' Memoir contains an account of experiments on the acid of these salts. The first experiments, in which sulphuretted hydrogen, and fused boracic acid were made to act on the salt, accorded with the opinion advanced by Mr. Porret, that the iron existed in the metallic state; but not considering them as decisive, the investigation was carried on still further. A portion of the anhydrous ferro-prussiate of potassa was heated with peroxide of copper, and this gas collected over mercury, it was a mixture of carbonic acid and nitrogen, in the proportion of three volumes of the former to two volumes of the latter, and no water was produced; when the experiment was repeated at a higher temperature, the same result was obtained; when the residue was digested in water an alcaline solution was obtained which precipitated carbonate of lime with lime-water. As these proportions differ from those of Mr. Porret and Doctor Thomson, the apparatus was tested by analyzing the cyanuret of mercury in it; the carbonic acid then exactly doubled the azote in volume, and by other trials the mode of operating was found to be perfectly efficient and correct.

The analysis was repeated with the ferro-prussiate of baryta, and the volumes of gases were again as 3 : 2.

It now became of importance to ascertain how much carbonic acid was retained by the bases of the salts analyzed, and whether these bases remained in the state of common carbonates, or were in some other state. To determine this point, carbonate of potassa was heated with six times its weight of oxide of copper, and at a red heat, carbonic acid gas was liberated; so that it appears, the oxide of copper has the power of

driving off a portion of the carbonic acid, and forming a kind of double salt in which it may be presumed $\frac{1}{3}$ of the potassa is combined with the oxide of copper, and $\frac{2}{3}$ with the carbonic acid. Water decomposes this combination, dissolving the caustic and carbonated alkali, and leaving the oxide of copper free.

Hence it became necessary to analyze a salt, the base of which would not retain carbonic acid, and that formed by lead was selected for the purpose. A certain quantity of this salt was heated with twenty-five times as much peroxide of copper, and yielded a mixture of 2 volumes of carbonic acid, and 1 volume of nitrogen. The quantities of the gases were such, as to give for 100 parts of the salt 11.05 of carbon, and 12.84 of nitrogen, or together 23.89 of cyanogen. This added to the weight of the other elements of the salt employed, surpasses the whole weight by 6.19, supposing the bases are in the state of oxide; but, if the prussiate be considered as composed of 1 atom of cyanuret of iron, with 2 atoms of cyanuret of lead, then the weight of the cyanogen, the iron, and the lead, would be almost exactly what it ought to be.

To prove that, in this compound, the metals were not in the state of oxides, sulphuretted hydrogen was passed over it in the heated state; no water was formed but hydrocyanic acid, protosulphuret of iron, and sulphuret of lead, were produced; and the weights of the products agreed as exactly as possible with the theoretical view taken of its composition, which is as follows:

	By experiment	By calculation.
Iron	8.81	8.68
Lead	65.91	66.18
Carbon . .	11.05	11.55
Nitrogen .	12.84	13.59
	98.61	100

The composition of the ferro-prussiate of potassa will be 1 proportion or atom of cyanuret of iron, 2 of cyanuret of potassium, and 6 of water, or,

Iron . .	12.85	= Protoxide of iron . .	16.54
Potassium	37.11	= Potassa	44.68
Cyanogen	37.22		
Water . .	12.82		

The composition of the other two salts is exactly analogous, the quantity of water only varying.

These experiments prove, that the salts called prussiates, or ferruginous hydrocyanates, are really cyanurets, composed of 1 atom of cyanuret of iron, and 2 atoms of cyanuret of the

other metal. As for the water which appears to be combined with them, M. Berzelius, for various theoretical reasons, considers it as existing in the state of water of crystallization, and not as converting the cyanurets into hydrocyanates.

It then became interesting to ascertain how far the ferro-prussiate of ammonia resembled in its habitude and composition, the salts already analyzed; and hopes were entertained that it might be reduced to the state of a double cyanuret, but all attempts to deprive it entirely of water were vain; when heated, it was decomposed, and gave hydrocyanate of ammonia, cyanuret of iron, and water. A singular phenomenon occurs in this decomposition, for when the mass has been heated until cyanuret of iron only remains in the retort, if the heat be then raised, the mass suddenly takes fire, and burns vividly, as if oxygen gas had been introduced, though in fact, azote is disengaged at the moment. A quadricarburet of iron remains in the retort, which, when heated in the air, takes fire, and burns like tinder, being converted into peroxide of iron, with scarcely any change in weight. Hence this salt appears to be a compound of hydrocyanate of iron with hydrocyanate of ammonia.

In examining the nature of prussian blue, M. Berzelius first describes some of it properties. It is very hygrometric, so that it cannot be perfectly dried by sulphuric acid in a vacuum. When dried by heat, and inflamed at one edge, it burns like tinder, liberating carbonate of ammonia, and leaving peroxide of iron. When obtained in the pure state, by adding muriate of iron to the ferro-prussiate of potassa, and repeatedly washing, it becomes soluble in water; and, when dried in this state, appears like extractive matter. Its solution is precipitated by any other salt, or by an acid.

Some prussian blue was prepared by adding neutral muriate of iron to ferro-prussiate of potassa, and a portion of this was then decomposed, by acting first with caustic potassa in excess, separating the iron thrown down, and then acting on the solution by corrosive sublimate, by which the second portion of iron was separated. The oxide separated by the potassa, was to the latter portion as 30 to 22. By further analytical experiments, it was decisively proved, that in prussian blue thus prepared, the peroxide of iron contained twice the oxygen of the protoxide, and that, consequently, its composition is entirely analogous to that of the other cyanurets that have been examined. At the same time it is to be observed, that experiments on the combustion of the prussian blue with oxide of copper, gave results which did not indicate the same proportion between the cyanogen and the iron, so that uncertainty still rests on the true nature of this substance.

Some pure prussian blue was diffused in water, and sulphuretted

hydrogen passed through it; when the substances had acted on each other for some time, the pigment became of a dull white colour, whilst the fluid became opalescent from the deposition of sulphur. The liquid was separated from the solid matter, and the sulphuretted hydrogen separated by exposure to air; it was then acid and precipitated salts of iron blue, so that, at the same time that the gas reduced the deutoxide of iron to protoxide, the excess of acid required for its neutralization in that state was liberated as ferro-prussic acid. The whole mass exposed to the air became blue, and at the same time partly soluble in water; but when again treated with sulphuretted hydrogen, the solution did not become acid, nor did it precipitate salts of iron blue, and the insoluble part became black: so that there are evidently two blue combinations, the one composed of three atoms of hydrocyanate of protoxide, and four atoms of hydrocyanate of deutoxide, in which the acid and oxygen of the second part is double that of the first; and another apparently composed of one atom of hydrocyanate of protoxide and two atoms of hydrocyanate of deutoxide.

"It appears," says M. Berzelius, "from the experiments mentioned, that the cyanurets of highly electro-positive radicals, as the alcaline metals, do not decompose water, or form hydrocyanates. The more feeble bases, as glucine, ammonia, and most of the metallic oxides, on the contrary, produce hydrocyanates, which, when heated, either do not produce cyanurets, or, in producing them, are in part decomposed by the action of the oxygen of the bases on the cyanogen, and the formation of carbonic acid, ammonia, and metallic carburets. With the exception of the hydrocyanate of iron and ammonia, it appears, that when one base is in the state of hydrocyanate, the other is also, so that there is no combination of a cyanuret with a hydrocyanate. When the cyanurets combine with an additional quantity of base, they appear to be changed into hydrocyanates, and the whole become sub-hydrocyanates; such is probably the state of the combination of cyanuret of mercury with oxide of mercury."

M. Berzelius then speaks of the nature of the ferro-prussic acid. This combination is produced by the action of a strong acid on the second base of the ferro-prussiates, which being removed by it, all the hydrocyanic acid unites with the protoxide of iron, so that it is combined with thrice as much acid as in the neutral compound. This substance was prepared by diffusing the moist cyanuret of iron and lead through water, passing sulphuretted hydrogen gas through, decomposing what portion of that gas remained in solution by adding a small quantity more of the salt, filtering, and evaporating under the air-pump-receiver. It left a white opaque uncrystallizable sub-

stance, having the following properties: solubility in water, the solution having a pure acid taste, followed by one slightly astringent; exposed to the air it deposited prussian blue, and became green; it has no odour previous to decomposition, but, when boiled, hydrocyanic acid is evolved, and a white powder is deposited, which becomes blue in the air. If cold water be saturated with the dry super-hydrocyanate, and the solution be left, small colourless transparent crystals form in it in groups. They appear to contain water, and the conjecture is hazarded, that in these the water replaces the second base of the ferro-prussiates. The white substance previously spoken of appears to contain no water, but to be a dry super-hydrocyanate of protoxide of iron. It may be preserved in vessels well closed, but in the air is gradually changed into prussian blue.

The double cyanurets of iron with potassium, barium, and calcium, when heated, evolve nitrogen, and the cooled mass, when dissolved in water, separates into quadricarburet of iron, and hydrocyanates of the other bases; so that the cyanuret of iron only has been decomposed, its nitrogen separated, and the other elements left combined in the carburet. When the dry cyanuret of iron and lead is decomposed by heat, it evolves nitrogen, and a double carburet of iron and lead is left, containing one atom quadricarburet of iron, two atoms quadricarburet of lead. If the salt be moist, the carburet of lead is in part decomposed, and carbonic acid formed. Prussian blue gives, on distillation, water, hydrocyanate of ammonia and carbonate of ammonia, water appearing the whole of the time. After these substances had come over the retort was heated red, and the matter within heated and glowed brilliantly, as with the ferro-prussiate of ammonia. The substance left is a tri-carburet of iron. Ferro-prussiate of copper produces, on distillation, water, nitrogen and carbonate and hydrocyanate of ammonia; the substance left is a compound of one atom quadricarburet of iron and two atoms bi-carburet of copper. Ferro-prussiate of cobalt yields, by distillation, nitrogen and carburets of the metals. The cyanuret of iron and silver is a true cyanuret; when distilled it yields cyanogen, nitrogen, silver, and quadricarburet of iron.

M. Berzelius draws the following conclusions from these experiments:—1. That the cyanurets of the alcaline metals retain their cyanogen at high temperatures, but that the cyanuret of iron combined with them is decomposed, producing nitrogen and quadricarburet of iron. 2. The cyanurets of the other and more reducible metals are decomposed by a high temperature. Those which may be obtained perfectly free from water yield nitrogen and double quadricarburets; those which preserve their water until the moment of decomposition lose a certain quantity of carbon, so that though the iron remains as

quadricarburet, the other metal remains combined with carbon in an inferior degree as a tri or bi-carburet. 3. The reducible metals lose their cyanogen, and retain no carbon.

M. Berzelius then remarks on the nature of these new carburets of the metals, which, containing four, three, and two proportions of carbon, present a class of bodies analogous to the sulphurets, arseniurets, &c.; he considers the decomposition of the cyanogen in the cyanurets as due to the affinity of the metals of the carbon; and observes, that in distilling vegetable metallic salts the residues which are obtained, and which till now have been considered as mixtures of carbon and the metal, are true compounds.

The observations which then follow on the phenomenon of ignition, observed in many of these experiments with the quadricarburets, &c., are highly interesting, but we must refer the reader to the original paper for them; the great length of this abstract prevents us from noticing any thing but matter immediately connected with the object of the paper.

The cyanurets with concentrated sulphuric acid are more or less dissolved without decomposition; those of iron and potassa, of iron and baryta, dissolve entirely, yielding a colourless solution, which is not decomposed at 212° Fahr., others dissolve in small quantity, whilst the greater proportion remain undissolved in combination with the acid. When the acid is poured into the powdered cyanurets, the mixture heats, swells, becomes pulpy, and if soluble, gradually dissolves, though a great quantity of sulphuric acid is required for this effect. The addition of a little water troubles the solution, and part of the acid compound falls, but if much water be added decomposition takes place, super-hydrocyanate of iron and super-sulphate of the other base are produced, or if the cyanuret is insoluble it re-appears with its common characters. If the acid solution be heated at a certain temperature, the cyanuret is decomposed, sulphurous and carbonic acids with nitrogen are disengaged, and super-sulphates of ammonia and of the bases employed, remain. M. Berzelius could not succeed in producing the new gas, which Dr. Thomson says is found on those occasions, nor does there appear to be any reason to believe in its existence. M. Berzelius describes the action of sulphuric acid on several of the double cyanurets, and concludes this part of his paper, by expressing his opinion, that they should be considered as double acid salts, where two bases are combined at the same time with excess of the two acids.

M. Berzelius concludes this very important paper by some observations on the preparation of alkaline cyanurets from prussian blue; if prussian blue of commerce and potassa in excess be made to act on each other, and the solution be made by

crystallization to yield the ferro-prussiate of potassa, a mother liquor remains which will not crystallize; but, by slow evaporation effloresces in greenish vegetations. This is a particular modification of the cyanuret of iron and potassium, which is soluble in water, and on exposure to a moist air, becomes brown. Its solution, when evaporated, yields small green scales; and these, when analyzed, so closely resemble in composition, the yellow salt, that no conclusion can be drawn from the experiment.

This salt may be converted into the yellow salt, by being carefully fused in a close crucible; and when cold, dissolved in water, the fluid will contain cyanuret of iron and potassa, hydrocyanate of potassa, and carbonate of potassa. Acetic acid will decompose the two last salts, the solution is to be evaporated and acted on by alcohol, the double cyanuret is then thrown down; it may be collected, dissolved, and crystallized.

Barytes, by acting on prussian blue, also forms the green compound. Lime produces very little of it, but ammonia forms it in such abundance, that sometimes nothing else is obtained. It then crystallizes in small green needles. Its solution is precipitated by alcohol as a sirup; during evaporation, it deposits a green powder, and by long exposure to the air, is in a great measure decomposed. *Annales de Chimie*, xv. pp. 144, 225.

14. *Ure's Chemical Dictionary.*—Dr. Andrew Ure, of Glasgow, has just published "A Dictionary of Chemistry, on the basis of Mr. Nicholson's; in which the principles of the science are investigated anew, and its applications to the phænomena of Nature, Medicine, Mineralogy, Agriculture, and Manufactures, detailed." We regret that the length of our observations on Dr. Thomson's System of Chemistry have prevented an extended notice of this work, in its proper place, and which we are obliged to reserve for a future Number. It is a work which displays considerable diligence, and equal knowledge of the subjects of which it treats, and will prove a valuable addition to the student's library.

III. Natural History.

§. Geology, Mineralogy, Meteorology, &c.

1. A very valuable work has just been published by Dr. Mac Culloch, entitled, "*A Geological Classification of Rocks, with descriptive Synopses of the Species and Varieties, comprising the Elements of Practical Geology.*" Upon a future occasion we propose to discuss the merits of this book more at length, and shall therefore confine this notice to a bare sketch of its contents, from which, however, our geological readers will be able to draw some conclusions respecting its interest and

importance. We have indeed regretted that Dr. Mac Culloch has so long withheld his practical information on systematic geology, since we perused his work on the western isles of Scotland, a work which displays attainments peculiarly fitting him for the task which he has now undertaken.

After some introductory remarks on the methods of arranging rocks, which have been adopted by different mineralogists, and on the plan of this arrangement and nomenclature, Dr. Mac Culloch gives the following general catalogue of rocks, succeeded by some remarks on their order of succession in nature:

PRIMARY CLASS.	SECONDARY CLASS.
Unstratified.	*Stratified.*
Granite	Lowest (red) Sandstone
Serpentine	Superior Sandstones
Stratified.	Limestone
Gneiss	Shale
Micaceous Schist	*Unstratified.*
Chlorite Schist	Overlying (and venous) Rocks
Talcose Schist	Pitchstone
Hornblende Schist	OCCASIONAL ROCKS.
Actinolite Schist	Jasper
Quartz Rock	Siliceous Schist
Red Sandstone	Chert
Argillaceous Schist	Gypsum
Primary Limestone	Conglomerate Rocks
Compact Feldspar	Veinstones

APPENDIX I.
Volcanic Rocks.

APPENDIX II.

Clay, Marl and Sand	Alluvia
Coal	Lignite and Peat.

Dr. Mac Culloch apologizes for the introduction of coal and peat into this list; but the connexion of the former with the strata in which it lies, and the important illustrations of its history afforded by the latter, amply justify their insertion.

With respect to the order of succession of the primary class, the claim of granite to the lowest place is unquestioned, but after it no certainty can be obtained, for the others are all found in its occasional contact and in uncertain order; to illustrate this fact, the author inserts a table shewing the irregular order of succession in rocks, in several parts of Britain.

The 7th, 8th, and 9th chapters relate to the aspect and structure of rocks, and in the 10th their composition is discussed, illustrated by a valuable catalogue of their component minerals.

Dr. Mac Culloch then proceeds to what we consider as a highly important part of geological science, though hitherto

very unscientifically treated; we mean, the transition which so often occurs in rocks, not only between the several varieties of each family, but even between the families themselves, in consequence either of their gradual variation of character, or of the loss of one or more of the ingredients which constitute the distinction. Upon these subjects, our author has some excellent remarks; they have generally been slurred over by modern geologists, in consequence of the difficulties in which they involve the theorist; but Dr. Mac Culloch, who is purely practical, and, strange to say, neither Vulcanist nor Neptunist, gives them their due importance and appropriate description.

The 13th chapter contains a synoptic view of the general characters of the families of rocks included in the arrangement before us. To describe the characters of rocks so as to enable the student to recognise them in mass, as well as in hand specimens, is a task of no small difficulty, and one which we do not hesitate to say, Dr. Mac Culloch has performed in a very superior manner; unlike some modern geological writers, who have aimed rather at obstructing the progress of the student, by throwing an accumulation of difficulties into his path, without giving any clue to their solution, he has succinctly, but clearly announced the obstacles, and, in the greater number of instances, has succeeded in their removal.

On the whole, the science of geology, if so it may be called, is much indebted to Dr. Mac Culloch. In his various papers in the Geological Transactions, and in his book on the Western Isles, he has shewn himself an indefatigable collector of facts, and a most observant traveller; in the work before us he appears equally successful as an elementary and systematic writer. We are indebted to him for the following notice of two new minerals, which ought to have appeared in our Number.

2. A new mineral, to which I gave the name of Conite, was described in my work on the Western Islands, as found in Mull and in Glen Farg It was subsequently mentioned to have been found in the Kilpatrick-hills, and I must now add, to increase the list of its localities, that I have since found it in Sky, in similar situations, namely, investing or filling cavities in trap rocks, and accompanying different members of the zeolite family.

It happened that Professor Schumacher had, about the same time, applied the same name to a variety of limestone, deriving his term from the Greek, κονια or κονις, as applied to chalk or lime. The inconvenience of this was, of course, immediately apparent; and although it is not likely that the term conite, thus used, will long maintain its place in our catalogues of minerals, since, like lucullite, and many others, it only serves to encumber the science with a catalogue of useless names, I have

been induced to change the name of the mineral which I have described, and to request you to give it circulation through the medium of your Journal. The name having been suggested from the powdery form in which this mineral has alone yet been found, the Greek word κονις, as applied generally to powder, may as easily be used in compounding the term KONILITE. It is not cacophonous, and answers the purpose of describing the most remarkable character of this mineral; while it avoids any collision with the term to which I have alluded.

3. *Native Oxide of Chrome.—A new Mineral.*—The combinations of this metal with two others, namely, lead and iron, under different forms, have for some time found a place in our catalogues of minerals. A place must now also be made for Chrome itself, in that division of mineralogical systems which is allotted to the metals. I am not aware at least, that the oxide of chrome has yet been found by any one in a native state; certainly it has not been enumerated in any system of Mineralogy.

I have recently discovered it here in Shetland, in the island of Unst. It is found in cavities in the chromate of iron, which abounds in this island, so as, for the space of many miles, to be scattered over the surface of the ground, and even to be used in common with the loose stones which it accompanies in the building of dykes.

This oxide is easily recognised by its beautiful green colour, and does not seem to differ from the green oxide produced in our laboratories by the action of heat. In some places it is merely diffused through the fissures of the ore; in others it occupies cavities resembling those of the amygdaloids. It is sometimes found in a powdery form; but at others it is compacted into a solid substance, bearing the marks of a crystalline structure, and somewhat translucent. Although it appears to be in abundance, when the specimens that contain it are broken, that effect is only the consequence of the brilliancy and contrast of its colour with the black and dark grey of the surrounding chromate of iron. It would be very difficult to collect many grains of it in a separate state from any of the fragments of the black ore which I examined.

The green oxide is accompanied by a yellow oxide of chrome, in cavities generally distinct from it, but sometimes intermixed, and in somewhat less abundance. This latter is more generally in the form of powder than the green. As the green oxide of chrome changes to yellow by heating it, M. Vauquelin appears to think that these are distinct oxides; but this point does not seem to have as yet been very satisfactorily examined. For the present purposes, it will, at any rate, be more convenient to consider them merely as varieties of one mineral

species. Those mineralogical writers who are desirous of increasing the number of species may easily follow a different course.

The mineral distinction of the oxide of chrome may be comprised in the following terms:

Oxide of Chrome.—This mineral is of a bright grass green colour, or else pale yellow; and is found either in a powdery or a compact form. In the former case, the aspect is dull; in the latter, the lustre resembles that of compactly crystallized limestone, or marble. It either invests surfaces, or fills cavities in chromate of iron.

Its specific gravity has not been examined. It is soluble by boiling in the alkalies, and communicates to them a green colour; but the solution is decomposed by further boiling, and the oxide is precipitated. By this character, and by its communicating a green tinge to glass, before the blow-pipe, it may be recognised and distinguished. It occurs in Unst, one of the Shetland Isles.

Lest your readers should conceive that I had fallen into an error, in describing this mineral as new, I ought to add to this communication, that the oxide of chrome, described in Monsieur Lucas's arrangement of minerals, is a very different substance, and, I may add, improperly named. I need not quote from a book which is in the hands of many mineralogists. It is sufficient to remark, that his mineral is a compound substance, into which the oxide in question enters only as an ingredient. It would be proper that its name should be changed, to prevent confusion; the right of possession is clearly in the present substance.—I am, yours, &c.

Shetland, August, 1820. J. Mac Culloch.

4. *On Fullers' Earth in Chalk, by the* Rev. C. P. N. Wilton, *Gloucester.*—The situation of the chalk-pit, in which the fullers' earth is found, is upon the side of a hill, forming part of the range of the South Downs, in Sussex, immediately above the village of Bepton, from whence that portion of the downs derives the name of *Bepton-hill.* It is distant three miles and a half, south, from the town of Midhurst. The elevation of that part of the hill, where the chalk is situated, above the level of the village, is about 400 feet. Upon my first entering the pit, in the month of May, 1820, I was struck with the appearance of an horizontal layer, consisting of a *greenish brown* earth, passing into *yellowish white* and *brown*; which, upon examination, was found to contain the characteristic qualities of fullers' earth. The layer varied from three to four inches in thickness; and was about a foot below the surface of the hill, having the chalk, which is of the upper formation, as well above as below it. The pit abounds with beautiful *chalk* specimens of different

varieties of zoophytes; and is remarkable, for containing near its summit, stalactites in the interstices of the chalk. The only flints I observed in the pit, were a few detached pieces, both angular and rounded, interspersed throughout the chalk. *This* pit having been worked but a few feet below the surface, no horizontal layers of flints present themselves; though in others, in the neighbourhood of this pit, worked to a greater depth, in the same range of the downs, and in the same chalk formation, they abundantly occur.

5. *Discovery of Retinasphaltum in the Independent Coal Formation.*

Dear Sir,—In pursuance of your request, I send you the following account of the Retinasphaltum in this vicinity. It occurs in the Independent Coal Formation, of the south part of Staffordshire. I found the first specimen about two years ago, near a village called Rowley; and have since perceived it at Oldbury, West Bromwich, and Tipton, but not in large quantities, as it is by no means a common mineral. The coal in which it is found, is chiefly composed of mineral charcoal, or carbonized wood, and the presence of retinasphaltum in it strengthens the idea, that some of the older species of coal owe their origin, at least partially, to ancient forests. Retinasphaltum has been hitherto considered as peculiar to the very recent formation of wood coal, at Bovey, near Chudleigh, in Devonshire; but this affords an instance of it in a very different geological situation. I have deposited specimens of it, and some other local fossils, in the collection of minerals belonging to the Birmingham Philosophical Institution, where they may be seen. The following appear to me to be its characters, but as it has been subjected to chemical analysis by Professor Brande, perhaps that gentleman will give any farther description that may be required.

It forms thin layers, about one-sixteenth of an inch in thickness, parallel with the lamina of the coal; its colour is blackish brown and brownish yellow; on rubbing it becomes very light brown; it is very brittle and light. On applying the flame of a candle, it takes fire with great rapidity, emitting bright white sparks and flame, and an aromatic smell. It is partially soluble in alcohol.—I have the honour to be, Sir,

Your most faithful and obedient servant,

JOHN FINCH.

Birmingham, March 15, 1821.

To Charles Hatchett, Esq., Belle-Vue House, Chelsea.

I have examined this substance, now discovered by Mr. Finch for the first time, associated with ordinary coal, and find that it, in all respects, resembles the retinasphaltum, as described by Mr. Hatchett, in the *Philosophical Transactions.*

W. T. B.

6. *Meteorological Observations at Melville Island.*

Abstract of the Register of the Thermometer and Barometer during ten months, at Winter Harbour, Melville Island, North Georgia, 1819 and 1820.

Latitude 74° 47′ 18″, Longitude 110° 46′ 30″ W.

DATE.		THERMOMETER.				BAROMETER.			
		Maximum.	Minimum.	Mean.	Range.	Maximum.	Minimum.	Mean.	Range.
		°	°	°	°	inches	inches	inches	inches
1819.	October.	+ 17.5	−28	— 3.46	45.5	30.32	29.1	29.818	1.22
	November	+ 6	−47	−20.6	53	30 32	29.63	29.945	0.69
	December	+ 6	−43	−21.79	49	30.75	29.1	29 865	1.65
1820.	January	− 2	−47	−30.09	45	30.77	29.59	30.076	1.18
	February	− 17	−50	−32.19	33	30.15	29.32	29 769	9.83
	March .	+ 6	−40	−18.1	46	30.36	29.	29.803	1.26
	April . .	+ 32	−32	— 8.37	64	30.86	29.4	29.976	1.46
	May . . .	+ 17	− 1	+ 16.66	51	30.48	29.25	30.109	1.23
	June . . .	+ 51	+ 28	+ 36.24	23	30.13	29.5	29.823	0.63
	July . . .	+ 60	+ 32	+ 12.4	28	30.01	29.13	29.666	0.88

REMARKS.—The thermometer was fixed, during the winter, on the south side of a david projecting from the ship's side, and was usually from 3° to 6° higher than one suspended freely in the air at a distance from the ship. This difference increased as the summer advanced, and the sun rose sufficiently above the horizon to heat the ship, amounting latterly to 15° or even 20° about noon. The thermometer was, of course, always shifted to the shaded side of the ship or david.

On the 15th of February, at 6 P.M., a thermometer suspended freely in the air at a distance from the ship stood at −55°, being the lowest degree registered during the winter.

The very low temperatures were invariably in calm and clear weather; the rise of the Thermometer being the immediate consequence of a breeze springing up, and being proportioned to its strength.

The Barometer rose with northerly and westerly, and fell with southerly and easterly winds. but it was not so decided that the indications preceded the changes as it is stated to be in more southern climates.

We are indebted for the above abstract to Captain Edward Sabine, and much regret that an opportunity has not been afforded us of communicating to our readers a similar abstract of a variety of important and interesting experiments and observations made by that gentleman during the Polar expedition. We shall anxiously look for them in Captain Parry's narrative about to be published.

7. *Chromate of Iron in the Island of Unst.*

To the EDITOR.

Sir,—The paragraph respecting the discovery of chromate of iron in Shetland, which appeared in a late Number of your

Journal, escaped my observation, until pointed out to me by a friend. That I found the chromate of iron, in the island of Unst, in the year 1803, as stated in your *Journal*, is true; but, having mistaken it for another mineral, and not having published any subsequent notice of it when I ascertained its nature, the honour of that discovery is justly due to Dr. Hibbert. As far as I recollect, there has appeared no notice of my visit to Shetland, except what is contained in my hasty letter to Mr. Neill; and I owe it to the public, to explain how my name has been connected with the discovery.

During my *only* visit to Unst, I found a substance which, at first, I conjectured to be horn-blende; but its great specific gravity induced me to consider it as an ore of iron. It is thus noticed in my original notes, taken on the spot, and still in my possession: "In the serpentine, find some veins of *micaceous iron ore?*" On comparing it with mineralogical descriptions, I was unable to assign it a place in my collection, until several years afterwards, when the sight of some specimens of chromate of iron, from America, led me to examine the mineral from Unst; and I became satisfied of their being of the same nature. Since that time, it has been arranged in my collection (now deposited in our Royal Institution), as a specimen of chromate of iron from Shetland; and as such it has been shewn in my lectures. But as I consider priority of publication the fairest claim to the merit of discovery, I regard Dr. Hibbert as entitled to the honour of having added an article of considerable importance in the arts to the native productions of our common country.—I have the honour to be, Sir,

Your most obedient Servant,

THOMAS STEWART TRAILL.

Liverpool, March 14, 1821.

IV. GENERAL LITERATURE, &c.

1. *Recent Discovery of a Fragment of Art in Newfoundland.*—A discovery has been made in Newfoundland, during the last summer, which, trifling as the object is, has not a little exercised the conjectures of the antiquarians of that island. About half a mile from the shores of Gander Bay, there was found a fragment of a small pillar of white marble. This fragment is octangular; about 18 inches long, and 10 inches in diameter. Its surface is as much corroded by the effects of the weather, as those parts of the statues of the Parthenon which have suffered most. It is probable, consequently, that it has lain there for a considerable time.

It cannot have been left in ballast, because it is half a mile inland, and because no ships can come within three-quarters of a mile of the shore of this place. This part of the country

is not inhabited, and no similar stones, or works of art, could be found on searching in the same neighbourhood. I must also observe, that the texture of this marble is very remarkable, resembling none that I have ever seen, and perfectly different from any of those used in sculpture or architecture. It is of a yellowish white colour. The texture is in some places crystalline granular, of a large grain; but there are every where intermixed with it parts of very complicated curvatures; capable of being separated in succession, in parallel curved laminæ as thin as paper. These scaly concretions are sometimes an inch or more in dimension. Besides this, there are found distinct irregular laminæ of hard calcareous clay, or very argillaceous earthy limestone dispersed through the stone.

If the Newfoundland antiquaries cannot settle this other point, it must be left to the ingenuity of those who have reasoned so ably on the works of ancient art, found in many parts of America. In tracing the migration of Asiatic nations thither, it is easy to settle a colony, and build a city, in Newfoundland.

J. M.

2. *Consumption of Food in Paris, for* 1819.

Wine	hectolitres,	805,499 or	21265173.6 galls
Brandy	ditto	43,849	1157413.6
Cider and perry	ditto	15,919	420261.6
Beer	ditto	71,896	1898051.4
Vinegar	ditto	20,750	547952.4
Oxen	head,	70,819	
Cows	ditto	3,561	
Ditto, milch	ditto	2,918	
Calves	ditto	67,719	
Sheep	ditto	329,070	
Hogs	ditto	64,822	
Cheese	kilogrammes	1,267,564 or	2796911 lbs.
Sea fish	in value, francs	8,165,920 or	1,340,230
Oysters	ditto	821,618	34,234
Fresh-water fish	ditto	502,780	20,949
Poultry and game	ditto	7,161,402	298,392
Butter	ditto	7,105,533	296,064
Eggs	ditto	3,676,502	153,187
Hay	trusses	7,822,640	
Straw	ditto	11,054,371	
Oats	hectolitres	923,022	24,367,781 galls

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THE

QUARTERLY JOURNAL,

July, 1821.

ART. I. *On the best Method of warming and ventilating Houses and other Buildings.* *By* Mr. CHARLES SYLVESTER.

[Communicated by the Author.]

[We are happy to comply with the request of several of our Correspondents, and to enter upon the subject of heating and ventilating our apartments; it is a subject upon which few persons venture to think for themselves, and is too frequently conceded to the management of the ignorant, or, what is worse, is intrusted to some half-informed speculator. The combustion of smoke is another branch of this inquiry, and, although but lightly touched upon in the following paper, shall not be forgotten. The absurdities which have of late been authoritatively thrust upon the public in relation to it, are so gross, as to merit more serious and extended notice than it is in our power to bestow upon them; but we shall humbly contribute whatever is within our reach to rectify the errors that have been diffused, and to show the inanity of the promises, with which this subject has lately been ushered into notice.]

THE action of the sun's rays on the surface of the earth, and the consequent accumulation of sensible heat is a most instructive lesson, for the best mode of applying artificial heat for warming buildings; and our best ideas of ventilation are derived from those mechanical changes in the atmosphere occasioned by the rarefaction of the air, from the heat it acquires in contact with the earth's surface. If the earth were perfectly transparent, or had a surface capable of perfect reflection, it would not be at all heated by the sun's rays; and our atmosphere, supposing it to exist under such circumstances, would be destitute of those changes which are daily evinced in an

infinite variety of currents. If the substance of the earth were a much better conductor of heat, we should experience less extremes of heat and cold upon its surface. The summer-heat would be more rapidly absorbed by the earth, and the rigour of winter would be much diminished by the heat derived from the earth in the sun's absence. The nature of soils, as regards their conducting power, has doubtless a great influence in limiting the extremes of temperature in winter and summer. The heat produced on any part of the earth's surface, will be the greatest where the rays of the sun are vertical, and the surface of such a nature as to receive the rays with the greatest facility, its substratum being, at the same time, the worst conductor of heat. The air immediately in contact with this surface becomes heated, and specifically lighter than its superstratum. This causes, in the first instance, two simultaneous currents; one perpendicularly upwards, and the other, a lateral one from all the surrounding parts towards the centre of the heated surface. After the ascending current has attained a certain altitude, it progressively assumes an oblique and ultimately a lateral direction, but in an inverse order to that of the lower stratum. By this beautiful provision of natural economy, the heated air of the torrid zone, and the chilling currents from the polar regions mutually contribute to the prevention of those extremes of heat and cold, which would otherwise be fatal to every class of animated beings.

To form some idea of the effect which would result from a vertical sun upon a good reflecting surface, such as a black soil, unattended by the currents of air above alluded to, we have only to observe the heat generated in hot-houses; in which case the heated air is to a certain degree prevented from ascending, and consequently the lateral current from coming in. The heat produced by these means, therefore, will be greater in proportion to the blackness and lightness of the soil, to the tightness of the surrounding walls and windows, and the perpendicularity of the sun's rays. Hence we see the importance of our atmosphere independently of its agency in respiration. Without it, bodies would receive their heat on those parts only

which are exposed to the direct rays, and would become unequally heated in the inverse ratio of their conducting power.

When bodies are immersed in a heated medium, such as in air or water, they receive their heat on every side; and it has been found by experience, that this mode of applying heat is of particular importance in the economy of animals and vegetables.

Nothing can be more unphilosophical than the common mode of warming ordinary rooms by open grates. To put an extreme case of this mode of warming, we have only to instance the effect of making a fire in the open air. In this instance, there is free access for the ascent of the rarefied current, and the lateral current rushing towards the fire is felt on every side, supposing no natural breeze prevailed. The effect of this cold current is so conspicuous on the human body, that few unaccustomed to such exposure would escape some variety of those affections called colds.

Our common dwellings approach this extreme case in proportion to the size of the fire, the width of the chimney, and the access of cold air by the doors and windows. In every case, as much cold air must be admitted as will effect the combustion of the fuel, and supply the demands of respiration. The air which would be barely sufficient for these purposes, coming immediately from a cold atmosphere into rooms with grates even of the best construction, will ever be a barrier to that comfort which we ought to experience, and which by the aid of other means can be easily attained.

Notwithstanding the absolute necessity of admitting a certain portion of fresh air into every room, it is a common practice with builders to make doors and windows so tight as frequently to be the sole cause of a smoky chimney. To obviate this evil, some have let in a certain quantity of atmospheric air under or near to the fire grate. By this expedient, those sitting around the fire are not annoyed by the cold current, but an inconvenience arises from this contrivance, which more than counterbalances its benefits. The air entering the room so near the fire immediately supplies the current up the chimney without changing the air of the room. A crowded room, and the presence of

a number of lights, would, under such an arrangement, soon render the air unfit to breathe. Hence will appear the necessity for two currents into a room. The inlet for fresh air should be in a situation not liable to annoy those sitting in the room; the outlet is generally provided for in the chimney, which is commonly sufficient for rooms of ordinary size, but is mostly too small for large public rooms.

It will be evident from what has been observed, that in order to render rooms comfortable and wholesome, two objects are required. The one is, to keep up an uniform and agreeable temperature; the other to provide for a change of the air sufficient to preserve that degree of purity essential to health, and which persons under certain pulmonary affections can so nicely appreciate.

It is evident that the former of these objects can never be attained by radiant heat; and yet, an open fire, which scarcely affords any other than radiant heat, is so connected with our domestic habits that it will be very long before the open grate will be entirely set aside. Under these circumstances, it has been found most expedient to use the combined effect of radiant heat with a constant supply of fresh air, raised to an agreeable temperature in the winter; and which, in certain cases may be cooled during the excessive heat of summer.

Great difficulties have been experienced in most of the means hitherto employed for warming air. In the first place, from what has been previously observed concerning the action of the solar rays on the earth, the air cannot be warmed by radiant heat passing through it; therefore we can only give heat to a transparent fluid by bringing its particles in contact with a heated surface, and, in proportion as elastic fluids are more expansible, they are heated with more difficulty.

There are a number of properties which a body should possess, to afford a surface proper for heating air intended to warm and ventilate rooms. For the sake of economy it should be a good conductor of heat, in order that the radiant heat which it receives on one surface may be freely transmitted to the other. The surface to be heated should be clean, that is, free from any foreign matter, but not polished; and when the temperature

can be limited, it should never, under any circumstances be allowed to exceed 300°. Metals appear to be the best substances for heating air. The temperature is limited to 300° because the animal and vegetable matter, which is found mechanically mixed with the air at all times, will be decomposed if the temperature be raised a little higher. When this decomposition takes place, as is very observable when the heated surface is red hot, certain elastic fluids and vapours are produced, which give to the air a peculiar odour, and a deleterious quality which never fails to affect the health of those who inhale it for a length of time. This oppressive sensation has been mostly felt in churches and other places where large iron stoves are used and are sometimes heated to redness. The peculiar odour accompanying it has been erroneously attributed to the iron; and on this account, earthen ware or stone has been employed to form the exterior surface of the stove. It will, however, be found that whatever be the material, if the temperature at all approaches a red heat, the same smell will be perceived; as it arises entirely from the decomposition of the matter which is in the air, and not from the heating body. This matter is very visible to the naked eye, in a sunbeam let into a dark room.

When earthen ware or stone has been employed for stoves, its inferior conducting power has seldom allowed the exterior surface to get sufficiently hot, to produce the effect on the air above alluded to. And hence it has been less objectionable as affecting the purity of the air.

It must however be admitted, that if the body used for heating the air, does not undergo any change, a metal from its being a good conductor must be preferred to any other substance. Silver or platina, if it were not for the expense, would set aside every prejudice. But long experience has shown that iron possesses every essential property. The slightly oxydated surface which is common to all iron coming from the forge or the mould in casting, is well fitted for receiving radiant heat. And if its temperature be kept below a red heat, there does not appear to be any limit to its durability. The latter point, there-

fore, is put out of all doubt, since it is essential, that the iron shall not be heated to a degree capable of decomposing animal and vegetable matter, in order to preserve the purity of the air which is warmed in contact with its surface.

With a view to ensure the above objects, it will be necessary to dispose of the heat as it is produced from the combustion of the fuel, in such a way, that an extensive surface of iron shall be heated uniformly without the risk of attaining a much higher temperature than 300°. This can be accomplished by making the fire of a size proportionate to the interior surface of an iron vessel, and it is found that radiant heat is much more efficacious than the heat produced by flame and conducting flues. Having heated the interior surface of an iron vessel it may be conceived that the exterior surface will quickly attain the same degree, and that whatever heat may be carried off from the exterior will be as quickly given from the interior, and instantly replaced by the radiant fire.

The next material object is the means of disposing of the heat from the exterior surface. If it be surrounded by an open space, and that be connected with a flue or tunnel of a certain height, supposing there to be no inlet at the bottom, or outlet at the top, the air will commence a circulation; that on the heated surface would ascend, and its place be as constantly supplied by the surrounding air. In this way two currents will be established; one ascending from the heated surface, and the other descending on the outside of the tunnel; and these currents will go on, as long as any difference of density exists in the air of the different parts of the surrounding space. If now an opening be made in the bottom of this tunnel and another at the top, an ascending current will be kept up; which will be as the difference of density between external air and that of the heated column, and as the square root of the height of the tunnel

Let D be the density of the external air;

d, that in the tunnel, which will be inversely as the heat supplied.

V = the velocity which a heavy body would acquire by

falling through the height of the tunnel; and v = the velocity of the ascending air.

Then $v = V \times \frac{D-d}{D}$. This equally applies to chimneys, d being the density of the smoke.

The mere exposure of the heated surface in an open space, such as a small room, is not sufficient to produce the greatest effect. This is, however, the method at present used by sugar-bakers for heating the rooms in which they expose their sugars. The vessel so employed is of cast iron, and is called a cockle.

Various modifications of this method of heating air have been employed. The wall surrounding the heated vessel has been placed at various distances, in order to find the maximum of effect of a given fire. It was considered a great improvement, to place the wall at a distance, to admit of a sufficient quantity of air, and make a number of apertures in the wall, about two and a half inches square, with a view to compel the air to blow upon the heated surface. This method was employed more than thirty years ago, by William Strutt, Esq., of Derby, in his cotton-works. He afterwards made a great improvement on this plan, by inserting tubes in the apertures in the wall reaching near to the heated surface. By these means, the air is prevented from ascending before it comes in contact with the heated surface. A further improvement was made in this apparatus, by inserting similar tubes over the surface of the cockle, the shape of which was a square prism with a groined top. The cold air was made to pass through one half of the tubes; and the air so heated, became still more heated by being compelled to pass in a contrary order through the other half, into a chamber above, called the air-chamber. The stove, thus improved, has been employed by Messrs. Strutts in their works ever since, with complete success, and is similar to that by which the Derbyshire General Infirmary is warmed This stove has been fixed in different parts of the country and in London, sometimes with success; but so many circumstances besides the stove itself interfere, in arrange-

ments of this kind, that the plan has failed in many instances. And such will ever be the case with the best inventions, in the hands of men who are unacquainted with the principles on which they are founded.

Nothing can be more obvious, than the decided advantage which this stove possesses over all others, and nothing remained for its improvement but to give its different parts their proper proportions, and to vary its construction, so as to admit of its easy management in domestic use. By the former improvement, a larger quantity of air is admitted in proportion to the fuel consumed, and of course at a lower temperature. The advantages which result from this improvement will be obvious. The ventilation of the rooms warmed by it, is much more complete from a greater quantity of air being admitted; the temperature is more uniform, from the air being more dispersed; and, lastly, from the air being heated by a greater surface at a lower temperature, the apparatus is not in the least degree injured by the fire, and hence there does not appear to be any limit to its durability.

Nothing can be more vague and uncertain, than the opinions which have been formed of the different apparatus used for warming rooms by heated air. It has in consequence appeared to me a desideratum in inquiries of this nature, to be able to ascertain the power and merits of a stove, as we do those of an engine. For this purpose, my first object was to get an instrument capable of measuring the velocity of currents. After trying a variety of methods, I have found one with which I am perfectly satisfied. It consists of a very light brass wheel, in the form of that for the first motion of a smoke-jack. An endless screw upon the same axis gives motion to a wheel of fifty teeth, on the axis of which is an index, which is watched by the eye, when the instrument is exposed to the current. The wheel acted on by the current, is about two and a half inches in diameter, and the vanes or sails are eight in number, and fill up the whole circle, when their faces are parallel to the plane of their motion, and they are adjusted to an angle of 45°. Under these circumstances, I have found that fifty re-

volutions of the first motion take place, while the current causing those revolutions moves through forty-six feet.

In order to ascertain the power and merits of a stove, I generally take a period of twelve hours, beginning with a good fire, and leaving off with the same. During this time, the velocity and temperature in the main warm air-flue should be taken every half hour, and then the average of each taken, keeping an account of the coal consumed in the same time. The temperature of the outer air being also known, the excess of the average temperature above the atmosphere is the datum required.

From the average velocity, the number of cubic feet of air passing through the flue in the twelve hours may be known.

Put A = The number of pounds of air heated in twelve hours, allowing 14 cubic feet of air to 1 lb.

T = The excess of temperature above that of the atmosphere.

W = The weight in pounds of coal consumed in the same time.

E = The effect of the stove, which, in stoves of all sizes on the same construction, should be generally a constant quantity: Since A the quantity, and T the excess of temperature, are advantages to be produced by W the weight of coal.

E, the effect, will be directly as A and T, and inversely as W. Therefore, $E = \frac{A\,T}{W}$.

To give an example in practice:—A stove which is capable of warming 100,000 cubic feet of space to 60° in the coldest season, when placed at the depth of nine feet below the level at which the warm air is discharged, will furnish about 45 cubic feet every second, raised 60 degrees above the temperature of the atmosphere. To keep up this current and excess of temperature for twelve hours, it will consume not more than three bushels of coals, or 252 lbs. In this case, 49 cubic feet of air in each second will be 1,944,000 in twelve hours, equal to 138,857 lbs. Hence $E = \frac{138{,}857 \times 60}{252} = 32{,}930$. This

number may be taken as a constant quantity, expressive of the power of any stove; but it also expresses the weight of air in pounds, which one pound of Newcastle coal heats one degree of Fahrenheit's thermometer.

This number will not be strictly a constant quantity, as small stoves will not act quite to the same advantage as larger ones; and local and other circumstances will in some degree alter the result of experiments made in the manner above stated. This is more especially the case, when the admission of cold air and the discharge of foul air, are in any degree influenced by the wind.

The cold air is generally brought directly from the atmosphere; and, therefore, as its progress along this channel is affected by the wind, a greater or less quantity will pass through the stove. If the air be deficient, less heat is carried off from the heating surface, and a greater proportion goes up the chimney; on the contrary, when the wind blows into the cold air-flue, the two forces conspire, more air is admitted, more heat is carried off with the air, and of course less is wasted up the smoke-flue.

In all situations where it is practicable, I use an effectual means of regulating the admission of cold and the escape of foul air, by placing at the commencement and termination of these apertures a turn-cap or cowl, in which the vanes are so fixed as to let the wind blow into the one, and assist the escape of air from the other. Although this contrivance will always prevent a counter current, which without its use is sometimes the case; it does not prevent unequal quantities of air from entering, according to the strength of the wind. This is not found in practice to be a great inconvenience; for during the most perfect calm, the air admitted by the power of the stove alone, is sufficient for every purpose of warmth and ventilation: whilst with a tolerable fire in the stove when the wind is considerable, the air comes into the rooms at a higher temperature than the rooms require which is at least erring on the desirable side. If the quantity of air admitted under all states of the wind were required to be uniform, the aperture in the turn-cap for cold air

might contain a self-adjustment, by the action of which its area would always be in the inverse ratio of the velocity of the wind; by which means equal quantities of air would always be admitted in equal times.

The turn-cap for the escape of foul air is placed at the top of the building, and is made common to the roof. Under this arrangement all the rooms into which the warm air is admitted have each a foul air flue terminating in the cavity of the roof.

The contents of all the foul-air flues are therefore ultimately discharged at the turn-cap. This arrangement is adopted at the Derbyshire General Infirmary, and at the Wakefield Lunatic Asylum. In the summer season, when the stove is not in action, the ventilation will depend on the wind, which at some periods may not be adequate to that change of air required in hospitals. In such cases I have adopted an additional means of ventilation. Instead of making the foul-air turn-cap common to the roof, I have placed it at the top of a cylindrical cavity built in the roof. Into this cavity I bring all the foul-air flues, which also in this case may be smoke-flues, if constructed with brick. I also connect with the same cavity, the stove chimney, and, if possible all the other smoke-flues in the building. By this means, it may be expected, that some degree of rarefaction in the cylindrical cavity in the roof will be constantly going on, and that hence a perpetual current will be established from every room towards the general outlet. It would be difficult to adapt such an arrangement to old buildings, without great alteration in the roof. But it would be easily introduced into new houses. The advantages derived from it in ordinary dwellings would be very great. In the first place, there could not be an instance of a smoky chimney; in the next, a down current in an unoccupied chimney could not occur, and therefore the passage of the smoke of one chimney down another would always be prevented; and lastly, by having only one outlet for smoke in every house, and that an object which may be made ornamental, we should ultimately get rid of the great deformity which arises from the present appearance of chimneys in buildings.

In all situations where it is practicable to make a cold air flue, of considerable length under ground, the advantage is well worth securing. I have found by experience that a cold air-flue of 50 yards in length is capable of cooling the air in summer to about an arithmetical mean between the temperature of the air and the earth, and a similar advantage is produced by the earth warming the air in the winter season. The shape of the cold air-flue should be such as to present the greatest possible surface; the very contrary being essential to the best construction of flues for the warm air.

These facts will successfully lead to the means of cooling buildings in the tropical climates, and of warming the air when the winter's cold is much below the temperature of the earth.

Great Russel-street, Bloomsbury, May, 1821.

Art. II. *On the Height of the Dhawalagiri, the* White Mountain of Himálaya. *By* H. T. Colebrooke, Esq.

[Communicated by the Author]

In an essay on the height of the *Himálaya* Mountains, which was inserted in the 12th volume of *Researches of the Asiatic Society*, I offered reasons for the opinion of their great elevation: relying especially on the measurement of the *White Mountain*, emphatically so named, which towers above the rest of the snowy peaks seen from the plains of *Hindustan*. Its height was computed, from three sets of observations, taken by Captain Webb, at 27,551 feet above the observer's stations in *Gorakhpúr*; or 27,677 feet, making that allowance for refraction, which was found to bring the result of the several observations nearest to agreement. Even assuming all errors to be on one side, and in the extreme, it would appear to be 26,862 feet at the lowest computation. But such extremity of errors is hardly presumable; and considering the supposition of compensation of errors, and ordinary rather than extraordinary refraction, to be more likely correct, the inference was that the *White Mountain* may be about 27,600 feet above *Gorakhpúr*, or nearly 28,000 feet above the level of the sea.

Arguments were likewise deduced from observations of Colonel R. Hyde Colebrooke and Colonel Crawford, for the altitude of other lofty peaks; many exceeding 22,000 feet, and some rising to 24,000, and even 25,000 feet.

These calculations and estimates of elevation were exhibited as a mere approach towards a determination of the true height, yet substantiating the general position that the *Himálaya* is the loftiest known range of mountains; its most elevated peaks greatly exceeding the highest of the Andes.

That position, as well as the approximated determination of heights on which it was grounded, has been controverted. But the course of political events having since afforded facility of access which was before denied by the jealousy of the *Gurkhális* Mountaineers, accurate measurements, both barometric and trigonometric, of a great number of points, in the vicinity of the upper *Ganges*, *Jumna*, and *Setlej* rivers, have been carefully taken by different Surveyors, which irrefragably establish the general position of the transcendent altitude of the *Himálaya*: and a great multitude of peaks have been determined, which exceed 22,000 feet; a few rising above 23,000; and one measured by Captain Webb, no less than 25,669 feet.

These however do not equal the stupendous altitude of *Dhawalagiri*, or the *White Mountain*; also named *Gásákóti*. The routes of travels and surveys have not hitherto been directed to its vicinity. Their direction has been towards the upper Ganges and the *Setlej*. The more easterly mountains, toward the sources of the great *Gandhac*, have not been approached; and the measurement of the *White Mountain*, taken from the plains of *Gorakhpúr*, is yet to be confirmed by observations from nearer stations.

Previously, however, to the occurrence of those events, which have been alluded to as having laid open the mountainous confines of Hindustan to research, observations had been again made in the plains of *Gorakhpúr* to determine the elevation of *Dhawalagiri*, on the same principle on which Captain Webb proceeded during his previous survey in that province. Captain Blake, to whose labours these further observations are due, has been so

good as to communicate to me the particulars of them: I subjoin his letter.

The height of *Dhawalagiri* and four contiguous mountains has been computed from the data furnished by Captain Blake's survey; using the formula which I gave in the 12th volume of *Asiatic Researches;* and taking terrestrial refraction at one eleventh of the contained arc, which is the estimated mean quantity of ordinary refraction. The elements of the computation are exhibited in a tabular form accompanying.

The elevation of *Dhawalagiri*, taking the mean of three sets of observations, is thus found to be 27,615 feet above the plains of *Górakhpúr*, or 28,015 above the sea; differing only 64 feet from the computation founded on Captain Webb's survey.

Or allowing for terrestrial refraction one twelfth of the base; which was the rate that appeared to bring the result of the different observations taken by Captain Webb, nearest to agreement; the elevation of *Dhawalagiri*, deducible from the later survey, is 27,704 feet above *Gorakhpúr*, or 28,104 above the sea; differing from that deduced from the former survey by no more than 27 feet.

This near coincidence authorizes an expectation, that the true height of *Dhawalagiri*, when it shall be accurately determined, will be found very little wide of 28,000 feet.

Captain Blake's survey determines likewise the positions and altitudes of four other conspicuous mountains in the vicinity of *Dhawalagiri*.

About thirty-six miles east of it, and equally distant from the plain, is a mountain which rises to the height of 23,708 feet above *Górakhpúr*, or 24,108 feet above the sea.

Nearly midway between them, but somewhat less remote, is situated a mountain called *Set-ghar** or *Nepal*; its position, as the last mentioned name implies, is near to *Nepal* proper. The elevation of the summit is 24,861 feet above the level of *Górakhpúr*, or 25,261 feet above that of the sea.

Twenty miles west of *Dhawalagiri*, but less remote from the

* Probably *Swétaghar*, or the White Tower.

plains, are two peaks, rising from the same mountain mass, for they are but four and a half miles asunder. The highest of the two is *Chandragiri*, or Mountain of the Moon, a name common to many others. Its elevation is 22,607 feet; that of the contiguous lower peak is 21,535 feet above *Górakhpúr*; or 23,007 feet and 21,935 feet respectively, above the sea.

These altitudes, though much short of *Dhawalagiri*, tend to corroborate the estimate of its great elevation: for it may be seen in Captain Blake's sketch of the appearance of this portion of the snowy range, how greatly *Dhawalagiri* overtops the rest, lofty as they are.

It is to be hoped that some traveller may be induced to visit the *Himálaya* in that quarter, and explore the great *Gandhaki* river to its source at the foot of *Dhawalagiri*, and determine the elevation of the mountain by observations at the nearest accessible heights. Besides other subjects of research, it presents one of much interest in the abundance of organic remains there found: for it is thence that devout *Hindus* are supplied with Ammonites, an object of their idolatrous worship, under the appellation of *Sálagráma*.

H. T. C.

To H. T. Colebrooke, *Esq. &c. &c. &c.*

Dear Sir,

Having been appointed, by the Government of Bengal, in the year 1812, to survey the extensive province of *Gorakhpúr*; that year, the whole of 1813, and part of 1814, were occupied in surveying the southern portion, or that division of the province, lying south of the *Gograh*, or Great *Saryú* River. Proceeding northward, at the subsidence of the rains, in 1814, I had on the 1st of November near the village of *Urwára* a distinct view of the snowy summits of the *Himálaya* mountains; and from this station I took, with one of Troughton's theodolites (of six inches radius), the bearings and elevations of five of the snowy peaks, being those that were most remarkable; of which three possess names, and two are anonymous: the former are well known to the inhabitants of the subjacent

champaign (situate southwest of the mountains from 60 to 140 miles) under the appellation of *Chandragiri*; *Dhawalagiri*, (or Gasah Kotee); and *Sétgar*, (or Nypal.) At this station, although distant nearly one hundred and forty miles, these mountains have on the whole the most sublime aspect: this may be attributed to the smaller range of dark hills being lost at that distance in the perspective, for upon a subsequent and nearer view, at the station of *Maha deva diurnya*, the smaller hills intercept a refracted portion of the great chain, I say refracted, for a right line drawn from the first station to the base of the snowy peaks, would (owing to the spherical figure of the earth) at that distance, pass far below the base of the smaller range. From this first station the bearings of the five snowy peaks (corrected for magnetic variation of 2° 13′ 39″ East) are as follows, viz.

Peak A. bearing N. 8° 23′ 39″ E., and elevation 1° 5′
B, or *Chandragiri*, N. 10° 5′ 39′ E., and elevation 1° 7′
C, or *Dhawalagiri*, N. 17° 13 39′ E., and elevation 1° 18′
D, or *Seitgur* (Nypal.) N. 26° 13′ 39 E., and elevation 1° 15′
E, bearing N. 34° 43′ 39″ E., and elevation 1° 6′.

From station No. 1, I proceeded to the north side of the town of *Bánsí*, which I shall call station No. 2, the latter station bearing from the former (allowing for magnetic variation) N. 14° 30′ E., and distant as inferred from survey (protracted on a scale of two miles to the inch) $25\frac{1}{10}$ British statute miles. From hence the bearings (allowance being made for magnetic variation as before) and elevations are as follows, viz :

A, bearing N. 6° 46′ 39 E., and elevation 1° 41′ 15′
B, N. 9° 3′ 39″ E., and elevation 1° 45′
C, N. 18° 1′ 39″ E., and elevation 1° 56′ 30
D, 29° 8′ 39″ and elevation 1° 53′
E. N. 36° 47′ 39′ E., and elevation 1° 33′.

At this station (No. 2,) on the 3rd November at day-break, the *Himálaya* displayed an exceeding white appearance, and the sun's rays passing through a red cumulo-stratus cloud, coloured

the snowy peaks on their prominent parts only, with a most beautiful pink tint. This took place a long time before the sun rose with us.

The first aggressions of the *Goorkahs* commenced in this province, by murdering some of the police, a T'hanadar and two or three others, who had been but a short time previous to the catastrophe in attendance, with me, to point out the boundaries of their respective districts. The *Goorkahs* were at this period posted near to our route, in defiance of the division of the army, which had been some time assembled under the command of General Sulivan Wood at *Gorrukpoor;* the alarm created among my people, owing to our vicinity to the enemy, was so great, that it was scarcely conquerable by the utmost persuasion. A curious instance of [illegible] as well as superstition of the enemy was current here, said to have occurred in this neighbourhood within a month. A part of the *Goorkah* force came to the banks of the *Rápti* river (which runs close by this station) and sacrificed a pig, as a propitiatory offering for success in the existing war; and on relating it to Colonel Fagan the adjutant-general of the army, he informed me, that upon the determination of the *Goorkahs* to war with us, they, horrid to relate, sacrificed in the mountains, a human being, as an offering for success to their arms.

From the station No. 2, still proceeding northward, I arrived at station No. 3, (situate near to the village of Mahá-Déva-Diúriya,) bearing from No. 2 (corrected for magnetic variation), N. 8° E., and distant from the same (as inferred from survey) 16 British statute miles. From hence the snowy peaks bear and elevate as follows, viz.

A.	N. 6° 48′ 39″ E.	elevation 2° 13′ 30″
B.	N. 9° 22′ 39″ E.	elevation 2° 16′
C.	N. 20° 47′ 39″ E.	elevation 2° 32′
D.	N. 32° 32′ 39″ E.	elevation 2° 26′
E.	N. 41° 16′ 39″ E.	elevation 2° 1′

At this station was distinctly heard the evening and morning guns of the *Goorkah* forces in the neighbourhood; and the alarm created among my people increasing, induced me to bend my course westward, for a base line parallel to the mountains, to ascertain their horizontal distance; which, in three days, brought us to the village of *Biskúr*, or station No. 4, bearing from station No. 3, (corrected for variation as before), S 77° 46′ 21″ W., and distant 24 § British statute miles. From this station No. 4, the snowy peaks, corrected for variation, bear as follows:

A.	N. 20° 43′ 39″ E.
B.	N. 23° 5′ 39″ E.
C.	N. 31° 55′ 39″ E.
D.	N. 42° 26′ 39″ E.
And E.	N. 49° 13 39″ E.

The distances of the aforementioned five peaks resulting from trigonometrical measurements, (and contained in a former paper, which I had the honour of presenting to you,) approximate to those distances, which result from the intersection of rhumb lines of the bearings of the peaks from the stations Nos. 3 and 4.

I remain, &c.

B. Blake.

Computation of the Height of Dhawalagiri and other Mountains.

MOUNTAIN.	Station.	DISTANCE.		Contained arc.	Altitude observed.	Corrected for refraction.	Angle subtended.	Elevation inferred.	MEAN.
		miles.	feet.	° ′ ″ ‴	° ′ ″ ‴	° ′ ″ ‴	° ′ ″ ‴	feet.	
A.	1	126¾	669240	1 50 26	1 5	0 55	1 50 21	21520	
	2	101½	535920	1 28 26	1 41 15	1 33 13	2 17 26	21449	21635
	3	85¼	452760	1 14 42	2 13 30	2 6 42	2 44 3	21636	
Chandragiri .	1	129⅓	681780	1 52 28	1 7	0 56 46 33	1 53 0 33	22435	
	2	103⅙	548460	1 30 30	1 45	1 36 46 20	2 22 1 35	22686	22607
	3	88⅛	465300	1 17	2 16	2 9	2 47 30	22699	
Dhawalagiri, or	1	139	733920	2 1 0 40	1 18	1 7	2 7 30 16	27255	
Ghasa-cótí	2	113¾	600600	1 39 7	1 56 30	1 47 29	2 37 2 30	27430	27615
	3	98	517440	1 25 23 10	2 32	2 24 14 15	3 6 55 50	28161	
Swetaghar, or	1	131½	693000	1 54 21	1 15	1 4 36 16	2 1 46 46	24576	
Nepal . .	2	106¾	563640	1 33	1 53	1 44 32 44	2 31 2 44	24785	24861
	3	91¼	484440	1 19 57	2 26	2 19 20 15	2 59 18 45	25223	
E.	1	136⅝	721380	1 59	1 6	0 55 11	1 54 41	24092	
	2	113½	599280	1 8 53 30	1 33	1 24 0 35	2 13 27 20	23292	23708
	3	99	522720	1 26 15	2 1	1 53 9 33	2 36 17 3	23740	

Art. III. *Remarks on Marine Luminous Animals.* By J. Mac Culloch, M.D., F.R.S., &c.

[Communicated by the Author.]

In my work on the Western Islands of Scotland, I had occasion to take notice of the causes which produce that beautiful appearance of light in sea water, so well known to seamen, and to all indeed who have been in the least conversant with the sea during the darkness of night. I there attempted to prove, that if, in every case it did not arise from the action and properties of living animals, but was sometimes owing to the luminous matter of fish dispersed through the water, yet that all the most conspicuous appearances of this nature were produced by these, and that the brilliant sparks of light, in particular, were always to be traced to some of the vermes or insects, which abound in the waters of the sea.

I have also given a list of such of these animals as had, by various naturalists, been found to possess this remarkable property; and had occasion to lament how circumscribed it was; partly owing to the deficiency of observers in this department of Natural History, and partly owing to unfounded theories respecting the nature and causes of the light of the ocean; in consequence of which, those who possessed the opportunities of extending this examination, had neglected it. I have also observed that many animals either very minute, or absolutely microscopic, and invisible without the use of a lens, existed in the sea; and that the neglect of these more obscure creatures, had probably been one reason why the property of emitting light was referred to the water itself, when it was, in fact, owing to these unsuspected animals existing in it.

The further investigation of this department of Natural History, was, in that essay, recommended to those who might have opportunities of pursuing it, as the subject had not at that time practically engaged much of my time, being occupied by geological pursuits requiring undivided attention, and every leisure moment of the night being employed in registering the observations of the day. But as it is not often that observers feel

much interest in pursuing a track which has been laid down by their predecessors, unless perhaps for the purpose of controverting or disputing the principles or facts on which it has been grounded, I thought it right to make use of such further opportunities as might occur towards the accumulation of new matter on this subject, and towards confirming the opinions stated in the paper to which I have alluded. A voyage to the Shetland and Orkney Islands afforded these opportunities; and the result has been to confirm the former views, by a series of observations carried on daily for many weeks. By these a large addition has been made to the list of luminous animals which was given in that essay; and it has been, in particular, proved, that the sea is very often crowded with worms and insects, often nearly invisible; and that the luminous property of the water, not only bears a relation to the existence and numbers of these at any time, but may almost always be traced to the individuals by which it is caused.

Those who are acquainted with this obscure and much neglected department of Natural History, will not be surprised to hear that I cannot at present give names to the numerous individuals which I examined for this purpose. Among them are many objects, of which, not only the names are doubtful, but the very genera, and even the analogies, obscure or uncertain. Many are absolutely unknown, and constitute new species which it will be my business to describe hereafter, when all the requisite comparisons have been made. For the present purpose, it is as unnecessary, as it would be impossible, to enter into details of so extensive a nature as would be required for assigning the names of the various animals in which I have now observed the property of emitting light, in addition to that list which was given in the essay to which I have here referred.

It will not be useless to those who may be inclined to pursue the same train of investigation, to describe the means which I adopted for examining the animals in question; while it will further the purpose of explaining the species of evidence by which I was satisfied respecting the nature of the objects which

were examined, and more particularly respecting their powers of yielding light. If there is any deficiency in the nature of this proof, as it relates to some of the more minute animals, there will still remain a considerable number to add to the list formerly given.

It must in the first place be remarked, that the whole of these observations were confined to spaces in the sea never extending above 8 or 10 miles from land; and that they were very generally made in harbours. They cannot in fact be made at sea; at least in a small ship, unless it is smooth water: as the agitation of the water under examination, no less than that of the observer's person, renders it absolutely impossible to catch and detain the objects before a lens in such a manner as to examine or delineate them. At all times, even in harbour, it is sufficiently difficult from the motions of the animals themselves, to obtain such views of them as to satisfy ourselves respecting the nature and characters of those which are minute, and of which the greater number are exceedingly restless and rapid in their movements.

Although a great many of the animals which fell under my notice, were found at the distances from land which I have just mentioned, many were only discovered in harbours, and, nearly at all times they were far more abundant in these situations than in the open sea. Some of them, it is true, seem to disregard boisterous weather; but there were many which almost invariably disappeared on the coming in of a fresh gale, and only re-appeared when the weather moderated. Other changes of weather or wind, often caused them in the same manner to disappear in the course of a few hours. It is probable that these animals, like the leech, are very sensible to atmospheric changes, and that they retire to deeper water to avoid that agitation, which, to many of the larger, would be fatal, from the tenderness of their texture and from their bulk. Many are probably destroyed by the violence of the sea at the surface. These are hints which may be of use to any naturalist inclined to enter on this department of his pursuit, while they assist in explaining the variations to which the luminous property of the ocean is sub-

ject; and the addition of a few more will not be misplaced to those who have not had any experience in these investigations.

These animals always abound most, with few exceptions, in the smallest harbours, and, more particularly, in narrow creeks, among rocks or under high cliffs, where the water is sheltered from the sea and wind, and where it is consequently seldom so much disturbed as in more open places. A large proportion of them indeed seems to be exclusively limited to situations of this nature, being never found in the open sea nor far from shore. Many of the minute marine animals also appear to affect exclusively those shallow and rocky situations where sea weeds abound, and which are equally the favourite haunts of many larger species, such as nearly the whole tribe of crabs, and many others which it is unnecessary to enumerate.

It is in such places then, and at such times, that is, in narrow and rocky creeks or weedy shoals, and in calm weather, that the naturalist will meet with most success; and it is in such circumstances also that the water will be found most luminous.

That it does not always appear luminous in calm weather, and when the vessel is quiet at anchor, is however certain; and it is this which has conduced to mislead observers respecting the causes of the light, as well as to lay the foundation of fallacious prognostics regarding the weather. It requires agitation to elicit the light of these animals in abundance; and as this naturally happens in troubled water, they have been supposed to abound in gales of wind and in a breaking sea, when they are, in fact, comparatively scarce. In calm weather, crowds of medusæ or other very luminous species, will often be floating around, yet betraying themselves only by an occasional twinkle; when any disturbance communicated to the water is sufficient to involve the whole in a blaze of light.

I formerly remarked, that the luminous action was voluntary; and this opinion has been amply confirmed by further attention to the animals possessed of this property. Among millions of these, of numerous species, the usual actions of locomotion will be performed for hours, or for a whole night, without the slightest indication of their presence; or perhaps some individual will

give an occasional spark as it passes by, when the dipping of an oar, or the drawing of a bucket of water, is sufficient to render the whole around luminous.

It is by such a test as this, therefore, that a naturalist will be guided in his pursuit after these animals. But it is proper to remark that it is often very difficult to take them, even when we are certain that they abound in the water; and this cause, like others, has often made it to be supposed that the water itself possessed a luminous property, because no animals appeared in a bucket when filled with it. A few bright lights produce a considerable effect in the night, so as to make the sea appear much fuller of sparks than it really is; and it is easy for a body so small as the ship's bucket to miss the animals by which they are produced. Moreover, as many of these creatures, and particularly the medusæ, swim near to the surface, they are apt to slip out with the wave which is produced by lifting the bucket out of the water; so that it sometimes requires many attempts before one can be secured.

There is another circumstance which is also an occasional source of error respecting the existence of these animals in the water when brought up; even when it is highly luminous alongside the vessel. Whether from fatigue, or from caprice, or from some voluntary efforts for an unknown purpose, they often refuse to show their light, even when violently agitated or injured; and, in all cases, when they have been compelled to shew it for a few seconds by violence, they again become dark and refuse to shine any longer. It is not unlikely that this is the effect of exhaustion; because after a repose of some little time, a fresh disturbance often causes them to give light again. A naturalist, unaware of this circumstance, may often imagine that he has failed in procuring specimens, even when the bucket is crowded with them.

Another circumstance leads to deceptions in these cases. In many of the luminous worms and insects, the spot of light appears much larger, if it is not really so, than the body of the animal; and very often a species which is invisible under ordinary circumstances, or only to be seen by bringing it opposite

to a bright light in a glass of water, will yield a very brilliant and large spark. Thus, in a ship's bucket, or a basin, it would not be conjectured that any animal existed, when many thousands are present; and, of these, perhaps the greater number, if not all, highly luminous.

It is, lastly, necessary to remark, respecting the size of these animals, as just mentioned, that many of the luminous species are absolutely, and under all circumstances, except when in the act of emitting light, invisible to the naked eye. This effect arises in some measure from the actual minuteness of many, their size not equalling the 100th of an inch; but in many others which subtend a visible angle, it proceeds from their transparency. Even under favourable circumstances, as when placed in a glass of water, where the vision is aided by the magnifying power of this species of lens, they cannot easily be discovered; owing to the water in which they abound being invariably muddy. Those only come into view which approach so near to the fore part of the glass as materially to diminish the column of water between them and the eye; and thus also they often escape observation, and the spectator is surprised to find that he can discover nothing in the light, when the water, in the dark, has abounded in luminous sparks. If the lens is used, it is still only in the observer's power to get sight of those which pass across its focus; so that he is, in this case also, apt to underrate their numbers, or, if rare, to doubt their existence. It is fruitless to attempt to bring them under the eye by using a small drop of water in the manner adopted in microscopic observations; as, even where most crowded, they bear so small a proportion to the water in which they swim, that such a drop may not possibly contain one.

These then are the most important circumstances which the naturalist should have in view in investigating the water of the sea for the purpose of discovering the minute animals which exist in it; whether for the purpose of ascertaining their luminous quality, or of examining their nature and structure. An attention to these cautions will probably assist others, as it did myself in these examinations; and induce them to believe what

seems to me fully ascertained, namely, that luminous animals abound in the water of the ocean even when they are least suspected, and that the property of emitting light is probably granted to every one of these neglected inhabitants of the

When the numbers of these animals are considered, it will appear less extraordinary that the water of the sea should be so generally luminous; and, when we attend to their minuteness, it is as little cause of surprise that they should escape ordinary observation. Having necessarily reserved the description and names of the species for future communications, partly for the reasons already stated, and partly because they could not be rendered intelligible without drawings; I shall not enter on this part of the subject, but merely attempt to convey an idea of the numbers of some of the most remarkable individuals which were examined.

In proceeding from the Mull of Cantyre to Shetland, with beating winds nearly the whole way, it is easy to understand that an immense tract of water must have been passed over. Those whose memory can so easily refer to the map of Scotland need not be told of the number of square miles which a vessel must traverse in this navigation. With very little exception throughout all this space, and in every one of the harbours of Shetland and Orkney, the water was full of one species, in particular, of an animal which I think is not yet described. It scarcely ever quitted the vessel, although more abundant in some seas than in others. On a very moderate computation a cubic inch did not contain less than an hundred individuals; and as they were brought up from all depths to which the bucket could be sent, it is useless to attempt a statement even of those which must have been contained in a few cubic feet, much less in the enormous mass of water thus examined. Their numbers, even in a superficial mile, supposing its depth not to exceed a few inches, baffles all imagination. This species was barely visible by the naked eye, when viewed in a glass against the light of the candle or of a moderated sunbeam.

In the same seas, and nearly at all times, the water was found filled with several different species, resembling in size some of the infusoria, and invisible without the lens. To estimate their numbers is equally impossible, but no body of water so small could be brought into a proper situation without being found filled with them. Other animals of larger dimensions, and of many species were equally constant; and, if less numerous, yet ten or twenty were always to be found within the space of a common tumbler glass.

In all these cases the water was luminous; and, that it was rendered luminous by these animals, admitted of no doubt, because the larger individuals could be taken out on a dry body, shining at the very moment of their removal, and then replaced for examination in water; while the light of the whole of these species disappeared when they died, either from keeping the water too long, from warming it, or from the addition of spirits. The facility with which the luminous quality of sea water is destroyed by those means which kill its inhabitants, is in itself a sufficient proof that the cause of this property resides in these.

I must further add, that it is perfectly easy to distinguish the different sparks of light given by different animals; that is, as far as they differ in dimensions; as the bright spot is quite distinct in the larger kinds, in which it also often varies in colour; while, in the smaller, agitation produces a general luminous appearance, in which separate spots, or the distinct action of individuals, is not to be recognised; it is probably therefore rather from this source, namely, the crowd of microscopic worms and insects, that the general luminous track produced by a fishing line, or the faint sheet of light elicited by the dash of an oar, is caused, than by the detached secretions of fishes, or by decomposing animal matter diffused through the water; while the brighter separate sparks arise from the larger kinds, to the size of which they are more or less proportioned. It will in the same way, be found, that the predominance of bright sparks in the vicinity of sea weed, or near rocks, arises from the great number of species, Squillæ, Scolopendræ,

Nereides, and many others, which make these places their exclusive residence.

It is now necessary to point out the method used in examining these animals, and deciding on their luminous powers.

With respect to the larger kinds, there is no difficulty; the smaller require many more trials; and where more than one species persist in occurring together, some uncertainty must always remain. Yet where a property is, in so many instances, ascertained to exist, and where it has probably been conferred for the specific purposes formerly noticed in the essay to which this communication must be considered as an appendix, it is not a rash conclusion to consider that no species is exempt from the general law or deprived of this power; since in the most essential circumstances, the habits of all are the same.

These animals, whether the smaller vermes or insects, are very rarely found in clear water, and wherever they are abundant it is muddy, or rather fouled with some animal matter which communicates to it a slight milky hue; although they are not, on the contrary, necessarily present when the water is in that state. It is preferable to examine the water by candle-light, as ordinary day-light is not sufficient for the purpose; and the light of the sun cannot easily be received in such a manner as to be endured by the eye, and, at the same time, to serve the purpose of illuminating the objects. It is desirable to use more than one candle, as it is convenient to have more than one luminous spot under command; the rapidity of the motions of most of these animals, carrying them so quickly beyond the limits of one spot, as to cause considerable trouble to the observer, who has many things to distract his attention at the same time. Some of them are better examined in the brightest light; others at its borders; and, very often, it is necessary to examine the same object in different lights before a just idea of its form can be obtained. A separate light is also required to illuminate the paper on which they are to be drawn; the eye being so far paralyzed by the excess of light required to view them, as not to see in a moderate degree of illumination, and it being absolutely necessary to draw them, without losing the least prac-

ticable interval of time after viewing them through the lens. A few seconds are sufficient to cause the observer to forget the exact figure of the parts which he is to delineate.

The most convenient receptacle in which they can be placed for examination is a rummer or conoidal glass, of such dimensions as to contain half a pint. It is, in the first place, quite necessary that they should be at liberty: as it is only when in motion that many of them can at all be discovered, and as the peculiar nature of their motions, which, in all, are very different and highly characteristic, is of great use in discriminating individuals otherwise much resembling each other. It is true, that this is productive of great inconvenience, from their passing so quickly out of the field of view; and thus it often requires a long time and patiently repeated examinations, to ascertain the exact figure of one individual. But it is impossible to confine them in a drop of water, unless when absolutely microscopic, without losing sight of their forms. In this case, they come to a state of rest; and their fins, legs, antennæ, or other fine parts, become invisible, generally collapsing close to the body. Moreover the affection of light produced by the contact of the animal with the surface or edge of the drop, or of that of the drop with the glass on which it stands, totally destroys distinct vision, and renders their forms quite unintelligible. A glass of smaller dimensions, such as a wine glass, is far less convenient than that abovementioned; as the smallness of the convexity produces a much less useful spot of light.

In many cases, where, from excessive activity, it is difficult to catch these objects in the field of view for a sufficient time to study their parts, I have found it useful to diminish their powers of motion. This may be done by slightly warming the water, by suffering it to stand for a few hours in the glass, or by the addition of a small quantity of spirits, and probably of other substances. But slight injuries are sufficient to kill them; and, as they then become invisible, the observer must be on his guard not to exceed in the application of these means.

From the necessity of using a large glass, and the freedom of motion thence allowed, it is evident that a high magnifying

power cannot be applied. It is scarcely possible indeed to make effective use of one greater than that produced by a simple lens of half an inch focal distance; and as, with this power the field of view is very contracted, it is often convenient to have two other lenses at hand of one inch and of two inches in focal distance. The very minute ones may be occasionally secured in a single drop of water under a compound microscope; but the observer will be disappointed much oftener than he will succeed in his attempts to examine them in this way; partly from the chance of his failing to find any in many successive small portions of water thus separated, and partly for the reasons just stated.

I have already mentioned almost all that occurs on the method used in determining those species which were luminous. Of the larger kinds, it seldom happened that more than two or three, sometimes not more than one, was contained in a tumbler. Being placed in the dark, and stirred with the finger, the same number of sparks were produced; and whatever failure might here have occurred in one trial, was removed by others made at different times. With regard to the smaller species, it sometimes happened that only one was found on a particular occasion, and the luminous state of that water on agitation proved the property to exist in that individual species. Respecting some of these species, however, doubts may remain; as in some cases no one of them was found alone. But these doubts are of little consequence; since if among so many animals resembling each other in their general characters, and often indeed apparently belonging to the same genus, the luminous property was certainly proved to exist in some, it probably existed equally in all; as there seems no reason to exclude any, or to suppose it especially possessed by one. On this subject, however, other naturalists must be allowed to judge for themselves; and those who are inclined to pursue the same train of investigation will probably complete the evidence respecting some where it is here left doubtful.

I may now therefore conclude this subject by remarking, that, from the investigation of last summer, I have added upwards

of 190 species to the list of luminous marine animals. I have already stated the reasons why I cannot as yet give even the names of many of these, of which a considerable number are certainly new, or nondescript animals. That subject must be reserved for a different species of communication; but I shall here add at least the generic names of those possessed of luminous properties, of which the genera are known; since, even in these, some of the species are still unsettled and many are new.

Among these, the most conspicuous are about twenty small species of Medusa, in addition to those already known to be luminous. In the ancient genus Cancer, a considerable number of Squillæ were also found possessed of this property. In the genera Scolopendra and Nereis five or six were luminous, being all the species that came under my observation. Of the remaining known genera in which luminous species were observed, I shall forbear to give any numerical account, but simply add that they consisted of Phalangium, Monoculus, Oniscus, Iulus, Vorticella, Cercaria, Vibrio, Volvox. To these I may also add, among the fishes, a new species of Leptocephalus. The rest consisted of new genera, or, at least, of animals which, for want of correct descriptions and of figures, cannot be referred to any as yet to be found in authors, and of which I trust at some future period to give those drawings and descriptions which are in my possession. It is sufficient for the present purpose to have shewn that the list of luminous animals is very extensive, and to have given this notice of the means used in investigating this object, together with such hints as may be useful to others; little doubting that their labours will ultimately prove this beautiful and remarkable property to be possessed by every one of the inhabitants of the ocean.

But I must not conclude this paper without noticing a circumstance which confirms the opinion stated in the former essay respecting the residence of many fish in depths which, according to Mr. Bouguer's observations, must be supposed inaccessible to the light of the sun; and in which, without that afforded by their prey, it is difficult to understand how they

can find their food. It is remarked by the Shetland fishermen, that the ling invariably inhabits the deep valleys of the sea; whereas the cod is always found on the hills, general known by the name of banks. In one of the most productive spots for the ling fishery, the valley which they inhabit is not only very deep, but is bounded by abrupt land or submarine hills nearly precipitous; the water suddenly deepening from 20 and 30 to 200 fathoms. In this, as well as in other valleys in which this fishery is carried on, always very far from the shore, it is found that the best fishing exists at the greatest depths, and it is not unusual to sink the long lines in water of 250 fathoms depth. The time required in setting and in drawing up from this depth, the length of line used, which amounts in some cases even to seven miles, is such as to prevent the fishermen from making any attempts in deeper water; but they are all of opinion that this fish abounds most in the deepest places, and might advantageously be fished for at much greater depths. Now allowing even 1000 feet instead of Mr. Bouguer's calculation of 723, it is plain that no light can exist in these valleys, and that the ling, like other fish which prey in the deep seas, must have some means of seeing his food, as well as of pursuing his social avocations of whatever nature these may be. This can only be effected by the luminous property, either of his prey, or of the animals which abound in the sea, or else by that elicited from his own body.

J. Mac Cullocii.

Shetland, August, 1820.

ART. IV. *A Translation of* REY'S *Essays on the Calcination of Metals*, &c.

[Communicated by JOHN GEORGE CHILDREN, Esq., F.R.S., &c.]

Continued from Page 83.

ESSAY IV.

Air and Fire have weight, and naturally descend.

HAD we as free a commerce with the elements of fire, as we have with the air, we should doubtless, be furnished with experiments, to confirm our assertion. True it is, that those

which we shall produce with regard to the latter, will be conclusive as to the former, from the proximity of their nature*. Now since it is agreed, that whatever falls downwards of its own accord, has weight, whence that motion proceeds, who is he, that shall deny this quality to air, seeing that we no sooner pull a stake out of the ground, than the air rushes into the hole, and fills it; and that we cannot dig a well so deep, that it does not immediately descend into it, without any external effort or violence? I say more: that, if there were a tube from the centre of the earth to the region of fire, open at both ends, and filled with the four elements, each in its usual position, if the earth were drawn downwards, water would descend and occupy its place, leaving its own to the air, and the air its place to fire. Then, taking away the water from this station, the air will come and fill it, and that again being taken away, fire will go into it, and fill the whole tube, descending to the very centre, merely by being deprived of that, which prevented it from doing so. They who shall say, that this happens, that a vacuum may be avoided, will not say much; they will shew us the final cause, whilst we are talking about the efficient, which cannot be a vacuum. For it is quite certain, that in the boundaries of nature, a vacuum, which is nothing, can have no place. There is no power in nature of nothing to have made the universe, nor to reduce it to nothing, which requires equal force. But the case would be otherwise, were there a vacuum, for if it could be here, it might be there, and if here and there, why not elsewhere? Why not everywhere? So might the universe fall into nothing, by its own power: but to Him only, who had power to make it, is the glory of the power to annihilate it due. But if a vacuum can find no existence, how can air and fire descend, full against the course of their nature? Does not a positive effect, always proceed from a positively existing cause? we truly affirm then, that it is weight which carries these bodies downward, in order to unite all their particles closely, and consequently shut all the avenues to a vacuum.

* *Literal.*

Essay V.

It is demonstrated that air and fire have weight, *by the greater celerity with which heavy bodies move, toward the end, than at the beginning of their motion.*

An error, however small, committed in the beginning of any *doctrine*, increases as we proceed, and most commonly leads to very * *serious* difficulties. We experience it in regard to this subject, for philosophers, having gone astray, almost on the very threshold of natural science, ascribing levity and upward motion to the two superior elements, saw themselves afterwards much troubled to account for the natural descent of heavy bodies being quicker towards the end than at its commencement. The variety of opinions that we find in authors on this question, sufficiently demonstrates their perplexity; I, who study brevity, have no intention to bring them forward; they who like it, may read a good number of them, in the "*Natural Principles*" of Pererius†, a judicious philosopher, in which, after having quoted, he learnedly refutes them, and embraces one that he professes to acquiesce in, till he finds a better: of this I shall say something hereafter, as we go along, to shew that it is not so true as it is plausible. I now present my own, which I have with much study devised, in support of the preceding demonstrations. The quickness of motion of a heavy body increases from the beginning to the end, by the increase of elementary matter which presses on it, and by the continual multiplication of the impulse which it gives it in its descent. A figure will make my assertion clearer, (see fig. 1.) Let A A be the heavens; B B the earth; C its centre; D an iron ball, descending towards the earth; E the same descending lower; F the same again, in the middle of its descent; G the same, near the end of it; H H two lines, drawn from the centre of the earth, to the heavens, and touching the ball at D in the two extremities of its diameter; I I two similar lines, touching the ball at E; K K, two

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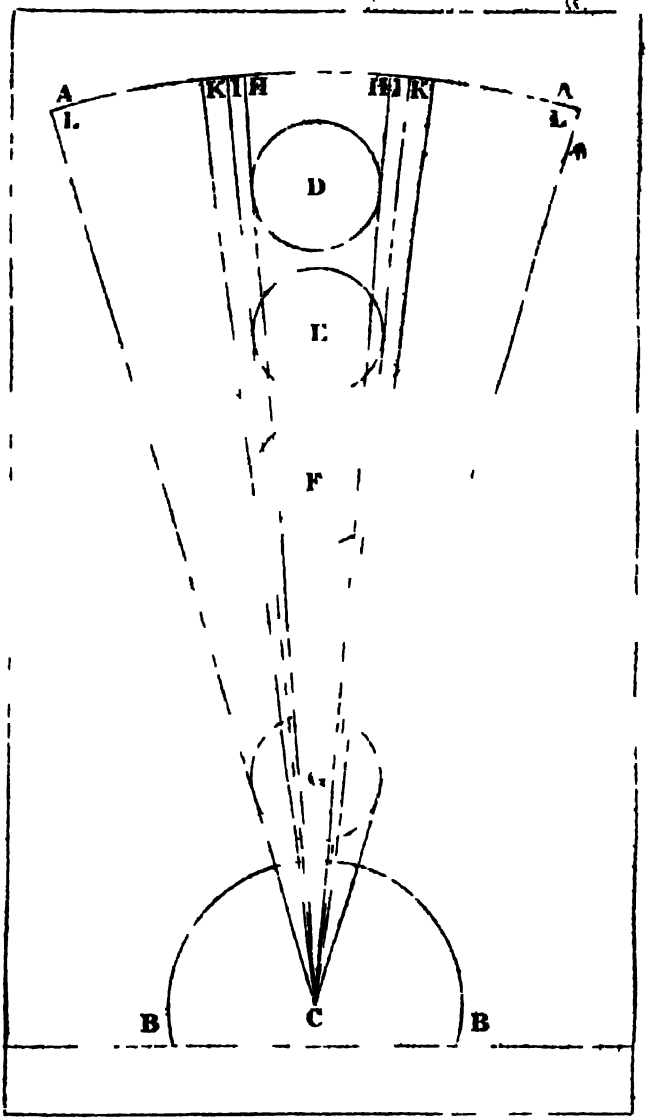
* Literally "*thorny*," "*espineuses.*"

† Benedicti Pererii de communibus omnium rerum naturalium. principiis et affectionibus. *Libri* XV. in 4to. Parisiis, 1589.

Others touching it at F; and L L, two others touching it at G. It is obvious, that the ball being at D, has upon it, besides its own internal weight, the matter of the elements of air and fire, contained between the lines H H; but when at E, there is



all the matter contained between the lines I I, which increases at F by the greater quantity contained between the lines in KK; and when at G the weight of the whole, contained between the

lines L L presses on it, whence the quickness of its motion must increase; to which is to be added the impulse which this matter is continually giving it, as it still keeps falling down on the ball. The opinion of Pererius somewhat resembles this idea of an impulse; for he thinks that the air which follows pushes the ball; but he is mistaken in this, that air being light, and naturally tending upwards, cannot push the ball downwards, any more than a boat, towed against the stream is impelled up the river by the water, which meeting the prow, divides, and passing the sides runs continually downwards; for how can it, following this course, strike the* stern above? The other part of his assertion is no better, contending that the air, agitated by the motion, yields more readily to the thing moved. It is just the reverse, for air and water, when agitated, are capable of supporting larger weights. Ashes are suspended in water, and feathers in the air, when they are agitated, and fall down when the fluids are at rest. Surely, according to this reasoning, the motion should be slower towards the end, the agitation being then greater.

Essay VI.

† Gravity *is so intimately united to the first matter of the elements, that when they change from one into another, they always retain the same weight.*

My principal object, hitherto, has been to fix in the minds of all, the persuasion that air has in itself a principle of gravity, since it is from this that I purpose to derive the increased weight of tin and lead when they are calcined. But before I shew how that happens, I must extend my observation, and add, that the weight of any body is examined in two ways, by reason or by the balance. It is by reason that I have found weight in all the elements; it is reason which now induces me to deny that erroneous maxim, which has obtained from the birth of

* Frapper en haut la pouppe.

† *Pesanteur*, I have translated this word by the term *gravity*, though believe it was not used in this sense before the time of Newton; in manner *peser*, is generally rendered *gravitate*.

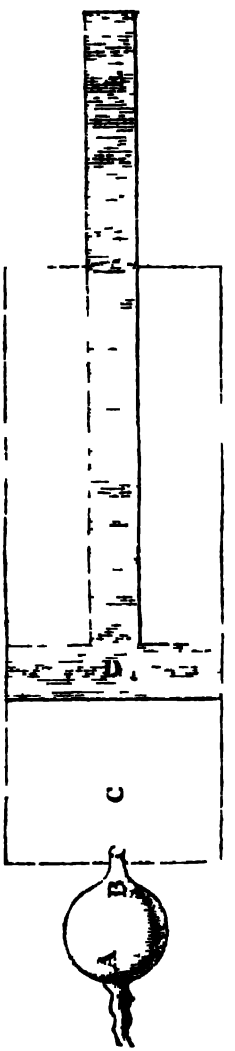
philosophy, that the elements, by mutual conversion from one into another, lose or gain weight, in proportion as they are rarefied or condensed, in the change. Armed with this reason, I boldly enter the lists, to combat the error; and I maintain that gravity is so united to the first matter of the elements, that it cannot be deprived of it. The weight that each portion receives in its cradle, it carries to its coffin. In whatever place, under whatever form, to whatever volume it be reduced, always one same weight. But not presuming that my assertions will rank with those of Pythagoras, and that it will be sufficient to have advanced them, I support them by a proof in which I think all liberal minds will acquiesce. Take a portion of earth possessing the least possible weight that can be conceived; let this earth be converted into water, by the means known and practised by nature, it is evident that this water will have weight, because all water must have it: now it will be greater or less, or equal to that in the earth. Greater, they will not say it is, (for they profess the contrary,) and I also deny it; less it cannot be, seeing that we have taken the least possible quantity: it remains therefore that it must be equal to it, which is what I intended to prove. What is demonstrated of this portion is demonstrable of two, three, or any, however great number, in short of the whole element, which is composed of nothing else, and is equally referable to the conversion of water into air, and air into fire, and *vice versâ* of these last into the others.

Essay VII.

How to ascertain to what volume of air, a certain quantity of water is reduced.

Philosophers have often talked of the extension which a solid element acquires, by conversion into one more rare, and have attempted to assign its proportion: but I do not remember to have read any thing supported by *sound* reasoning or experiment. Now since in the preceding essay, I have spoken of this enlargement, the knowledge of which opens the door to many beautiful and admirable devices, I will not deprive the curious reader of a means, which I have thought of, to make

the trial, and accurately ascertain to what volume a certain quantity of water can dilate itself, by transmutation in which experiment may serve for, and be proportionably referred to other elements. Make a brass tube of convenient size, well polished within, open at one end, and closed at the other, except a very small hole in the middle; place on the inside a piston or stopper, like that of a syringe, that may slide easily through every part, and be so correctly fitted, that no air can escape; the piston being slided to the bottom, let the tube of an æolipile, or philosophical bellows, be applied, and closely fixed to the small hole. Fill the æolipile with water, and set it on the fire; then the water becoming rarefied, and converted into air, will pass out through the little hole, and entering into the tube, in search of its liberty, push the piston by degrees, till all the water is converted into air. The capacity of the tube and æolipile, which will both be filled with it, will shew the increased bulk which this matter has acquired. Whoever wishes to know the same more easily but not so accurately, may take the intestine of a pig or other animal, and having well cleaned and flattened it, and emptied it of air, let him put it into a vessel full of water, accurately closed by a lid, having a small hole above, to let the water run out: let one end of the intestine, projecting out of the vessel by a hole on one side, be fastened to the tube of the æolipile, which, filled with water and placed on the fire, will blow into it the air, into which the water will be converted; in proportion as the intestine swells up, the water of the vessel will flow out, by the little hole in the lid, which, when col-



lected, will show the dilatation of the air in the intestine, to which the capacity of the æolipile being added, the question is solved. To these well-ascertained methods, I add the following, not unplausible one, for converting air into water, and ascertaining the diminution of volume. Let the hole of the before-mentioned tube, be closed, and the piston pushed down with great force, as far as the compression of the enclosed air will permit, and stopping it at that point, so that it cannot fly back, expose the apparatus to a frosty air, for a whole night; the air compressed on the inside, will freeze, or be converted into water, leaving only that space occupied by the air, which may remain free, (*i. e.*, unfrozen.) The measure of the water, or the ice, will give the loss of volume. I have not made this experiment: if any curious person is beforehand with me, I request he will give me an account of the result, as all the reward I ask for having taught him the method, and to the end that I may be *spared the trouble.*

A the æolipile; B its tube, entering the brass tube; C the brass tube; D its piston; E the piston rod.

Essay VIII.

No element gravitates in itself, and why?

I resume my argument, and say, that the examination of weights by the balance differs greatly from that by reason. The latter is only employed by the man of judgment, the *rudest clown* practises the former. This is always just, that is generally deceitful; this is attached to no circumstance of place; that commonly exercised in air, and sometimes, though difficultly, in water. It is hence, that the error which I have combated, (that air has no weight,) derives an argument capable of dazzling weak eyes, but not the clear-sighted. For weighing air in itself, and not finding it to gravitate, they have concluded that it has no weight. But let them weigh water, (which they believe to have weight,) in water itself, and they will find it equally void of it, it being most true, that no element gravitates in itself. Whatever gravitates in air, whatever gravitates in water, must contain more weight in an equal volume (in consequence of there being more matter) than the air or water, in

which the weighing is performed. I proceed to deduce the cause of this, which few persons have discovered. Whatever gravitates in air (the same may be said of water) divides it, pushes it aside, and makes it give place, in order to sink to the bottom of it. This is called exercising its force and action in air. Now it is a fact, that no agent acts in its like, all action presuppposing some difference. One hot body has no action in another equally hot, but rather the two will embrace and unite their actions, and by this union will no longer be two agents but only one. But if a very hot body acts in that which is less so, it is because in this case there is a dissimilitude, and in some respects an opposition; the less hot claiming the title of cold, with reference to the hotter. Thus air cannot act by its weight in air equally heavy, the two airs rather unite, and make one same weight. But whatever is heavier than air, by the dissimilitude and opposition, arising from this greater or less, will act in it, dividing, pushing it aside, and making itself a way through it to get to the bottom. But if air evince not its weight in air itself, on account of the equality of their weights, it follows *à fortiori* that it will not evince it in water, which is heavier. For even if it be placed below, it will descend no lower, the weight of the water above it only serving to compel it to seek a higher place, not allowing it a station under itself.

Essay IX.

Air is rendered heavy by the mixture of some matter heavier than itself.

I PURPOSE to shew that it is the air, which mixing with the calces of tin and lead, when they are calcined, increases their weight; which it would be impossible for me to do, without removing a no small difficulty that presents itself in this place. For I may be asked, how can this that I assert be, since the examination of this weight is effected by the balance, *and in air*, where air can have no weight, according to the doctrine of the preceding essay? To clear up this doubt, I say, that portions of the air may be changed and increased in weight, so that these portions, so altered and made heavier, being weighed in pure air, will afford evidence of their weight. But what is this

change which causes it to become heavier? I remark, that it may happen in three different ways; either by the mixture of some heavier foreign matter; by the compression of its parts; or by the separation of its lighter portions. Let us speak, first, of the first, and then of the two others. It is certain that the air is capable of containing many matters heavier than itself; such are the vapours and exhalations which rise from the water, or the earth. A portion of air imbibed with these matters, will weigh more than an equal portion of another air containing nothing of the kind; like as sea-water is heavier than the water of fresh rivers; the former containing much salt, which the latter is free from. Observe, I pray you, how, in cloudy weather, at first opening your high windows, the air enters your chamber loaded with fog! Do you not conclude that this weighs more than the other, since it cleaves it and falls down in it? Fill a balloon with this cloudy air, it will weigh more than the same filled with pure unmixed air. Reason accords with this experiment, saying thus; if to two equal weights, we add two unequal weights, the two weights will be unequal, and that will be the greatest, to which the greatest weight has been added. If we take, for instance, two portions of the same air, each equal to ten cubic inches, and add to one of them two inches of water, and to the other two inches of air, who but perceives that these two portions will be very equal in volume, but unequal in weight, and that the one containing the water will be the heaviest? This is so manifest, that I abstain from saying any thing more about it, especially as this mode of increasing weight, has not much to do with our subject. Proceed we, therefore, to the others.

Essay X.

Air is rendered heavy by the compression of its parts.

The second mode by which air increases in weight, is by the compression of its parts; for nature has willed, for reasons known to herself, that the elements should dilate and contract, within certain limits, which she has prescribed to them. Within this space, we see a portion of an element now narrowly contracted, now widely distended. Observe the pot, half full

of water, under which the cook is about to make a large fire. The water will dilate till it runs over the brim: but if the fire be extinguished, it will contract and return to its original bulk. Take this syringe in which the piston is pushed half way down, and the opening in front well closed; push it forcibly, you will reduce the enclosed air to a small compass. Draw back the piston towards you, and though you do not pull it out, yet you will cause the air to dilate to more ample dimensions than it had before. The air being thus compressed, do you doubt that it will have sensible weight in a free air, since it contains more matter in an equal space? If the reasons already given in the eighth essay be not sufficient for you, make the experiment. Fill a balloon with air, strongly compressed by means of a pair of bellows, you will find it weighs heavier when full, than empty. And by how much? By so much as the additional quantity of air in the balloon, weighs, in proportion more than that which is free, under an equal volume. Many have indeed remarked the greater weight of the balloon when full than when empty; but it has not come within my observation, that any one, hitherto, has known the cause of it. Leaving aside persons of low account, Dr. Scaliger who possesses the true genius of Aristotle, did not understand it; for in the hundred and twenty-first exercitation against Cardan, he follows the beaten track, holding that pure air is light, and that the balloon gains weight because the air which is next the surface of the earth, such as is forced into the balloon by the bellows, is mixed with vapours, and those little terrestrial bodies clearly discernible in the sun's rays*. But alas! what good does this mixture do him? since the experiment is made in an air perfectly similar: certainly it could evince no weight in it unless compressed. If the balloon were forcibly filled with the purest air in nature, or even with elementary fire, reason says it still must have weight, if balanced, in the first instance, in the same, and in the second,

* "Purum aërem levem esse. Inflatū utrem plenū esse aëris impuri: "sive ab homine sufflatus sit: udi enim multū vehit secū: sive à folle. "Satis enim patet, aërem hūc, qui circū terræ est superficiem, vaporibus "atque terrestribus corpusculis mistum esse: quæ in solis radiis apparent "manifestè." *Jul. Cæs. Scalig. de Subtil. ad Hier. Cardan. Exercit.* CXXI. p. 1051. Lutetiæ, M. D. LVII.

in fire itself. This compression of air, is a fertile field in which ingenious minds will collect rare devices. From hence the Sieur Marin, Citizen of Lisieux, has derived his Arquebuss, which I discovered many years since, before the Sieur Fluranse had described it*, but which far excels that of Marin, (I say it without vanity,) in having much greater force. I could acquaint the reader with another elegant and profitable invention, which I have derived from the same source, but I am purposely silent concerning it; hoping one day to have the happiness of presenting a most humble petition to his Majesty, that he will honour me with the privilege of the exclusive use of it, for a certain time, in order somewhat to reimburse me for the expense I must be at in bringing the said invention, as well as some others which till that time I keep secret, into use.

[*To be continued.*]

* This relates to an air-gun invented by Jean Rey. He quotes the work of David Rivault, Sieur de Flurance, a native of Laval on the Maine, but descended from an ancient family in Britanny, a counsellor of state, and preceptor to Louis XIII. The work is called *Elements of Artillery, &c.* 8vo. Paris, Adrien Denis, 1608. It is dedicated to the Duc de Sully, and the preface contains the history of the invention and first use of fire-arms, ancient and modern. Flurance left the service of Louis XIII, in consequence of a blow he received from the king, for having kicked a favourite dog, which was troublesome to him, whilst giving the prince a lesson. He was afterwards recalled to court, and died at Tours, in January 1616, at the age of 45.

The Editor of the reprint of Rey's essays, adds, that air-guns were discovered in France, by the Sieur Marin Bourgeois, an inhabitant of Lisieux in Normandy, whom Flurance calls "a man of most rare judgment "in all sorts of inventions, of the most artful imagination, and of consummate dexterity in handling the tools of every art known in Europe, "without having learnt of any master. He is an excellent painter, statuary, musician and astronomer, and works more delicately in iron and "copper than any other artist that I know. The king, Louis XIII, has "a table of polished steel, made by him, in which his majesty is represented to the life, without the help of engraving, modelling, or painting, "but merely by fire, which this admirable workman applied more or less "to the different parts, as the figure required to be bright, brown, or "obscure. He has a sphere made also by him, in which the motions of "the sun, moon and stars are represented. He has, likewise, invented "a musical scale, for his own use, by means of which he writes down in "a manner only known to himself, the airs of all songs, and plays them "afterwards on his viol, in concert with those who play the other parts, "without their knowing any thing of his method, or he understanding "their science." Flurance saw Marin Bourgeois' air-gun at Lisieux in 1607, and having become intimate with him, obtained the description of it, which he published in 1608. Experiments were made with the air-gun in the presence of the king and one of his ministers. "It is right," adds the editor, "to publish this anecdote, so honourable to the artist, and which secures to us the priority of the invention."

Art. V. *Contributions towards the Chemical Knowledge of Mineral Substances. By the late* Martin Henry Klaproth. Vols. IV. V. *and* VI.

[It has, we believe, been frequently regretted that no English translation of the three last volumes of *Klaproth's Analytical Essays* has hitherto appeared; in compliance, therefore, with the suggestion of several of our chemical readers, we propose to lay before them an account of the principal analyses contained in those volumes; giving sometimes an entire translation of the original, at others an abstract, and sometimes merely the results of the author's experiments; being in these respects guided by the novelty and importance of the details, and by the originality and efficacy of the manipulations. We trust that the chemical student will especially derive advantage from an acquaintance with the latest labours of this celebrated and accurate analyst.]

1. *Chemical Examination of Electrum, a native Alloy of Gold and Silver.*

The term *electrum*, commonly applied to amber, has also been used to denote an alloy of gold and silver. "*Omni auro inest argentum vario pondere: ubicunque quinta argenti portio est* electrum *vocatur* *," says Pliny; whence it would appear that the term is only properly applicable to the alloys containing excess of gold, which is the case with the *electrum* of Schlangenberg in Siberia, where it occurs native, of a pale gold colour, in small plates, imperfect cubes, &c., associated with a grey coarse-grained sulphate of baryta, and also with a splintery variety of grey horn-stone: the matrix of the specimen employed in the following analysis was sulphate of baryta. To separate any free silver or gold it was first digested in nitric acid, to which muriatic acid was afterwards added; it was then fused with borax.

A piece of the electrum thus purified and weighing 25 grains, was beaten flat and boiled in nitric acid, which exerted no action upon it; an equal portion of muriatic acid was then added, but still without effect.

b. The electrum was then fused with the addition of its weight of silver, laminated, and boiled with nitric acid, which

* Lib. xxxiii. cap. iv. sect. xxiii.

only dissolved the added silver, and left the original twenty-five grains of electrum untouched.

c. It was then melted with thrice its weight of silver, laminated, and digested in nitric acid, which now effected a complete separation, leaving the gold in heavy brown scales, which being washed, and fused into a button, weighed 16 grains.

d. The *silver* was separated from the nitric solution by the immersion of a plate of copper, and amounted to 84 grains: deducting the 75 grains, which were added, 9 grains remain as the proportion of that metal contained in the native alloy; hence the electrum consists of gold 64 grains

silver 36 . .

100

As this alloy resists the action of nitric, and even of nitro-muriatic acid, it is evidently not a mere mixture of gold and silver, but a peculiar definite ore in which those metals are chemically combined.

2. *Chemical Analysis of the Pacos, or red Silver Ore of Peru.*

I am indebted to M. von Humboldt for the specimen of the Peruvian silver ore, which is called *pacos*, employed in these experiments. When minutely examined, it has the appearance of a brown oxide of iron traversed by filaments of native silver, which is therefore easily separable by amalgamation.

a. 100 grains of this ore heated to redness lost 8.5 grains, and acquired a brown red colour.

b. 200 grains digested in nitric acid furnished a colourless solution, when filtered, from which muriate of soda threw down 37.25 grains of muriate of silver, equivalent to 28 grains of metallic *silver*. The remaining liquid supersaturated by carbonate of potassa, afforded a light brown precipitate of *oxide of iron*, which after ignition weighed three grains.

c. The portion which had resisted the action of nitric acid was digested in boiling muriatic acid; nine grains remained undissolved, consisting of seven grains of impalpable *siliceous* earth and two grains of coarse sand, amongst which minute octoëdral crystals were perceptible, consisting probably of fer-

riferous titanium, and some small particles of sulphur, which burned away with a blue flame. The muriatic solution when cold, was duly diluted and precipitated by carbonate of soda; the precipitate, collected, dried, and ignited, gave 189 grains of *oxide of iron*. The remaining fluid boiled with excess of carbonate of soda afforded a trace of *oxide of manganese*: 100 parts of this ore therefore contain,

Silver	14.
Brown oxide of iron . . .	71.
Silica	3.50
Sand, *&c.*	1.
Water	8.50
	98.

Chemical Analysis of the Hepatic Mercurial Ore from Idria.

The specimen employed in the following analysis was compact, and its colour intermediate between cochineal red and lead grey; it is opaque, gives a dark red streak, and acquires a shining surface when rubbed; it is soft, tasteless, and of a specific gravity=7.100 It is susceptible of a bad polish, and then appears of a liver brown colour.

A. 1000 grains of this ore, distilled with half its weight of iron filings, gave 818 grains of pure *mercury*. The residuary sulphuret of iron was mixed with a black powder.

B. *a*. 100 grains of the ore in fine powder were boiled with 500 grains of muriatic acid, which occasioned the evolution of sulphuretted hydrogen: 100 grains of nitric acid were then gradually added, by which the whole of the ore was dissolved with the exception of a black residue of 10 grains; this residue was carefully heated upon a porcelain capsule, so as to burn away the sulphur only: there remained three grains of carbonaceous matter, which, when more strongly ignited, left one grain of red ash.

b. The above nitromuriatic solution was precipitated by muriate of baryta, and the sulphate of baryta thus produced weighed after ignition 46.5 grains, whence it appears that 6.5 grains

of sulphur were acidified by the nitric acid: if to these we add the above seven grains which were burned, and estimate that lost in the sulphuretted hydrogen as equal to 0.25 grains, the proportion of *sulphur* in the ore may be considered as = 13.75 grains, *per cent.*

C. *a.* 1000 grains of the ore were gradually heated to redness in a retort connected with the pneumatic apparatus: after the expulsion of the atmospheric air, 34 cubic inches of sulphuretted hydrogen were evolved, besides a portion absorbed by the water.

b. The receiver contained some globules of mercury: the neck of the retort contained a mixture of moist black sulphuret of mercury, globules of metallic mercury, and some particles of cinnabar; the mercury, mechanically separable, weighed 317 grains. In the upper part of the neck of the retort was a compact sublimate of pure cinnabar, weighing 236 grains.

c. There remained in the retort a light black powder, weighing 39 grains, which, after incineration upon a capsule, left 16 grains of a brownish grey, earthy powder; hence the portion of burned carbonaceous matter may be considered as = 23 grains.

d. This earthy residue was digested in muriatic acid; there remained *silica* weighing, after having been ignited, 6.5 grains.

e. The muriatic solution, which was of a greenish colour, was supersaturated by ammonia, which produced a brown precipitate, leaving the supernatant liquid of a pale blue colour. The precipitate was digested in hot solution of potash, and there remained *oxide of iron*, which having been brought to the magnetic state, weighed two grains.

f. Muriate of ammonia being added to the alcaline liquor, *alumina* was precipitated, weighing, after having been ignited, 5.5 grains.

g. The ammoniacal liquor, after supersaturation with muriatic acid, afforded upon a piece of immersed zinc, 0.20 grains of metallic *copper.*

From the above experiments it appears, that 1000 parts of the compact hepatic ore of mercury from Idria contain

Mercury	A.		818.
Sulphur	B. *b*		137. 50

Carbon	C. *c*	23.
Silica	*d*	6.50
Alumina	*f*	5.50
Oxide of iron	*e*	2.
Copper	*g*	0.20
Water and loss		7.30
		1000.00

To the above analysis Klaproth adds some remarks respecting the state of the mercury in the red sulphuret.

Chemical Examination of the Lenticular Red Copper Ore of Siberia.

a. 100 grains of selected crystals of the above ore were digested in cold muriatic acid: the mixture became warm without effervescing; the ore lost its red colour, becoming greyish white, and the acid acquired a dark brown colour. A fresh portion of acid was added, by which the grey portion of ore was taken up, and some particles of metallic copper remained undissolved, which however ultimately disappeared on continuing the digestion.

b. On pouring the solution into water, it became decomposed, and deposited a white precipitate of sub-protomuriate of copper, which, upon adding excess of potassa, acquired an orange colour; the whole was then poured upon a filter, and the residue being washed and dried, proved to be protoxide of copper, of a rhubarb-yellow colour.

B. From these experiments it appears that the copper in the above ore is in the state of protoxide. To ascertain the relative quantity of oxygen to that of metal, 100 grains of the ore were carefully digested in a stopped phial with muriatic acid, so as to leave the metallic copper undissolved, which was collected in small crystallized particles, and weighed 12 grains. The solution, containing 78 grains of pure red copper ore, was heated to its boiling point, and nitric acid added drop by drop, till it became of a grass-green colour; it was then diluted, and after a small addition of muriatic acid, was heated and decomposed by zinc, by which the copper was separated, and after being washed and quickly dried, was found to weigh 71 grains

As no other ingredient was to be detected, it appears that the 78 grains of ore contained seven of oxygen.

If, therefore, we exclude the metallic copper, which is to be regarded as mixed and not chemically combined with the ore, the composition of 100 parts of the octoedral red copper ore of Siberia is as follows:

Copper	91
Oxygen	9
	100

Analysis of the fibrous red Copper Ore of Siberia.

This ore occurs in the Turjin mines of the Ural mountains, generally in globular concretions of dark blue crystals, which appear to be somewhat oblique four and six-sided prisms. The specimen selected for examination was coarsely powdered, and washed to separate some adhering earthy particles.

A. 100 grains ignited in a covered crucible, acquired a black lustre, and lost 30 grains.

B. Nitric acid dissolved the ore with effervescence, forming a transparent blue solution, which was not affected by the addition of acetate of baryta, acetate of lead, nor nitrate of silver.

C. 100 grains of the ore dissolved completely in muriatic acid, forming a dark green solution, which became bluish when diluted. On supersaturation with caustic ammonia, the precipitate first formed was entirely redissolved; the ammoniacal solution was then supersaturated by sulphuric acid, and a plate of iron immersed, which caused the separation of 56 grains of *copper*.

D. 100 grains of the ore were put into a counterpoised phial, containing a sufficient quantity of sulphuric acid diluted with four parts of water, and were gradually dissolved without the aid of heat. The evolution of *carbonic acid* produced a loss of weight equal to 24 grains: the solution precipitated by zinc gave 56 grains of *copper*.

E. 100 grains of the ore heated in a small glass retort con-

nected with the pneumatic apparatus, afforded about 35 cubical inches of carbonic acid gas: the intermediate recipient contained several drops of pure water. The ignited residue was black and shining, and weighed 70 grains. As this residue has already been shown to consist of pure oxide of copper, of which the oxygen constitutes a fifth part, the above 70 grains are obviously composed of 56 grains of copper and 14 of oxygen. Hence the following are the constituents of the ore:

Copper	56
Oxygen	14
Carbonic acid	24
Water	6
	100

Contrasting these results with those afforded by the analysis of malachite*, it appears that the blue carbonate of copper differs from malachite in containing more carbonic acid and less water.

Analysis of a Green Copper Ore from Siberia.

This ore occurs in the Turjin mines; it is massive, has little lustre, is translucent in thin fragments, brittle, and of a verdigris green colour inclining to sky-blue.

A. *a.* 100 parts heated to redness, lose 24 parts, and acquire a black colour.

b. It is slowly acted upon by nitric acid, evolving air-bubbles and leaving a residue of pure silica. The weight of the *carbonic acid* is 7 *per cent.*, which, deducted from the 24 *per cent.* lost during ignition, gives 17 *per cent.* as the weight of the *water.*

B. *a.* 100 grains digested in nitric acid leave 26 grains of *silica.*

b. The nitric solution, of a pure blue colour, was supersaturated by ammonia; the precipitate at first thrown down was perfectly redissolved, and a deep blue solution was obtained.

c. This solution, supersaturated with sulphuric acid and pre-

* *Beiträge*, 2 B. s. 290.

cipitated by zinc, afforded 40 grains of *copper*, which are equivalent to 50 grains of peroxide of copper. The following, therefore, are the components of this ore:

Copper	40
Oxygen	10
Carbonic acid	7
Silica	26
Water	17
	100

The silica in the above mineral is not merely mechanically blended, but forms one of its chemical components.

Analysis of the Copper Glance of Rothenburg

The compact variety of the above ore, of a greyish black colour, soft, giving a dull black streak, and of a specific gravity of 4.685, was employed in the following experiments; it also occurs at Rothenburg in hexaëdral prisms.

A. *a*. Having ascertained the absence of silver, lead, antimony, *&c.*; and that copper and sulphur are the components of this ore, 100 grains were digested in muriatic acid, the action of which was aided by the occasional addition of some drops of nitric acid, till the ore appeared decomposed. The liquid was then poured off, and the remaining *sulphur* having been digested in a fresh portion of muriatic acid, was washed and dried: it weighed 22 grains, and burned away without any appretiable residuum.

b. The colour of the solution, at first brown, became green and blue by dilution; the addition of ammonia occasioned a precipitate perfectly redissoluble in excess of the alcali, with the exception of some brown flocculi of oxide of iron, which were found equal to one-half grain of metallic *iron*. The ammoniacal liquor supersaturated by sulphuric acid, and decomposed whilst warm by the insertion of a plate of iron, gave 76½ grains of metallic *copper*.

B. 100 grains of the same ore were reduced to powder, and distilled to dryness with six parts of dilute sulphuric acid (com-

posed of two parts of concentrated acid and one of water). The residue was digested in water, and a plate of zinc immersed in the filtered solution, caused the precipitation of the same quantity of copper as in the previous process. Hence 100 parts of this ore contain

Copper	76.50
Iron	0.50
Sulphur	22.00
Loss	1.00
	100

Art. VI. *Description of the Balance represented in* Plate V. fig. 1.

A good pair of scales is an essential implement to the chemist and mineralogist, but it has hitherto been scarcely attainable, except at a price far too considerable for the generality of purchasers. We are indebted to our friend Mr Children for the drawing of the beam represented in the above-mentioned plate, and which will, we believe, be found to possess considerable accuracy in a small and convenient compass, and at a very moderate expense.

The beam, which is of platinum and made so light as to be extremely sensible, is at the same time rendered sufficiently strong by its form. The adjustments for bringing the points of suspension of the scale pans, equidistant from, and in a right line with the axis, are performed by the screws *a* and *b* at the ends of the beam; these screws also serve to strengthen the curved ends of the beam and prevent their bending. The axis of the beam is a piece of very hard steel, of an equilateral triangular form, passing through the beam and bearing on agate planes; the knife edges are ground to an angle of 120°, which has been chosen thus obtuse from the liability of a more acute edge to be broken when suddenly lowered upon the agates. The ends of the axis are chamfered from the top to the knife edge, so that when the beam is lowered upon its bearings, they are clear of the lifting frame. An index or pointer descends from

the beam to a divided scale; upon this index is screwed the ball *c*, the office of which is to adjust the oscillation of the beam, or in other words to give it its required sensibility; should the index not exactly coincide with the middle division of the scale, it may be adjusted by turning the piece of wire *d*, at the top of the beam. The lifting frame, *e f g h*, is pressed upwards by an internal spring, its use being to lift the beam from the agates, when not in use, or when weights are put into the scale pans: this frame is lowered by the lever *i*, and is retained in that position by turning the lever into the notch on one side. At the bottom of the short stringed pan is a hook for the convenience of attaching substances whilst ascertaining their specific gravities. The whole instrument is covered with a glass case, and provided with a level for ascertaining the horizontality of the agates, and with tweezers, and a box of platinum weights from $\frac{1}{100}$ of a grain up to 100 grains. The price of this apparatus as manufactured by Mr. Robinson, of Devonshire-street, Portland-place, is £6, with the beam and weights accurately adjusted, or £4 when unadjusted.

ART. VII. *On the Magnetism impressed on Metals by Electricity in Motion; read at the public Sitting of the Academy of Sciences, 2d April, 1821.* *By* M. BIOT.

WHEN the electric current, evolved from a voltaic battery, is transmitted through any metallic bodies whatsoever, it gives them instantaneously a magnetic virtue; they become then capable of attracting soft and unmagnetized iron. This curious fact was discovered by M. Oersted. If we expose to these metallic bodies, a magnetic needle, they attract one of its poles, and repel the other, but only relative to the parts of their surface to which the needle is presented. Needles of silver or copper are not affected, but merely those susceptible of being magnetized. These effects subsist only under the influence of the electrical current. If we suspend the circulation of the electricity, by breaking off the communications

established between the opposite poles of the voltaic apparatus, or even if we retard considerably its velocity, by joining its poles with bad conductors, the magnetic power instantly ceases, and the bodies which had received it return to their usual state of indifference.

This simple sketch already displays many new properties. All the processes hitherto employed to magnetize bodies had produced an effect on only three pure metals, iron, nickel, and cobalt, and on some of their compounds, steel for example, which is merely a carburet of iron. Till now it was never possible to render silver, copper, or the rest of the metals magnetic. But the electric current gives all of them this property; it bestows it transiently by its presence; and, as we shall presently see, it diffuses it through the whole mass, in a manner equally singular, and which has no resemblance to what is produced, when we develope magnetism by our ordinary processes, which consist in longitudinal friction with magnetic bars.

To produce these novel phenomena in the simplest manner, we must with M. Oersted, establish a communication between the two extremities of the voltaic apparatus, by a simple metallic wire, which may be easily directed and bent in all directions. We place afterwards in the neighbourhood of the battery, a very sensible magnetic needle, horizontally suspended. As soon as this is settled in the direction due to the magnetic force of the terrestrial globe, we take a flexible portion of the conducting, or *conjunctive* wire, as M. Oersted calls it, and having stretched it parallel to the needle, we bring it gently near it, either from above, from below, from the right, or from the left. We shall see an immediate deviation of the needle; but what is not the least remarkable circumstance, the direction of this deviation differs according to the side by which the conjunctive wire approaches it. Duly to comprehend this astonishing phenomenon and to fix its peculiarities with precision, let us suppose that the conjunctive wire is extended horizontally from north to south, in the very direction of the magnetic direction in which the needle reposed, and let the north extre-

mity be attached to the copper pole of the trough, the other being fixed to the zinc pole. Imagine also that the person who makes the experiment looks northward, and consequently towards the copper or negative pole. In this position of things, when the wire is placed above the needle, the north pole of the magnet moves towards the west; when the wire is placed underneath, the north pole moves towards the east; and if we carry the wire to the right or the left, the needle has no longer any lateral deviation, but it loses its horizontality. If the wire be placed to the right hand, the north pole rises; to the left, its north pole dips; and in thus transporting the conjunctive wire all around the needle, in directions parallel to one another, we merely present it to the needle, by the different sides of its circular contour, without affecting in the least the proper tendency of the needle towards the terrestrial magnetic poles. Since then the deviations observed in these successive positions are first of all directed from right to left, when the wire is above the needle; then from above downwards, when the wire is to the left; from the left to the right, when the wire is beneath; and, finally, from below upwards, when it is to the right hand, we must necessarily conclude from these effects, that the wire deranges the needle, by a force emanating from itself, a force directed transversely to the length of its axis, and always parallel to the portion of its circular contour, to which the needle is opposite. M. Oersted drew this inference from his first observations.

Now this revolutive character of the force, and revolutive according to a determinate direction, in a medium which like silver, copper, or other pure metal, seems perfectly identical in all its parts, is a phenomenon very remarkable, of which we had heretofore only one singular example in the deviations which certain bodies impress on the planes of polarization of the luminous rays. The first fact of the magnetism transiently impressed upon the conjunctive wire by the voltaic current, might have offered itself to a vulgar observer. I do not know whether some traces of this property may not have been previously perceived and indicated; but to have recognised this peculiar character of

the force, and to have delineated it, agreeably to its phenomena, without hesitation and uncertainty, is the praise which truly belongs to M. Oersted, and which constitutes a condition entirely new in the movement of electricity.

As soon as this beautiful discovery was known in France, England, and Germany, it excited the most lively sensation among men of science. One of our colleagues, in particular, M. Ampere, ardently verified it in all its circumstances. Seizing with sagacity the revolutive character of the force impressed on the conjunctive wire, he directed it with judgment, and skilfully developed the consequences which flowed from this property. His researches, which preceded those of the other French philosophers, have considerably occupied the Academy; but as the order of exposition, occasioned by the mutual dependence of the phenomena, hinders me from beginning with them, I have endeavoured to compensate for this inversion by rendering justice at once, to labours which have anticipated and facilitated others.

In the above experiments which M. Oersted had made, the conjunctive wire is presented to steel needles, previously magnetized. It may be asked, if the action then exercised is proper to the conjunctive wire, as the action of a bar of steel tempered and magnetized is proper to this bar, or if the action is communicated to the wire by the presence of the magnetic needle, as we see soft iron, which exercises no magnetic power of itself, acquire transiently this power in the presence of magnets? To decide this question it was necessary to examine whether a body, not magnetic in itself, but capable of becoming so by influence, soft iron for example, would experience a sensible action at the approach of a conjunctive wire, traversed by the voltaic current. This was effected by M. Arago, who shewed that filings of iron are attracted by these wires; a simple but important fact, which defines clearly one of the characters of the force by which the phenomenon is produced.

I shall now proceed to indicate briefly what has been done towards completing the analysis of the electro-magnetic forces.

The first thing which we must determine, is the law according to which the force emanating from the conjunctive wire decreases at different distances from its axis. This inquiry has been the object of experiments which I made along with M. Savart, already known to the Academy by his ingenious discoveries in Acoustics. We took a small needle of magnetized steel in the form of a parallelogram, and to ensure its perfect mobility, we suspended it under a glass bell, by a single fibre of the silk-worm, and gave it at the same time a horizontal direction. Then in order that it might be entirely at liberty to obey the force emanating from the conjunctive wire, we screened it from the action of the magnetism of the earth, by placing a magnetized bar at such a distance, and in such a direction, that it exactly balanced this action. Our needle was thereby placed in the same freedom of movement as if there did not exist any terrestrial globe, or as if we had been able to transport ourselves with it to a great distance in space. We now presented it to a conjunctive cylindrical wire of copper, stretched in a vertical direction, and to which we had given such a length, that its extremities necessarily bent in order to attach them to the poles of the electric apparatus, should have, in consequence of their distance, so feeble an action on the needle that it might be neglected with impunity. This disposition represented therefore the effect of an indefinite vertical wire, acting on a horizontal and independent magnetic needle. As soon as the communication of the voltaic current was completed, the needle turned transversely to the axis of the wire, conformably to the revolutive character indicated by M. Oersted; and it set itself to oscillate around this direction, as a clock pendulum, moved from the perpendicular, oscillates round the vertical by the effect of gravitation. We counted with an excellent seconds watch of M. Breguet, the time in which a certain number of these oscillations, twenty for example, were performed; and by repeating this observation, when the wire and needle were at different distances, we inferred the decreasing intensity of the force, precisely as we determined by the oscillations of the same pendulum, the variations of gravity at different latitudes. We thus

found that the force exercised by the wire was transverse to its length, and revolutive, as M. Oersted had observed; but we discovered besides that it decreased in a ratio exactly proportional to the distance. However, the force which we thus observed was in reality a compound result; for on dividing in imagination the whole length of the conjunctive wire, into an infinity of segments of a very small altitude, we perceive that each segment ought to act on the needle, with a different energy according to its distance and direction. Now these elementary forces are precisely the simple result which it is important to know, for the total force exercised by the whole wire, is merely the sum of their actions. Calculation enables us to remount from this resultant to the simple action. This has been done by M Laplace. He has deduced from our observations, that the individual law of the elementary forces, exercised by each section of the conjunctive wire, was the inverse ratio of the square of the distance; that is, precisely the same as we know to exist in ordinary magnetic actions. This analysis shewed that in order to complete the knowledge of the force, it remained to determine if the action of each section of the wire was the same, at an equal distance, in all directions; or if it was more energetic in a certain direction than in others. I have assured myself by delicate experiments that the last is the case.

What we now know of the law of the forces, is sufficient for explaining and connecting together a multitude of results, of which I now proceed to indicate briefly some of the most curious. For example, let us conceive as we have done above, an indefinite conjunctive wire, stretched horizontally from south to north. Let us present laterally to it a magnetic needle, of a cylindrical shape, and suspended so that it can take no movement but in the horizontal direction. For greater simplicity, let us withdraw it from the influence of the terrestrial magnetism, by neutralizing this influence with the action of a magnet suitably placed. This being done, when the needle rests at the same height as the wire, so as to point exactly to its axis, it is neither attracted nor repelled; but if we raise it above the wire, it presents one of its poles to it, and makes an effort of

approximation. If, on the contrary, we sink it below, the needle turns about, to present its other pole, and is then attracted anew. But if we constrain it to present the same pole as at first, the needle is repelled, and the effects are precisely inverse on the right and on the left hand of the wire.

If instead of transmitting the electrical current across a simple wire, we make it pass through tubes, plates, or other bodies of a sensible breadth, whose surfaces are composed of parallel right lines, we find that all these bodies act on the magnetic needle, as bundles of wires parallel to their length would do; which proves that the power developed in them by the electrical current is exerted freely through their very substance, and is not weakened by its interposition, as the radiation of heat through hot bodies is enfeebled and intercepted by the interposition of these very bodies.

Instead of leaving the needle in the preceding experiment at liberty to move, fix it invariably, but render the conjunctive wire mobile, by suspending it on two points; then it will be the latter which will move towards the needle, or recede from it. In fact, it is a general law of mechanics, that reaction is always equal to action. If the wire attract or repel the needle in certain circumstances, the needle ought in the same circumstances to attract or repel the wire. This experiment belongs to M. Ampere.

Now, let us operate no longer with a magnetic needle on the wire placed in its position of mobility, but let us expose it to the magnetic action of the terrestrial globe, which is known to be perfectly similar to that of a common magnet whose poles are very distant. This force will likewise make the conjunctive wire move according to the same laws, at least if it be sufficiently freely suspended, and it will impress on it a determinate direction relative to the plane of the meridian, just as it would direct any other magnetic body. This result was realized by M. Ampere*.

* The accuracy of this result has been questioned by some able philosophers in this country, on both theoretical and experimental grounds. M. Ampere's

Finally, instead of presenting a conjunctive wire to a magnet, present two conjunctive wires to one another, in parallel positions. Then if the revolutive direction of the force be the same for the two wires, they will both concur in giving one direction to a magnetic needle placed between them; but if the direction of the revolutive movement be opposite in each, they will tend to turn the needle in opposite directions. These are simple consequences of the law of the forces. Now on trying these two arrangements, M. Ampere has found that in the first the two wires come together, and that in the second, they mutually repel each other. Thence we must make two inferences; first, that the wires exert on each other actions perfectly analogous to those which they exercise on magnetic needles; and next that the distribution of these forces in each of their particles is analogous as to direction, with what it is in magnetic needles themselves. These two new conditions relative to the nature of the force, render this experiment very important.

In the different arrangements which we have just described, the conjunctive wires and the magnets attract or repel principally by their most contiguous parts; for with regard to the rest, their distance rapidly diminishes their action. Hence it is evident that we should augment the energy of the effects, if we approximated together the different parts of the conjunctive wire, preserving to them however the same general line of action. M. Ampere has also verified this position, by coiling the conjunctive wire in the form of a flattened spiral, on the plane of whose contours he acts with magnets, as on the side of a single wire.

M. Ampere's mode of operating, consists in bending a portion of wire, about two or three feet long, into a circular form, recurving its extremities so as to make each point dip into a little cup of mercury, which serves as pivots on which the circle is suspended and round which it may revolve. Into one cup, the ordinary wire from the copper end, and into the other, the wire from the zinc end of the voltaic trough is plunged. The plane of the circle then slowly places itself, according to M. Ampere, at right angles to the magnetic meridian.—TRANSLATOR.

Among the arrangements which he has thus formed, one of the most remarkable consists in winding the conjunctive wire around a cylinder of wood or glass, forming an elongated spiral. Then the force emanating from each point of the thread being always directed transversely to its length, becomes in each element of the spiral, perpendicular to the plane of the coils, and consequently parallel to the length of the spiral itself. Farther, on account of the revolutive character of the force, all the inner points of the different rings exercise, in the interior of the spiral, forces which are equal, and operate in the same direction; whilst in their action exteriorly, the forces emanating from the different points of each ring, oppose and weaken each other greatly by their obliquity. Thus the result of all these actions ought to be much more energetic within the spiral than outside of it; a consequence which actually happens. If we place in the interior of a spiral, unmagnetic steel needles, they will acquire in a few instants a permanent and very perceptible longitudinal magnetism; whereas, if we place them without the spiral, they suffer no change. This experiment is due to M. Arago. Sir H. Davy, without being acquainted with it, has since succeeded in magnetizing small steel needles, by rubbing them transversely on a single conjunctive wire, or even without contact, by placing them at some distance from it. This process does not differ from the preceding, except in using the force of only one wire, a force which the spiral multiplies.

Since the electricity developed by the friction of our ordinary machines, differs in no other respect from that evolved from the voltaic apparatus, than that the former is retained and fixed, while the latter is in motion; we find that whenever we cause the electricity of our machines to flow in a continuous current, it has produced absolutely the same effects as the voltaic battery. The similarity, or rather the identity, of these two forms of electricity, is manifested likewise in the production of the electro-magnetic phenomena. This has been shewn by M. Arago, who transmitted along the spirals of M. Ampere, no longer the voltaic current, but a succession of very small

sparks, drawn from a common electrical machine. Small steel needles, placed in the interior of these spirals, were thus magnetized in a few instants, and the direction of their magnetic polarity was found to be determined in reference to the surfaces charged with the resinous or vitreous electricity, precisely as happens with the copper and zinc poles of the voltaic apparatus. Sir H. Davy has also obtained similar results, by passing common electricity along a simple metallic wire, in the vicinity of which, small steel needles were placed at right angles to its length.

Art. VIII. *Description of a new Sinumbral Lamp: in a Letter to the Editor of the* Quarterly Journal of Science and the Arts.

Sir,

May I beg the favour of you to insert in the next Number of your *Journal*, the following brief notice and sketch of a new Shadowless Lamp, invented and manufactured by Mr. Thomas Quarrill, of Bell-court, Doctors' Commons.

I am, Sir, your constant reader,
and occasional contributor,
L.

A section of Mr. Quarrill's lamp is represented in Plate V, Fig. 2, from which it will be apparent that the oil reservoir is so shaped as to conform with the direction of right lines issuing from the brightest part of the flame, a portion of the light of which is thrown down by a small reflector upon the circular plate of ground glass, which fills the lower part of the lamp, and which is surrounded by the oil vessel. The chimney of the lamp is constructed as usual, and the whole is surmounted by a ground-glass light-distributor, so formed as to do away all shadow from any portion of the lamp, and at the same time not to offend the eye by any want of elegance in shape or dimension.

[We have received a drawing of the entire lamp which we have not thought it necessary to engrave, as the section above alluded to exhibits its material parts, and shows the peculiar excellence and advantages of Mr. Quarrill's construction.]

Art. IX. *Observations on the Solar Eclipse, September 7th, 1820.*

[Continued from p. 39.]

The observations contained in the preceding Number were almost exclusively confined to details of the different appearances exhibited by the moon's surface. From the facts observed, it has been endeavoured to deduce some conclusions respecting the general forms, magnitudes, and relative positions, of its more prominent inequalities. Those circumstances in the present phenomenon, which appear to be more immediately connected with the question of a lunar atmosphere, still remain to be considered.

Whether the moon, like the earth, be surrounded by an aëriform medium, is an inquiry that has long engaged attention, and in which various modes of investigation have been pursued. Prior to the time of Halley, it does not appear that much attention, as regards this matter, had been paid to the observation of eclipses*. Subsequently, however, both in the original

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* Kepler, about the commencement of the sixteenth century, had stated, in a general manner, that several appearances in eclipses of the sun, seemed to indicate the presence of matter of extreme tenuity surrounding the moon (*vide Astron., pars Optica*). There are also a few remarks of the solar eclipse of 1706, tending to establish the same opinion (Flamstead, Cassini, *Phil. Trans.*, for that year, and *Mem. de l'Acad. de Scien.*, 1706, p. 347). Scarcely any thing authentic, however, was known previous to the publication of Dr. Halley's *Memoir* on the total eclipse of 1715. A circumstance there mentioned still further confirms this. No eclipse of the sun had been observed in London from the year 1140, although the path of the umbra on several occasions must have crossed that meridian.

accounts of these phenomena, and in the more elaborate works on astronomical science, considerable importance, in relation to the above inquiry, attaches to certain appearances, observed externally round the margin, as also within the circumference of the moon while visible on the sun's disc. In the instance now under review, several of these appearances were observed. Instead, therefore, of merely enumerating isolated, and in themselves, perhaps, unimportant, facts, it is proposed in the following remarks, to view them in connexion with the subject at large, and as compared with the results of similar observations in preceding eclipses, arranging the different circumstances in the order of their occurrence.

From the original accounts* of the more remarkable solar eclipses, since the period above-mentioned, flashes of light darting in various directions, and from different parts of the moon's darkened hemisphere, appear to have been frequently observed. In general, these appearances seem to have been visible about the time of greatest obscuration only. "During the whole time of the *total eclipse*, (says Dr Halley, in describing that of 1715). I kept my telescope constantly fixed on the moon, and I saw *perpetual flashes of light*, which seemed for a moment to dart out from behind the moon, now here, now there, on all sides, but more especially on the western side, a little before the emersion†." In his *Memoir* on the eclipse of 1748, nearly the same account is given by M. de Thury:—

* The eclipses here referred to are chiefly those of 1715, 1724, 1736-7, and 1748, of which the principal accounts are by Halley, Louville, Cassini, Maraldi, M'Laurin, Le Monnier, De L'isle, &c. *Phil. Trans.* and *Mem. de l'Acad. de Sciences.* Of that in 1724, there is no account by an English astronomer, notwithstanding, as appears from the map which accompanies Le Monnier's *Memoir sur les Eclipses totale du Soleil*, the path of the shadow must have traversed great part of Britain, (*Mem. de l'Acad. de Scien.* 1731, p. 243). Of the annular eclipse of 1764, the accounts are few and uninteresting. Though great preparations were made by the French astronomers, Card. de Lugues seems to have been the only person who witnessed the formation of the annulus, by a momentary opening of the clouds. (*Mem. de l'Acad.*, &c., 1764.)

† *Phil. Trans.*, No. 342, vol. 29, p. 249.

"J'ai découvert vers *le milieu* de l'eclipse, sur la surface de la lune, comprise entre les cornes du soleil, des *rayons* de lumiere rouge*," &c.

A general conclusion from these appearances is, that "they are flashes of lightning, such as a spectator in the moon would see in our earth if its whole hemisphere were interposed between him and the sun†. Hence it is inferred, that "the moon has an atmosphere similar to ours, wherein vapours and exhalation may be supported, and furnish materials for clouds, storms, and tempests‡."

Of these phenomena, another explanation has been given, in which it is assumed, that cavities in the body of the moon, acting as concave mirrors, reflect the solar light obliquely to the eye of the observer, and that the progressive motion of the planet in its orbit, by constantly changing the position of the reflecting surfaces, in relation to the line of vision, causes the rays thus suddenly turned from their former direction, to appear in momentary streams or coruscations of light§.

Without insisting on the numerous objections which are opposed to the former of these hypotheses, such as the casual nature of the occurrence on which it rests, the small space over which its influence could be visible, the weakness of the illumination which it could produce, as compared with the extent, duration, and brilliancy of the effects observed—the very cause assumed would remove the possibility of viewing the effect from which it is inferred. Granting that such an atmosphere as is contended, does actually surround the moon, an observer on the earth would not, in that case, be able to discern the lightning, since the whole mass of vapour must necessarily intervene, and conceal it from view; as, by the laws which regulate its motions, the flash would pass between the surface

* *Mem. de l'Acad. des Sciences*, 1748, page 56.

† *Phil. Trans.*, as quoted above.

‡ Long's *Astronomy*.

§ *Astron.* de La Lande; M. De Pelo, *Memoires pour servir à l'Hist. de l'Astron.*, 1733, page 248; and *Mem. de l'Acad. des Sciences.*

of the moon and the adjacent clouds, in the lowest stratum of this supposed atmosphere.

That the appearances in question, therefore, are simply modifications of the solar rays, seems extremely probable. To the principle of the second theory, however, it is justly objected, that from the known properties of light, and the laws of vision, no form or position of cavities in a spherical body, or indeed of any figure interposed between the spectator and the source of illumination, could by reflection render the rays of light from that source visible on the opposite dark surface of the intervening mass.

To compare these different opinions with the inferences from observation. From an early period in the instance before us, appearances resembling those above alluded to might be distinguished. As the progress of obscuration advanced, the flashes became gradually more distinct, exhibiting the greatest intensity of light, immediately preceding and subsequent to the ecliptic conjunction. From this to within 12′ or 15′ of the egress, when they ceased entirely, a similar gradation, but inverted, was observed, these luminous streams appearing more and more faint, as the termination of the eclipse approached. This is merely to be considered as a general statement, implying that coruscations of comparatively the strongest brilliancy were most frequent about the time of greatest immersion; but, as has already been observed *, no regularity in the individual alternations could be perceived These radiations were not confined to any particular part of the lunar circumference; they were, however, *cæteris paribus*, more rarely visible on that division of the lower or western limb, where the largest portion of the sun's disc remained, at the time, unobscured. Their splendour was often considerable, more especially when viewed near the cusps; but in no situation so bright as not to have easily escaped the notice of an observer whose researches were directed to other objects. Sometimes they appeared to shoot

* *Journal of Art and Science*, No. 21, page 28.

from behind the moon, at others to play along the margin, and not unfrequently to dart in various directions, and to different distances towards the centre of the moon's disc.

Some time had elapsed before any indications of a general law could be discovered to regulate these effects. It was at length invariably observed, that wherever the inequalities of the lunar circumference were most conspicuous, flashes of light appeared to proceed most frequently from that quarter. Also, that the greater the space in the darkened hemisphere, over which they seemed to traverse, the more distinct they shewed, and the nearer did their colour approximate, from a very dilute purplish tint, to a red or dusky light.

These two facts seem to afford an easy solution of the phenomena in question. From the former it sufficiently appears that the primary cause is to be sought for in the inequalities of the moon's periphery. The solar rays, then, which were intercepted by the circumference falling upon the declining sides of the lunar mountains, would be variously reflected, not merely from the angular direction of the incident rays, but also as regards the position of the reflecting surfaces in relation to the whole mass,—when reflected from the side directly exposed to the sun, coruscations of thin light would appear to radiate from behind the moon—as the reflecting point was situate nearer the anterior surface, they would tend with proportional inclinations, towards the interior of the disc. The second fact is rather a necessary consequence, than a cause of these effects. If these luminous streams or flashes proceeded from the reflection of the sun's light, it is obvious that the direction of the reflected rays would materially influence their brightness. When the path of the reflected rays traversed any considerable portion of the moon's dark surface, they would produce a flash, necessarily more brilliant than in a direction nearer the circumference, both from the greater contrast of the surrounding shade, and as being more remote from the splendour of the sun's unobscured segment. The greater the darkness, therefore, the more distinct they would appear; and hence they differed in this respect, according to the different phases in

which they were observed. Hence, also, although by careful observations they might probably have been discovered in nearly all the phases, as in the present instance; yet, in the greater number of eclipses, they have been observed during the continuance of greatest immersion only. From similar causes, appearances resembling the above have been most conspicuous in total eclipses, and might then, from a casual view, have been easily mistaken for flashes of lightning. As an additional proof, it may be worthy of remark that after attentively watching their progress, by taking into the field of the telescope that part of the disc only where their appearance might be anticipated, these radiations were always found to come from the periphery, never from the interior of the moon's orb.

According to the quantity of the reflected rays, and their being occasionally, from position, longer exposed to the view of the observer; those broader and more permanent streams of light, which appear to have been visible in almost all eclipses of this nature, would be produced. One of these, which occurred nearly at the moment of conjunction, on the moon's western limb, as illustrative of the situation of the lunar mountains, has already been described.

In reference to the present inquiry, it may be further remarked, that no haziness, or melting of the light into the surrounding shade, could be observed; on the contrary, the line of demarcation was harshly traced, and the confines perfectly distinct, which would hardly have been the case, had the illuminated surface been surrounded by a medium capable of refracting the solar light. The reflected light, certainly, was less vivid at a distance, from the circumference; but this evidently arose from its being divided into unequal streams by a ridge of mountains. The outline of this ridge, as also that of the periphery, were likewise very distinctly marked, without the slightest appearance of an external and more faint illumination, such as would have been produced by any attenuated luminous matter surrounding them. (See Fig. 4, Plate III, of last Number.)

Another class of phenomena, from which considerable diversity of opinion has originated, is that of detached masses, or

spots of light, of various extent and brilliancy, sometimes during former eclipses, observed within the moon's disc. The appearances in question, however, seem so little different from those now discussed, that they may be considered under the same head, a deviation from the order of time which it was proposed to follow, being the only inconvenience attending this arrangement.

At 2h. 51′ 24″, in the present eclipse, one of these luminous spots was observed. Its position was considerably within the margin of the limb, and distant from the eastern cusp, about $\frac{1}{5}$ of that portion of the moon's circumference then visible on the solar disc. It remained perfectly distinct for nearly $1\frac{1}{2}$ minutes, exhibiting with little variation in size, one uniform appearance of a large irregular macula of reddish light, and when brightest, equal in splendour to a star of the 4th or 5th magnitude. The outline on the upper and lower sides, was well defined with deep angular curvatures, especially in the former. From the point of apparent intersection of the two peripheries, in the moon's eastern limb, a stream of paler light could be traced in a direction nearly parallel to the circumference, till it joined this spot From its opposite side, a similar stream, but of still fainter colour, extended almost in a line with the point of contact in the western limb, to about one fourth the distance between them. (See Fig. 5. Plate III.)

Appearances similar to the above, not unfrequently occur, in the accounts of preceding eclipses. Thus, M'Laurin, in describing the one of 1737, states, that "before the annulus was complete, a remarkable point or speck of pale light appeared near the middle of the part of the moon's circumference that was not yet come upon the disc of the sun: and a gleam of light, more faint than that point, seemed to extend from it to each horn." He adds, "I did not mark the precise time, when I first perceived it, but am satisfied it could hardly be less than $\frac{1}{4}$ of a minute before the annular appearance began*". In that of 1748, in like manner there was seen by Short, "about the middle of

* *Phil. Trans.* vol. lx. p. 181.

the eclipse, a remarkably large spot of light, of an irregular figure and of a considerable brightness, about 7′ or 8′ within the limb of the moon." It is rather singular, that the Earl of Morton, who was making observations with a reflector, in the apartment immediately adjoining, could not perceive this appearance. "I lost this light" continues Mr. Short, "several times: whether this was owing to shutting my eyes in order to relieve them, I cannot tell; when I first observed it, I called to my Lord Morton, but he could not perceive it*." Many other instances might be adduced similar to those now quoted†. On former occasions, these appearances seem to have been almost invariably observed about the middle of the eclipse; that, in the present case occurred towards the commencement of the last quarter: the only circumstance, however, materially different, is, that in several, the streams of faint light were not seen, or at least are not mentioned.

To explain these phenomena, various hypotheses have been framed‡. Among the proofs of an atmosphere, the supposed existence of lunar volcanos, as almost decisive of the question, naturally holds a prominent place. The preceding appearances accordingly are made to favour that opinion, and these isolated sparks of light, have been attributed to volcanic effects. The evanescent nature of these appearances, their apparent great extent, their having been hitherto visible, at certain phases only, the lines of fainter light proceeding from them, are circumstances seemingly incompatible with such an origin. The solutions opposed to this opinion are, however, not less liable to objection. Since the appearances in question, have been successively ascribed, to perforations in the body of the moon, to igneous vapours in the atmosphere floating across the field of vision, and to reflections of the solar rays from the surface of the earth, which impinging on the summits of the lunar moun-

* *Phil. Trans.* vol. xlv, p. 588.

† *Phil. Trans.* vol. xxxviii, p. 332. vol. xli, p. 94, &c. *Mem. de l'Acad. de Sciences.*

‡ See Schroter's *Selenotopographische Fragmente*, &c.

tains, are again transmitted to the eye of the observer. The last is the supposition least exceptionable, yet is obviously far from accounting for all the effects. Besides, were this the real cause, the light would be more universally diffused, as is seen in the new moon, when the whole unenlightened part of the orb is faintly illuminated by rays reflected in the same manner as is here supposed.

To apply the facts already discussed, to the explanation of the present phenomenon, let it be observed that the lunar mountains are probably arranged in chains of great extent*. That one instance has been adduced in which reflections of the sun's rays were separated into different streams by ridges apparently in this situation; and that, during the whole eclipse, the luminous appearances previously mentioned, were most conspicuous near the cusps, simply because in that position the inequalities of the periphery were most advantageously placed for intercepting, and reflecting the solar rays, in directions traversing the moon's disc. One of the larger and more permanent streams of reflective light, then, passing behind one of these mountainous ridges, in a line inclining to parallelism with the moon's horizontal diameter, would either be altogether invisible, or but faintly seen, according to the degree of its elevation. If the ridge were continuous throughout its whole extent, no new effect would follow; but if a discontinuity of any considerable length occurred, the illumination becoming then more diffused, would present the appearance of an irregular lucid spot, varying in extent, form and brilliancy, according to the different modifying causes. The probable cause has been pointed out why these streams generally appear to proceed from one of the cusps; striking on the elevations at the other extremity of the opening, the rays would again be reflected, from a well known law in optics, in a direction tending to the opposite cusp, in proportion as the point was nearer the centre of the intervening circumference. Exactly the same effects would follow, if instead of being diffused through an

* *Journal of Arts and Sciences*, No. 21, p. 38-9.

interruption, it be supposed that the light is reflected from a more elevated portion of the ridge. The former supposition better explains those instances, in which, like the present, the illumined spot lies in the lower limb; the latter is more applicable to such, as, like that mentioned by M'Laurin, have it in the upper margin. It is obvious also, that when the direction of the reflected rays was so depressed as not to appear above the summits of the ridge, a solitary speck of light, such as seen by Short, would alone be observed.

In the preceding investigation of one class of phenomena from which the agency of a lunar atmosphere is inferred, no other principles of discussion, except those derived from actual observation, have been admitted; the inequalities of the moon's surface, and reflection of the solar rays. That the effects described, are at least intimately connected with these causes, still further appears from the fact, that in those eclipses in which the former have not been clearly perceived, these luminous streams, flashes, and spots have not been observed. Thus, Weidler states, that on one occasion, although very distinctly marked, the outline of the moon's disc appeared without any elevations or depressions, such as he had formerly seen, and even attempted to measure; as also unaccompanied by any of those indications of an atmosphere which he had previously noticed. "Cæterum lunæ discus, sub sole, peripheriam *accurate terminatam*, absque ulla *inequalitate* nec non faciem nigerrimam ostendit. *Nullum* quoque *atmosphericæ* orbi lunæ insidentis *vestigium* potuit deprehendi*". On the whole, therefore, it may be concluded, that from the appearances now described, no proofs of a lunar atmosphere can be deduced.

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* *Phil. Trans.* vol. xli. p. 94, Weidler appears to have paid great attention to the observation of solar eclipses, with a view to establish the hypothesis of a lunar atmosphere. He has published details of several, of considerable magnitude, in which he ascribes various streams of light, &c. to atmospheric effects, and adds, that he not only saw them, but "*quadam vidit.*" On all these occasions the mountains were very conspicuous, the depth of one valley is estimated at $\frac{1}{100}$ of a diameter.

Among the phenomena, which have been considered as most directly indicating the presence of this atmosphere, are to be reckoned those luminous circles or halos, which, at the time of complete immersion, in total and annular eclipses have been observed concentric with, and immediately surrounding the moon's circumference. No appearance of this nature could be observed on the present occasion; about 2′ indeed after the conjunction, as determined by previous calculation, the dark and hitherto strongly defined outline of the periphery seemed less distinct, but without the slightest appearance of any extraneous matter.

From the original observations, it appears, that these luminous rings seemed to break out from behind the moon, varying in breadth from a digit to $\frac{1}{10}$ of a diameter. They were brightest near the body of the moon, and of a pale. or, as it is expressed, "pearly" light, gradually diminishing in splendour as the distance increased, and apparently "terminated by the extreme rarity of the matter of which they were composed*."

In order to account for these appearances, three different theories have been proposed. The hypothesis of Cassini, in which it was assumed that they proceed from the effects not of a lunar, but of a solar, atmosphere, appears never to have been generally adopted. The principles of the other two are thus briefly stated by Le Monnier "Ou bien la lune est environnee d'une atmosphere tres deliee, et dont la matiere est fort

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* *Phil. Trans. Mem. des l' Acad des Sciences Mem. de Berlin, (Euler). Mem. de l' Institut. (Lalande).* There is a remarkable coincidence in the estimates of the breadth of these circular areas. Flamstead, Halley, Cassini, &c., stating it as in the text, and others even greater. M. de Ihering's measurement it is not now easy exactly to ascertain. "Un filet de lumnière sembloit masquer le disc de la lune, et qui s' etendoit a une distance de cornes egal a peu pres a l'espace compris entre trois fils de micrometre." (*Mem. de l' Acad. des Sciences, an.* 1748, p. 56) The colour of these rings does not appear to have been invariably a pale white, but in some instances extremely brilliant: thus Cassini mentions, that at Zurich, "Le soleil fut couvert pendant quatre minutes, et le bord de la lune paroissoit comme un anneau d or. *Mem. de l' Acad. &c, an.* 1706, p. 317.

rare, ou ce qui revient au meme les rayons du soleil qui rasent la lune, souffrent une refraction d'autant plus grand qu'ils s'approchent a ce corps, et par consequent, s'inclinent vers l'axe du cone d'ombre au lieu de parvenir en ligne droite du soleil jusqu'à notre œil*."

It is not intended to enter into any discussion of the various details connected with this part of the subject, as being foreign to the present design; no opportunity having occurred of comparing former opinions with the results of actual observation. It may be necessary, however, briefly to shew, how far the conclusions legitimately derived from the two principal theories are decisive of the general question. In one respect their principles are identical; both assuming that these luminous rings arise from the rays of light being inclined towards the axis of the conical shadow; they differ only as to the primary cause in which this defection originates. In the one it is maintained, that the moon being surrounded by an atmosphere of the same nature as ours, its effects in refracting the solar rays must also be similar, and that these illumined circular areas seen round the margin, to the extent of $\frac{1}{12}$, or in some instances $\frac{1}{10}$, of the moon's diameter, are thus produced. Now, as the refractive properties of homogenous media depend on their densities; and, since, from the diminution of gravity, the density of the earth's atmosphere is to that of the moon's nearly as 408 : 139†, their refraction must be in the same proportion. The mean elevation, also, at which the atmosphere of the former is capable of reflecting the twilight, is about $\frac{1}{24}$ of a diameter of the latter, therefore, as 408 : 139 :: $\frac{1}{24}$ (chr.) : $\frac{1}{29}$ (diur.) the altitude of lunar refraction on the highest estimate, which, however, is not equal to one-tenth of the extent of the appearances in question. But as

* *Mem. de l' Acad. des Sciences, an.* 1765. See also *Mem. de Berlin*, 1748, p. 103. Where Euler adopts the theory of a lunar atmosphere, and *Mem. pour servir a l' Hist. del Astron.* 1733, p. 243—50, &c. in which De l'Isle supports the hypothesis of inflection.

† *Newt. Princip. Mathem.* From an erratum in page 34 of last Number, gravity at the surface of the moon is said to be diminished "one-third" instead of *to* "one-third" "nearly."

clouds and vapours are never found above the highest mountains on the earth, none of which exceed $\frac{1}{438}$ of the moon's diameter, it is to be concluded that the grosser part of our atmosphere, capable of producing any *sensible* refraction of the rays of light, does not extend beyond that elevation. Consequently, as 408 : 139 : : $\frac{1}{438}$ dia. : $\frac{1}{1279}$ dia. which, on the same principle of analogy as is recognised in the theory itself, gives a very near approximation to the true extent of lunar atmospheric refraction; a quantity, nevertheless so small as not to subtend an angle of 1″, and which does not exceed one mile in perpendicular elevation, whereas the circles of what is termed refracted light, appear to have extended at least 160 miles beyond the moon's surface.

The second theory is found on a property of matter, by which it deflects, or attracts towards the perpendicular, the rays of light which pass very near its surface. From experiment, however, the quantity of light thus deflected is so inconsiderable as to be apparently inadequate to the production of the effects in question. If the observations of Maraldi are to be considered as accurate, the body of the moon must be regarded as even altogether destitute of this property of terrestrial matter. But we are not left to doubtful speculation on this subject. Since it has been determined that the aberration of the rays produced by the attraction of the moon's surface does not exceed 3.5″, a quantity which certainly appears insufficient to account for the phenomena above described*. Such are the limits prescribed, both by analogy and experiment, to the operation of those causes, which are assumed as perspectively producing the effects in question. That they cannot far exceed these limits also appears, for if the inflection of the solar rays, whether effected by atmospheric refraction, or by the attraction

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* M. du Sejour trouva qu'il falloit faire l'inflexion (de rayons que rasent les bords de la lune) d'environ 3½″. M. Mechain, et M. Lexelle ont trouvé le même resultat.—*La Lande Astron* tom ii. p. 445 *Sejour. Traité des Mouvem.*, &c.

of the moon's body, exceeded 7″, the apex of the shadow would fall within the mean distance of the node, and consequently a solar eclipse could never happen. The nature of these luminous rings, therefore, seems altogether inexplicable; at least, the theories which have hitherto been proposed on that subject, leave the question of a lunar atmosphere undecided.

The evolution of greater light and heat from particular portions of the unobscured part of the sun's disc, is one of the most interesting phenomena connected with the subject of eclipses, and at the same time appears one of the most inexplicable. Dr. Halley is the first, and indeed the only observer, who distinctly noted the circumstance, and seems to have remarked it, about the time of greatest obscuration only. On the present occasion, however, from the obscuration of $5\frac{1}{2}$ or 6 digits, to the same phase in the egress, it was frequently experienced that more light and heat were transmitted from the western than from the eastern divisions of the sun's disc. This effect was very sensibly felt on the eye and face, when the telescope was pointed in immediate succession, to the respective portions of the unobscured segment, taking care to move it across the moon's dark surface, or below the sun's lower limb; and to admit into the field, only a small part of the enlightened disc near each cusp. Dr. Halley's statement is somewhat different. "When the first part of the sun remained on his east side, it grew very faint, and was easily supportable to the naked eye even through the telescope, for above a minute of time before the total darkness; whereas, on the contrary, the eye could not endure the splendour of the emerging beams through the telescope even for a moment. He ascribes this to two causes, "the dilatation of the pupil during the darkness, which before had been contracted by looking on the sun," and the eastern parts of the lunar atmosphere being replete with vapours raised from a surface exposed during thirty days to the rays of the sun, and from the opposite cause the western parts would be pure and transparent. Neither of these seems a satisfactory explanation; the fact indeed scarce admits of one, but

if a conjecture may be hazarded, the cause is, perhaps, to be looked for in some property of the sun itself*.

At the termination of the eclipse, although a small triangular portion of the moon's periphery could be seen, when every other part had passed off the solar disc, not the least appearance of refraction, or any indication of an atmosphere could be perceived. For some moments after egress, the moon totally disappeared, but the observation was anxiously continued, in hopes of realizing M'De Isle's suggestion. About 2″ from the sun's margin, the finely attenuated film of pale light was descried, which gradually increasing, at length appeared to extend along nearly $\frac{1}{3}$ of the moon's circumference, exhibiting at the same time considerable breadth, much greater, indeed, than could have been supposed considering its extreme proximity to the sun. The colour of the illuminated surface was similar to, but more faint than, that of the moon, when sometimes seen during the day. The light towards the extreme points seemed to disappear by degrees; at the centre it shewed more acutely defined, the circular outline, nearest the sun, was perfectly distinct; the appearance, however, was so transient, that a general description only can be given.

The straight line joining the extremities of the enlightened segment, would have been nearly at right angles to the path of the centre; and the illumination evidently was such as would arise from the effects of the solar rays falling on a spherical body, unconnected with any atmospheric medium. (Plate III. Fig. 6)

At the time of greatest obscuration, the diminution of light, although considerable, was by no means so great as had been anticipated. A mild agreeable lustre was diffused over the nearer objects, and it was only in the deep blue tones of the back ground that one could recognise

> ———A faint erroneous ray,
> Glanced from the imperfect surfaces of things,
> Fling half an image on the straining eye.

* M. Le Monnier seems to have misunderstood this description of Dr. Halley's, and to have taken for indications of atmosphere, what the latter has ascribed to the effects of the lunar mountains.—See *Mem. de l'Acad. des Sciences*, 1761, p. 252.

Art. X. *On the Divisibility of Matter.*

To the Editor *of the Journal of Science and the Arts.*

Sir,

The following calculations, on the extent to which the divisibility of matter may be carried in the particular instance which I have endeavoured to illustrate, have never, to my knowledge, been yet offered to the public; and as they may be the means of inducing others, more adequate thereto, to take up the subject, I have ventured to request their insertion in your *Journal*, should you deem them admissible: entreating the candour of your readers in the perusal of the present crude statement,

I remain, Sir,

Your obedient servant,

S——.

June 14, 1821.

It has been calculated by Mr. Boyle, I believe, that fifty square inches of leaf gold weigh only one grain; and that an inch in length may be divided into two hundred parts, each visible to the naked eye. Consequently, each square inch will contain forty thousand such parts: for $200 \times 200 = 40.000$, and this multiplied by the fifty square inches, will make two million visible pieces, into which a single grain of gold may be divided; this, however, does not come near the ideas of an eminent professor, who has recently asserted, that gold, in the gilding of silver wire, may be reduced to the thinness of a twenty-millionth part of an inch; and, as he illustrates it, will bear only the same relation to an inch, as the thickness of a sheet of paper would to a mile in length. If this be the fact, and we allow only 200 visible parts in the inch, it follows that each 200th part, as above, may be further divided into 100,000 other parts, so that a single grain of gold may be capable of being divided into one hundred thousand times two millions, or

200,000,000,000, which, though approaching to almost infinite divisibility (at least, according to our limited ideas,) yet we must all feel to be mathemically true; and even these instances appear to fall far short of the extent to which the operations of nature carry the actual divisibility of matter, as exhibited more particularly in the minute particles of odoriferous bodies, constantly filling surrounding spaces to a considerable distance, without any perceptible diminution; and perhaps still more so, in the wonderful formation of the animal kingdom, as more peculiarly displayed in the minute (in many instances, invisible,) insect tribe, each of which possessing attributes of the larger animals, as muscles, circulation of the blood, *&c.*, must very far surpass any ideas which the human mind can form on the subject; and yet it is possible, that even these may be still further surpassed by the divisibility of the particles of light. Let us take the well-known effect of a lighted candle, which may be seen at the distance of two miles, and probably further; in this instance, light is diffused almost instantaneously, and that without any sensible diminution of weight, throughout a circle, whose diameter is four miles; or rather, supposing the light placed upon a plane, it will extend or diffuse light throughout a hemisphere of that dimension, whose centre is the flame of the candle. During the process of combustion, the light, according to *Richter's Theory*, proceeds from the combustible body; however this may be, it should appear evident that, in the production of light (from a candle, for instance,) a certain quantity of matter, either combined or uncombined, is diffused through a given space in a given time. Let it be allowed, that a candle, weighing four ounces, will burn, or diffuse light, for six hours; and that, during that period, it fills unceasingly a hemisphere, whose radius is two miles, or 126,720 inches, containing by computation, if I am correct, 4,261,820,184,605,491$\frac{2}{10}$ square inches. Now each square inch was found capable of being divided into 40,000 visible parts; consequently, the hemisphere contains 170,472,

807,384,219,648,000 parts visible to the naked eye, and which are unceasingly illuminated by the diffusion of light from a certain portion of matter (combined or uncombined), weighing four ounces, for the space of six hours. Now say, four ounces is 1,920 grains, which, for six hours, will give about $\frac{1}{12}$ grain per moment; this $\frac{1}{12}$ grain of matter is thus found to be instantaneously divided into 170,472,807,384,219,648,000 visible parts; and consequently each single grain into twelve times that amount which is 2,045,673,688,610,635,776,000, or upwards of two thousand millions of millions of millions. Now a grain of gold was found divisible into two millions of such parts, it therefore follows, that the divisibility of gold to light, contained in the inflammable matter, supposing the foregoing to be a correct statement, is as 1 to 1,022,836,844,305,317 $\frac{8}{10}$, or as one to above one thousand millions of millions; and even this may be comparatively trifling, to the probable diffusion of the solar light.

Our limited powers of comprehension are very inadequate to form just conceptions of infinity, and the preceding view of the divisibility of matter, may perhaps tend, in some degree, to elucidate a subject which, to the generality of those who are not in the habit of studying the power of numbers, would appear possibly as incredible as the immensity of space exhibited in the starry heavens, as laid down in astronomical calculations; for when we say, that the distance of certain heavenly bodies is millions of millions of miles; or, that a single grain of matter may be divided into millions of millions of visible parts, a smile of scepticism would with some be the only result of an endeavour to enforce the truth thereof. May it not serve to familiarize the subject to the inquiring mind, to observe, that we may suppose it sufficiently easy to comprehend that the space of the tenth part of an inch may be divided into twenty parts, which is two hundred to the inch. Allow this, and we readily prove that each square inch contains forty thousand, and the solid inch, eight millions such parts; yet to assert, that a solid inch of matter may be divided into only eight millions,

would appear to the mere superficial observer, as beyond credulity, though it is capable of actual and practical proof, and even, as in the case of gilt silver wire, to an extent immensely beyond. Why, therefore, doubt the deductions made upon principles that cannot err, merely because they are beyond our present ideas of possibility? As well might we deny the existence of the Creator because his works transcend our limited powers of comprehension. We not, from this deduce a powerful argument in favour of the truth of revealed religion: for if each individual is to doubt of every thing that exceeds his own peculiar ideas of probability, at what point shall incredulity find a barrier? That immensity and divisibility (and who shall say where they may find a limit?) approach the confines of infinity, must appear evident to every one who has seriously contemplated the results; and if the preceding may, in any way, tend to illustrate the subject, or induce others to lend their aid thereto, the end of the present attempt will be accomplished.

Art. XI. *On a New Pyrometer.* *By* J. F. Daniell, Esq. F.R.S. and M.R.I.

[With a Plate.]

It would be needless to preface much upon the utility of an instrument to measure the higher degrees of heat, as nothing in science has been more eagerly desired, and nothing, it is generally allowed, would tend more to the perfection of many of the arts. The difficulties which oppose themselves in practice to any contrivance for this purpose, are best appreciated from the knowledge of the fact, that but one attempt has ever been made, with any degree of success, to solve so interesting a problem. The late Mr. Wedgwood invented an instrument with this view, founded on the principle that clay contracts in

its dimensions in proportion to the intensity of the heat to which it is exposed. His experiments and results are alone referred to in books of science upon this subject, but the pyrometer itself has long fallen into disuse, partly from the difficulty of obtaining clay-pieces of an uniform composition, but chiefly from the discovery that a long continuance of a lower degree of heat produces the same effect of contraction as the shorter continuance of a higher degree.

Being struck with the importance of the subject, I have lately bestowed much of my time, and made many fruitless endeavours to attain this desirable object. At length, however, I flatter myself that I have been fortunate enough to combine an instrument extremely simple in its construction, very manageable in its use, not liable to injury, when injured easily repaired, and which will extend the scale of the thermometer, at least to the fusing point of cast iron. Its sensibility also is very great, considered with regard to the extensive range which it is destined to measure. A change of about seven degrees of Fahrenheit's scale is distinctly indicated by it, while, on the other hand, every degree of Wedgwood's pyrometer was calculated to be equivalent to 130° of the same thermometer.

The results which I have obtained with this instrument differ, unfortunately, very widely from those of Mr. Wedgwood, but I shall, as I proceed, state my reasons for believing that mine are the more accurate of the two. I shall not enumerate the different steps by which I proceeded, but shall at once describe the pyrometer in the most perfect state which my hitherto limited experience of its use enables me to suggest.

Plate VI., fig. 1, represents the instrument drawn to the scale at the side of the plate. Fig. 2 represents a part of the same of half the real dimensions. The tube *a b c* is made of black-lead earthen ware, and the shoulder in its centre is moulded in its construction. The extremity *a* is close, and the extremity *c* open; *d* is a ferule of brass into which the end of the black-lead tube is accurately fitted, and to which the scale *e f g h* is attached. In the inside of the tube *a b c* lying upon

it, and extending to *b* is a bar of platinum 10.2 inches long, and 0.14 of an inch in diameter. It is immovably fixed at *a* by a nut and screw of the same metal on the outside, and a pin or shoulder on the inside. It is likewise confined to its place at *b* by a small perforated plate of platinum through which it passes. From its end *b*, proceeds a fine platinum wire of about $\frac{1}{100}$ of an inch diameter, and coming out of the tube at *d* passes two or three times round the axis of the wheel *i*, fixed on the back of the scale *e f g h*, and represented at fig. 2. It is then bent back and attached to the extremity of a slight spring *m n*, which is stretched on the outside of the brass ferule, and fixed by a pin at *n*. The wire is thus kept extended by the action of the spring. The axis of the wheel *i* is 0.062 of an inch in diameter, and the wheel itself is toothed and plays into the teeth of another smaller wheel *k*. This smaller wheel is $\frac{1}{3}$ the diameter of the larger, and carries on its circumference $\frac{1}{3}$ the number of teeth. To its axis, which passes through the centre of the scale *e f g h*, is attached the index *l*. Now the theory of this combination is, that any alteration of the relative lengths of the metal wires and earthen tube will cause the wheel *i* to move from the action of the spring *m n*, which motion will be multiplied three times by the wheel *k*, and measured by the index *l*. The scale is divided into 360°. Instead of passing the fine platinum wire round the axis of the wheel it has been found better in practice to attach a short silken thread to its extremity, and pass that round and fix it to the spring. The dimensions, which I have stated above, may, of course, be varied to suit different purposes. Nothing depends upon their nice adjustment, or upon intricate calculation. The value of the degrees, it will be seen in the sequel, is determined for each instrument by two fixed points in a manner perfectly analogous to the graduation of a thermometer.

If the extremity of the instrument *a b* be now gently heated the index will be seen to move forward with a gradual and very equal motion, and by careful cooling, will return as gradually from the point from which it started. The accession

of heat causes the metal bar and wire to expand more than the earthen tube; the consequence is, that the action of the spring always keeping the wire tight draws the wheel round. In cooling the metal again contracts, and restores the spring to its former degree of tension.

If instead of heating the instrument gradually, it be plunged at once into a brisk fire up to the shoulder *b*, the index will at first move back some 10° or 20°: it will then become stationary for a short interval and afterwards move rapidly forward. The reason of this is that the sudden application of a high heat causes the tube to expand before its effect is felt by the included bar, the consequence is that the bar becomes relatively shorter, and the effect of contraction is produced upon the wheels; but directly that the influence of the heat reaches the metal, it rapidly overtakes the counter expansion of the tube, and the index immediately moves forward to the point which it would have attained if it had been gradually heated. The reverse of this takes place if it be suddenly taken from a high temperature into the cold air. This is one beautiful testimony of the delicacy and accuracy of the instrument. It is well known that an analogous effect is produced upon a thermometer under similar circumstances; if its bulb be placed suddenly in the flame of a candle, or it be otherwise suddenly heated, the mercury will appear to fall in the tube, that is to say, the expansion of the glass momentarily exceeds that of the metal.

After having ascertained that the effect of the combination was such as I had anticipated, and that the index moved forward regularly in proportion to the heat applied, and returned in cooling to the point from which it set out, my next object was to ascertain, if this effect were perfectly equable, and to obtain the value of the degrees, if possible, in degrees of the thermometer.

For this purpose I procured a cast-iron case, one foot in length, two inches wide, and two inches and a half deep, in which was a partition one inch from the end. In the centre

of this end, and in the partition, were round holes which just admitted the stem of the pyrometer. The instrument was passed through these holes up to the shoulder, the small division of the case was then tightly stuffed with tow and lute, and the larger division was filled with mercury. The whole length of the platinum bar was thus immersed in that metal. The apparatus so arranged, with the needle pointing to 0°, and the temperature of the air being about 60°, was placed over two Argand lamps, or a small furnace, and gradually heated. The index of the pyrometer moved slowly and steadily forward from 0, and when it had reached 85° the mercury boiled rapidly. It was kept in a state of ebullition for half an hour, and the index remained stationary at that point. The strong concussions, indeed, of the boiling metal caused the needle to vibrate, but its motion was confined between the degrees of 83 and 85. Now, if we assume the boiling point of mercury, under the pressure of the atmosphere, to be 656° of Fahrenheit's scale, as we are justified in doing from the best authorities, and deduct 56° for the temperature of the air when the pyrometer stood at 0°. We have 85° degrees of the pyrometer equivalent to 600° of the thermometer, making each degree of the former equal to about 7.0 of the latter.

Having in this manner calculated the value of the degrees, I proceeded to ascertain the equability of the expansion throughout the thermometric scale. The same apparatus was made use of with the addition of a thermometer plunged into the mercury. The heat was applied very carefully and gradually, and the progress of the two instruments was compared at every 50° of the thermometer, both in heating and cooling. The results of the experiment are contained in the following table. The first column contains intervals of 50° of Fahrenheit's thermometer; the second shews what the corresponding degrees of the pyrometer ought to have been, by calculation from the former experiment; the third exhibits the actual ascent of the index while heating; and the fourth its descent when cooling.

Thermometer.	PYROMETER.		
	Calculation.	Experiment.	
		Ascent.	Descent.
°	°	°	°
50	7.2	7.2	8.0
100	14.4	14.0	15.5
150	21.6	22.5	23.0
200	28.8	30.5	30.0
250	36.0	38.5	36.5
300	43.2	45.5	43.5
350	50.4	51.5	50.5
400	57.6	58.5	57.5
450	64.8	66.9	65.0
500	72.0	73.5	72.5
550	79.2	77.0	79.7
580	83.6	84.	——
600	86.4	——	——
656	93.6	92 Mercury boils	

The exact point of 600° it was impossible to compare with any safety to a close thermometer. The point of 580° agreed very well in the ascent, but too rapid cooling prevented me from catching it on the return.

When the difficulty of comparing the fine divisions of two instruments so situated, and both in progressive motion, is considered, this will no doubt be regarded as a very close agreement, and quite sufficient to establish the fact of the equability of expansion of both the tube and the metal bar. The experiment has been often repeated with a perfect accordance of result. On one occasion, the lamps employed to heat the mercurial bath had been placed accidentally nearer to one extremity than the other: not having attended to this circumstance, I was surprised not to find the usual accordance between the pyrometer and the thermometer; but, upon considering the matter, I placed a thermometer at each end of the bath, and found that there was a difference of 30° between the two, and the pyrometer indicated as nearly as possible the mean. I was not previously acquainted with the possibility of such a difference

existing in a bath of this fluid, however unequally heated; but, upon inquiry since, I find that the remark has been often made. The result has afforded another very satisfactory testimony of the delicacy of the pyrometer, and proves that it will very accurately indicate the mean of the temperature to which the bar is exposed. It is for the purpose of ensuring the application of heat always to the same point exactly, that the shoulder is made on the tube denoting the depth to which the instrument should invariably be immersed.

I shall now proceed to enumerate some precautions which are necessary to be taken in the construction and use of the pyrometer, especially when intended for the observation of very high temperatures, such as that of the fusion of iron. I have selected the black-lead earthen ware, after several trials of other materials, on account of the equability of its expansion, its infusibility at high temperatures, and the perfect manner in which it sustains sudden transitions from heat to cold. I have not only repeatedly taken the tubes at once from a white heat into a cold atmosphere, but have plunged them when red hot into cold water without their sustaining any injury whatever. The heat at which this ware is commonly baked is not very high, and I consider it necessary to ensure accuracy, that the tube should have been exposed for some time to a temperature at least equal to the highest which it is intended to measure with the pyrometer. For this purpose those which I have made use of were placed in an iron-founder's furnace. After this operation they assumed exteriorly a brown appearance, but were as soft and as easily cut as before. When it is required to expose the pyrometer to a naked fire, it is proper to furnish it with an exterior tube of the same, or some other ware which is easily fitted to the shoulder by grinding; otherwise the fuel is apt to act as a flux upon the tube, which, becoming vitrified, will crack with sudden transitions of temperature. This precaution is not so necessary when a muffle is used, but is perhaps always advisable at very high heats.

When it is proposed to keep the pyrometer for a long time in a very strong fire, a piece of cloth may be wrapped round the

brass ferule and upper part of the tube, and kept moist with water to guard against too great an accumulation of heat in that part. I have not, however, myself had occasion to make use of this expedient, although I have kept the instrument for half an hour at a time at the temperature of fused iron. The black-lead is so very bad a conductor of heat, that its transmission is very slow from one end to the other.

The value of the degrees of each instrument must be taken for itself, and this is very easily done by means of the apparatus before described. The boiling of mercury furnishes an admirable fixed point for this purpose. The number of degrees equivalent to 656° of Fahrenheit should be marked upon the scale.

The motion of the index, as before stated, is very gradual, and it stops directly the intensity of the fire ceases to increase. The difference of a greater or less draught of air in a furnace is instantly denoted by it. In the furnace which I have been in the habit of employing, the opening of a small door instantly increased the advance of the needle, and the closing it as suddenly checked it. When properly managed in cooling, the needle never failed to return within two or three degrees of the commencement of the scale; but to ensure this effect, it is necessary that the instrument should be cooled very gradually in the furnace to which it has been applied, or that it should be removed suddenly and at once into the cold air. If it be withdrawn gradually, the partial cooling is apt to produce a slight alteration in the form of the tube, the effect of unequal contraction. From this cause I have seen the index not return within twenty or thirty degrees of the point from whence it set out. This small deviation in the form of the tube when it occurs is not of any serious practical consequence as the index may always be set at the beginning of an experiment, to the commencement of the scale, or to the temperature of the air, without in any way affecting the accuracy of the subsequent observations.

I shall conclude by recording the results of some experiments which I have tried upon the fusing point of some of the metals.

I do not offer them as positive and accurate determinations of the different degrees, but only as nearer approximations than any that have yet been furnished from actual observation. The only method which I have yet had it in my power to adopt for this purpose, I do not consider to be susceptible of absolute accuracy. The arrangement made, consisted of a muffle of black-lead placed in an excellent draught-furnace. This muffle was furnished with a door through a round hole in which the stem of the pyrometer was passed up to the shoulder. Another hole in the top of the door which could be stopped at pleasure, admitted a full view of the interior. The metal to be tried was placed in a small black-lead receptacle of the same thickness as the pyrometer tube, in the middle of the muffle. Now it is evident that the pyrometer so situated would indicate the mean heat of the whole of the muffle, which heat might and did vary in different parts. Of two pieces of silver, of the same size, placed within an inch of each other, one fused some time before the other. Every precaution was taken to place the metal to be tried as near as possible to that part where the mean heat probably existed, but still the method is not susceptible of extreme precision. Means might be contrived to surround the instrument with the metal in a state of fusion much in the same manner as it is surrounded with mercury for the purpose of graduation, but this would require particular opportunities which it is to be hoped that those will avail themselves of who have them in their power. The experiments were repeated more than once with a very close agreement of results, but the fusing point of silver is most to be relied on, as having been furnished by three different trials, all of them agreeing to a degree.

	Pyrom.	Therm.
Boiling point of mercury . . .	92 =	644
Fusing point of tin	63 =	441
Ditto bismuth . . .	66 =	462
Ditto lead	87 =	609
Ditto zinc	94 =	648

These four last points were all determined by placing the muffle over two Argand lamps.

	Pyrom.	Therm.
Fusing point of brass	267	= 1869
Ditto pure silver	319	= 2233
Ditto copper	364	= 2548
Ditto gold	370	= 2590
Ditto cast iron	497	= 3479
A red heat just visible in the day-light is about	140	= 980
The heat of a common parlour fire	163	= 1141

The difference between most of these results, and those of Mr. Wedgwood is indeed enormous. His determination of the same points is as follows:—

	Pyrom.	Therm.
Mercury boils	$-3.\frac{673}{1000}$	= 600
Red heat fully visible in the dark	−1.	= 947
Ditto in the day-light . .	0.	= 1077
Brass melts	+21	= 3807
Copper	27	= 4587
Fine silver	28	= 4717
Fine gold	32	= 5237
Cast iron	130	=17977

When the nature of the two pyrometers is considered, and the principles upon which they are founded, there will not exist, I trust, much doubt, as to the degree of confidence to which each is entitled. It must be recollected, that the equal expansion of platinum, with equal increments of heat, is one of the best established facts of natural philosophy, while the equal contraction of clay, is an assumption which has been disputed, if not disproved. The instrument which I have proposed, has the further advantage of confirming the indications of its ascent when heating, by its gradual return to its original state when cooling, an advantage which is totally uncompensated in that of Mr. Wedgwood. There is yet, another argument,

drawn from a very different method of reasoning, which I think will convince those who are at all conversant with the effects of heat upon metals, and the management of a furnace. Mr. Wedgwood fixes the heat of an enamelling furnace, at 1857°, and the fusing point of fine silver at very considerably more than double, viz., 4717°. Now, any body almost knows, how very soon silver melts after it has attained a bright red heat, and every practical chemist has observed it to his cost, when working with silver crucibles. Neither the consumption of fuel, nor the increase of the air-draught, necessary to produce this effect, can warrant us in supposing that the fusing point of silver is 4½ times higher than a red heat, fully visible in day-light. Neither on the same grounds, is it possible to admit that a full red-heat being 1077°, and the welding heat of iron 12,777°, that the fusing point of cast iron can be more than 5000° higher. The welding of iron must surely be considered as incipient fusion.

Much, however, very much remains to be done in this wide field of research; and when it is considered, what important results have arisen from the accurate estimation of the degrees of heat, comprehended within the scale of that invaluable instrument, the thermometer, it is surely sufficient to inspire ardour in our inquiries, into the almost boundless range of which that instrument measures comparatively so small a part. Happy indeed shall I be, if it shall be found that I have been fortunate enough to suggest the means of advancing one step in a pursuit, which promises so much benefit to science and the arts.

I shall now terminate this paper by the record of two facts, which although not in immediate connexion with the previous inquiry, have arisen collaterally from it, and I believe are new and worthy of attention. The first is, that mercury amalgamates readily with platinum at about its boiling temperature. The combination is very intimate, and it requires a strong red heat to volatilize the mercury from it; the platinum is then left in a honey-combed or dissected state.

The second is, that a piece of cast iron strongly heated, and afterwards slowly cooled in a muffle, became covered with

small but very distinct octohëdral and tetrahëdral crystals, of black oxide of iron. The specimen which I have preserved, is very pretty, and the facets of the crystals very perfect and brilliant.

The pyrometers are made and sold by Mr. Newman, Lisle-Street, to whose ready comprehension and execution of my ideas, I am much indebted.

ART. XII. *Reply to some further Observations and Experiments, relative to the Eighth Pair of Nerves, by Dr.* HASTINGS, *Physician to the Worcester Infirmary. By* S. D. BROUGHTON, *Mem. Roy. Col. of Sur., Surgeon to the St. George's and St. James's Dispensary, and in H. M.'s 2d Regt. of Life Guards; in a Letter to the* EDITOR.

DEAR SIR,

In the Observations and Experiments which you did me the favour to publish in January last, I endeavoured to avoid the mazes of speculation and argument, and to confine the attention of my readers to simple deductions from facts. Wishing to avoid every thing likely to promote discussion, my anticipations of setting the question at rest were rendered more confident from the knowledge, that my experiments tended to confirm the opinions of the best physiologists of the day, and probably of a large majority of the profession. Some observations upon my paper, having appeared in your journal for April last, by a Dr. Hastings of Worcester, containing *gross misrepresentations* of my statements, I am somewhat reluctantly compelled to appeal from so very unfair an attack.

When an Author appears purposely striving to misrepresent, some suspicion as to the motive which influences him will naturally arise; and, the impartial reader may probably entertain some doubts respecting the candour of an opponent, when informed that he is the pupil, assistant, and zealous advocate of the doctrines of the master, at whose "*Gamaliel foot*" he has received his education. In fact, Dr. Hastings is the "grand caterer and dry nurse" of Dr. Wilson Philip's experi-

ments, and nervous-galvanic theory. And accordingly while the great man is employed in grappling with "the grown serpent*," it naturally falls to the disciple's charge to deal with the "insolent worm," who presumes to turn upon his Master's doctrine.

The authorities which I quoted were merely intended to exhibit a general view of what is known, relative to the influence of the par vagum in the animal economy, and whether it made for, or against me, was a matter of indifference, as I had no object to serve, but that of submitting truth to the test of experiment, upon a point which seems never till of late, to have been particularly inquired into, though often noticed collaterally. That the result of my observations is not at variance with those quoted, (as Dr. Hastings asserts it to be,) no unbiassed reader can fail to perceive, from the very apparent circumstances of the *variety* of accounts given of the effects of dividing the eighth pair of Nerves, and the express opinion of Le Gallois, that, though the most obviously urgent symptom is often the affection of the stomach, yet he found the disturbance of its functions *extremely variable in degree* in different species of animals, and at different ages, and even in the same species of animals. Dr. Hastings endeavours to shew that Le Gallois does not favour the idea of the disturbance of the stomach arising from that of the lungs. But, it will be found on reference, that he particularly asserts it to be his opinion, that the affection of the stomach is a *secondary* effect, *not constant*, and *variable in degree*, (excepting efforts to vomit, which always occur from the first shock of the operation,) and that that of the thoracic viscera *is constant*, and by its influence on the circulation reduces the functions of life to a state which finally renders them incapable of continuance. So much for my being "contradicted by *all* former and cotemporary experience," and, that "no fact is better made out, than that the gastric secretions immediately cease on dividing the par vagum in the neck universally."

Among our cotemporaries, I am accused of omitting the

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* See the controversy between Dr. Wilson Philip, and Dr. Alison.

experiments of a Dr. Clarke Abel, another "Daniel come to judgment." But, though he may possibly be what the *transposition of his names* would unquestionably make him, it was sufficient for my purpose that I stated Dr. Wilson Philip did not stand alone and unsupported in his argument.

Dr. Hastings takes it upon himself to put the ignorant on their guard against being misled by my application of the term *digestion;* and explains to them what it really means. Possibly the reader, like the lady in the farce, may say, "Here are two very civil gentlemen trying to make themselves understood, but the interpreter is the most difficult to comprehend of the two."

Then we have a playful quibble about *mucus* and *chyme*, in which my meaning is ingeniously perverted. I used the word *mucus instead of chyme*, because I wished to state simply the fact as it appeared to me, leaving it open to others to judge from their own experience of the truth of my suggestions*. Had Dr. Hastings followed this general rule, by which I govern myself, in detailing his experiments, it would have been more fair, instead of adopting the *self ductum style*, and thus giving us no means of judging for ourselves. In place of saying such and such were the results of certain experiments, it seems to me more candid to note the *exact appearances*, leaving it open to others to form their own opinions as to the indications of such appearances. For, it is the great misfortune of almost every art and science, that their professors are too apt to imagine theories without the experience of facts, which they afterwards warp to support their own views, rather than in aiding the cause of truth; and which is pretty much the same as erecting a structure before the foundation is secured.

But I have yet a graver charge to bring forward; no less than a palpable misrepresentation, absolutely unwarranted by any ties or debts of gratitude, that may be due between Dr. Wilson

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* Dr. Wilson Philip says, this semi-fluid is the mucus of the stomach, and not chyme. I believe it to be the result of combination of gastric fluid with the parsley, and not secreted originally in the form described.

Philip and his tyro. The reader is informed that in the whole, or the greater part of my experiments no alteration in the food was observable, but in its different degrees of brownness. Whereas, the case really stands thus: not wishing to fatigue my readers with a repetition of the same detail of appearances after every experiment separately and in succession, I contented myself, having once named the appearances after death, with stating that *no deviation in them was observed, in the succeeding experiments, from those noticed antecedently, excepting the brown tint of the parsley;* thereby obviously meaning to imply, that *similar appearances shewed themselves in the succeeding experiments to those of the preceding.* How then can Dr. Hastings venture to state, as a man of veracity, that in a large majority of my experiments, nothing appeared in the food and the stomach when examined, but the brownish colour of the former, and thence argue that the food was unaltered? How can he suppose that I should be incautious enough to bring forward such evidences in proof of the continuance of digestion, if it were as he represents it? I regret that I am compelled to lengthen these remarks, but I must in justice to my own reputation, and to the cause I have espoused, contradict the following assertions:—

First, that I did not compare animals fasted and fed, but not operated upon, with those similarly prepared and operated on. My answer is, that I did so and *have mentioned it.* Secondly, that I did not observe after death whether the eighth pair of nerves in the dog, were divided. I again answer that I did so, and have mentioned it in this, and every other case. As to the objection that this dog may have vomited up his milk at night, since it does not appear that I watched him all night; I certainly did not feel it necessary that I should keep watch, but I took care to place him under such circumstances, as would have rendered it impossible for me not to have perceived it, if he had rejected any more milk after the last he took. Moreover how else came the *whey* to be found in the stomach, and the *curd disappeared,* but from the decomposition of the milk by the gastric fluid, and digestion of the curd?

Thirdly, that the nerves may have re-united. I again answer to this, that I found the divided ends of the nerves uniformly separate and apart from each other. Nor can I credit the possibility of so quick a re-union. From experiments performed by Mr. Sewel at the Veterinary College, I believe it to be impossible, and also that the re-union is slow and imperfect in its sensibility.

Fourthly, that the three horses cited, make against me most fatally. To this last charge I answer that the first died in an hour, so that no inspection was made of the stomach.

The objection to the second horse, which lived *fifty hours, and was quite well twenty-four of them,* is, that the food was not weighed before eaten, and after death. It is true, it was not, because, even Mr. Field's stable-boy could perceive, without the aid of scales, that the quantity of hay found in the stomach was so much less than that eaten, that (as a horse can't vomit,) it was *evident much had been digested, especially as the colon was empty.* As he was eating just before he died, this accounts for the small portion of hay in the stomach. The third horse, was a single instance (of fifteen experiments,) of no digestion having gone on, after division of the nerves; thus shewing the analogy between the experience of former, and cotemporary writers on the subject, and myself, as to the *variability of the effects,* always excepting the advocates of the doctrine which ascribes digestion entirely to the agency of the nerves of the eighth pair.

In the present general eagerness for information and novelty, some of other habits and callings take an interest in speculative inquiries; and among such, many may be seduced into a belief of the most visionary theories. It is right, therefore, that these should be put upon their guard, and assuredly just and natural that I should not allow the force of my experiments and remarks to hazard the danger attendant upon misrepresentations and idle quibbles, for the correction of which, this letter is solely written. Having discharged this duty, I beg leave to decline all further controversy, which might

otherwise become as interminable, as it is to all appearance, useless to the cause of science.

I remain, dear Sir, yours, &c.

S. D. Broughton.

Great Marlborough-St.,
April 1821.

P.S.—Since the above letter has lain at the Publisher's, I have been gratified by a personal acquaintance with Dr. Wilson Philip; who, with a degree of zeal unrivalled, and *candour* worthy of imitation in *others*, has been occupied of late in retracing the ground formerly trodden; for the purpose of settling the doubts cast upon his alleged results, to the satisfaction (if possible) of all parties. How far this desirable end is attained, the following notice will shew.

After the usual fasting, and subsequent feeding with parsley, the eighth pair of nerves were divided in the necks of three rabbits. One was submitted to the galvanic influence, after the usual manner of Dr. Philip, and the other two were left to themselves. After a few hours the two latter were successively killed, and *in both digestion was perfect.* The galvanised rabbit was then destroyed, and in the stomach of this animal the parsley had undergone *very little alteration.* I may add, with respect to the respiration of the latter rabbit, that, though there was a difference more or less observable at times in its breathing, yet, that it did not continue throughout the experiment to breathe freely and naturally. Whatever difference was observable was *in favour of the galvanic influence,* which was occasionally interrupted for a time.

Dr. Wilson Philip acknowledged (in the most candid manner,) the results of these experiments, so contrary to his alleged constant experience, and felt himself called upon to persevere in his endeavours, to ascertain the cause of such a striking difference between these and Dr. Hastings' results. Several rabbits were subsequently operated upon, in the usual manner, with one exception, and that, it appears, a most important one. The nerves being divided, pains were taken to keep their extremities asunder, so as entirely to prevent any contact

between them, a plan always adopted (says Dr. Philip,) by Dr. Hastings, who made *all* Dr. Wilson Philip's experiments for him. In all the rabbits so treated, and some, in which a piece of the nerve was cut out, including two rabbits sent by Dr. Hastings from Worcester after being killed, it was much to my surprise, that though in *all* there was more or less digested parsley to be found, yet in *none* had digestion gone on so completely, as in my own experiments, and in the two first rabbits mentioned above.

The difference indeed was very striking, so much so as to afford strong grounds for supposing that Dr. Wilson Philip was correct in believing that in the different modes of dividing the nerves was to be found the cause of the different results; and there is to be attributed a power in the nerves, to carry on the influence of the brain, after they are divided simply, without any obstruction to their subsequently coming into contact.

At the suggestion of Mr. Andrew Knight, who has taken much interest in these experiments, and afforded us the advantages of his ingenious and philosophic mind, Dr. Wilson Philip once again resorted to his usual experiment with galvanism, having two other rabbits simply operated upon (after the manner of Dr. Hastings,) as a comparative experiment.

The galvanised rabbit had remained singularly quiet the whole time, breathing freely, and with no more apparent distress than the twitches usually produced by the galvanic influence, which in this case was uninterruptedly kept up. The other rabbits laboured strongly in their breathing. They were all three killed at the same period, and their stomachs successively opened. In the two non-galvanised rabbits, digestion had *scarcely made any progress*, but in that galvanised, it was *perfect*, in the manner, to all appearance, avowed by Dr. Wilson Philip and his supporters. However we may differ in opinion, as to the real state of the food in the non-galvanised rabbits, as to Dr. Wilson Philip's theory, or, as to the cause of the formation of chyme and chyle being found under the influence of a galvanic battery, Dr. Wilson Philip cannot be denied the merit of correctness in his assertions, (hitherto almost univer-

sally distrusted) relative to the simple fact of a certain power of galvanism producing digestion, after dividing the eighth pair of nerves, under circumstances in which it is impeded without the galvanism.

It is also the merit of Dr. Wilson Philip, that he has discovered cause for believing that nerves can convey the influence of the brain, after being simply divided, and no means used to obstruct their extremities coming into contact.

It is proper to state that the President and several members of the Royal Society, and of the Colleges of Physicians and Surgeons, among whom were Mr. Brodie and myself, inspected the progress of these experiments, which were carried on under the constant superintendence of Dr. Wilson Philip.

I beg leave to take this opportunity of apologising to M. Majendie, who has honoured me by the publication of the greater part of my experiments in his *Journal of Physiology*, for stating that he had not performed similar experiments himself. The error arose from my not, at the time I wrote, having seen the second volume of his *Physiologie*.

It is a great gratification to me to find, that a physiologist of such high reputation as M. Majendie should concur with me on the subject of my experiments; and that his similar results, from dividing the nerves below their distribution to the lungs, favour the idea of the state of the respiration after dividing the nerves above being the probable cause of the interruptions to perfect digestion.

Great Marlborough-Street, June 1821.

ART. XIII. *A Letter to* Mr. SAMUEL PARKES, *occasioned by his* Observations *on the "Oil Question." By* RICHARD PHILLIPS, F.R.S.E. *&c.**

SIR,

It is not my intention to notice every statement contained in your "*Additional Observations on the Oil Question*; I shall

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* See *Journal of Science, Literature, and the Arts*, Nos XX. and XXI.

select certain passages to show the character of the means to which you have resorted, for the purpose of obtaining what you term "*triumphant success,*" and then leave you to enjoy it.

For the sake of reference I shall call your first paper the *Observations,* and the second the *Reply;* between the styles of which there is an amusing difference. In the *Observations* you nearly confine yourself to the statement, or misrepresentation or suppression of evidence; but in your *Reply* you take a more elevated flight, and quote Horace and Persius and Virgil, with a facility truly surprising to those who know you.

You must of course be right about *sweet acids* and *silent explosions:* the first expression is correct because you borrowed it; the second is absurd, because it was used by an opponent. *Explosion with noise,* a term which you employ and defend, does not necessarily imply, you say, that there may be *noiseless explosions;* and you inform us in the *Reply* that an *explosion* is "a hissing or an inferior kind of noise;" whilst in the Chemical Catechism, you describe an explosion as having nearly killed Pilatre de Rozier, although it took place only in his mouth; and in your laboratory an *explosion* or inferior hissing noise shattered several twelve-gallon glass receivers into ten thousand pieces. So much for your ridiculous contradictions; I shall presently notice your serious ones.

Speaking of the Associates (meaning those who replied to your *Observations**) you say, (*Reply,* p. 92, note) "they have made two calculations as to the quantity of oil which their vessel would hold, *viz.*, one at p. 44, and the other at p. 51 of their book. And though they are calculations which any schoolboy could have made, they are both erroneous."

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* See *Remarks* on a communication published in No. XX. of the *Journal of Science, and the Arts,* entitled 'Observations on the Chemical Part of the Evidence, given upon the late trial of the action brought by Messrs. Severn, King and Co., against the Imperial Insurance Company. By Samuel Parkes, F.L.S., M.G.S., &c.' By Richard Phillips, F.R.S.E. &c., Philip Taylor, J.G. Children, F.R.S., &c., John Martineau, Junr, John Bostock, M.D., F.R.S., &c., and John Taylor, M.G.S.

I deny this, and assert that they are both correct. Mr. Children, after mentioning the size of the vessel, *viz.*, 3 feet long, 15 inches wide and 15 inches deep, says "this measurement reduced to cubic inches gives the capacity of the vessel equal to 8100" (*Remarks*, p. 44.) Dr. Bostock having also quoted the dimensions of the vessel as above given, says, "which will give a capacity of 8,100 cubic inches". (*Remarks*, p. 51.) Now unless you are prepared to deny that $15 \times 15 = 225 \times 36 = 8100$, both these "calculations as to the quantity of oil the vessel would hold" are correct.

It is indeed true, that in one part of Dr. Bostock's statement, 6 is erroneously printed instead of 5; but the result is not affected by it, for he has correctly calculated both the capacity of the boiler, and the degree of expansion which the oil undergoes by heating. Mr. Children has certainly committed an error in allowing for the expansion of the oil, but why have you not pointed it out? Because to have made the correction would have convicted you of greater error; and therefore you applied your general rule for unpleasant and inconvenient evidence, and suppressed it. In adding two numbers together, Mr. Children intending to admit even more expansion than you state oil to undergo, allowed the unoccupied space of the boiler to be only 970 instead of 1170 cubic inches.

I next notice the following passage from p. 95 of your *Reply*, "But when I examine the volume which has been put forth, and perceive that the writers have not adduced one new experiment, nor have taken the least pains, &c. &c." Now in your *Observations* (p. 329, note) you say that you are convinced that "no inflammable gas is produced until a portion of the oil becomes actually decomposed, and charcoal formed;" and again, speaking of some gas which you obtained from oil, heated from 590° to 620°, you say, "I have no doubt but the gas, in both instances, was carbonic acid gas; for when cooled, and mixed with atmospheric air, it would not inflame on the application of a match." Once more, in p. 344 of your *Observations*, you say, with respect to "inflammable gas," "I have since the trial, proved by unequivocal experiments, that it cannot

be obtained but at a temperature much beyond what our thermometers will measure." Such were your opinions, previously to the printing of our *Remarks*, and let us examine what they contain on the subject. Mr. Philip Taylor, one of the Associates, states, (" volume which has been put forth," p. 30,) that having put a pint of whale oil into a retort, and heated it by a gas light to 620°, he procured an inflammable emanation; and he adds, " after agitating some of this gas with water, I tried its inflammability, and found it very similar to oil-gas. I then received a fresh portion into an air jar, graduated in cubic inches; fifty inches were thus collected, and after standing more than three weeks, in my experiment room, the absorption and condensation only amount to six cubic inches, and the gas continues to exhibit the appearance of oil-gas. There was no formation of charcoal in the retort during the production of the gas."

Now observe the dilemma to which you have reduced yourself: either you knew or you did not know that inflammable gas could be procured in the manner described by Mr. Taylor. If you did not know it then you must have perceived that the Associates have adduced one new experiment; and seeing its importance and its direct bearing on the question, you must have purposely suppressed it. But if, on the other hand, you did know that inflammable gas might be produced from oil without charcoal being formed; that it was not carbonic acid that was evolved at 620°; and if also you knew that inflammable gas might be obtained at a temperature not beyond what our thermometers will measure, then the triumph of which you boast is such as your friends must deplore, and the associates would have spared you.

The next passage which I shall notice occurs in page 98. Here, in consequence of having been charged with garbling and suppressing evidence, you say " can any candid and ingenuous person suppose that I should, without being publicly called upon, have undertaken to give a correct account of the chemical evidence that was adduced on this most important trial, and then have entered upon the task with a determination to conceal some parts, to garble and curtail others, and to misrepresent

and pervert what had been adduced by the defendants' witnesses, for the purpose of making a false impression on the public, &c.?" You must be tried by facts and not by professions; and I for one having charged you with concealing, garbling, curtailing and perverting evidence, repeat and will substantiate the charges.

I wish to know whether the public call to which you allude is a mere gratuitous assumption on your part, or rests upon satisfactory evidence: my inquiries have hitherto been unsuccessful, and I am curious to learn when, by whom, or through what channel, this demand was made upon your talents and experience.

With respect to your concealing evidence, several instances were exhibited in the Associates' Remarks; and to one of the most palpable you have not offered even the shadow of a reply; I mean, the charge which I brought against you, of entirely omitting the evidence of Mr. Daniell and Mr. Martineau; because it went to prove that there is no danger in boiling sugar in the common mode, and admitting and enforcing Mr. Robinson's evidence because he thought the boiling dangerous, although he confessed he never knew it set fire to a sugar-house: this I think amounts to concealment. Again, I call your attention to what you have stated in the *Observations*, p. 351, respecting the Juryman's opinion of the oil used in one of our experiments. You were a party to the conversation respecting it, and yet you take advantage of a mistake in the printing of the trial, to insert the word *not*, totally altering what the Juryman said; and not content with this, and effectually to misrepresent what he did say, you have suppressed one half of it; the Juryman is represented in the printed *Trial*, p. 212, as having said, "I think we need not trouble Mr. Taylor at all, we are not satisfied about the oil being pure." You, eager to retain this error of the print, and perceiving that the first part of the sentence was at variance with the second, omitted the words "I think we need not trouble Mr. Taylor at all," and you stated the matter merely thus, "After which one of the Jurymen said, We are not satisfied about the oil being pure." This is I think, an example of your method of curtailing and

perverting, and for this you have offered neither defence nor apology.

As to garbling evidence, your treatment of Wilkinson proves this in the clearest manner; he states, and you admit, that the Associates employed a boiler 3 feet long 15 inches wide and 15 deep; and they detail an experiment made with this boiler, in which owing to the formation of a volatile oil, in a fixed one the oil spouted out with considerable force, to the height of several feet. To invalidate this experiment, you have repeatedly stated that the boiler was nearly full, and that the violent expulsion of the oil, which the Associates attributed to the formation of vapour in a viscid fluid, was the effect of common expansion occasioned by heat, on account of the vessel being nearly full and this you assert to have been the case, knowing that Wilkinson had stated that only 24 gallons were put in, which would occupy but little more than two thirds of the boiler, and knowing that his evidence was corroborated by Dr. Bostock, Mr. Taylor, Mr. Faraday and myself. In your *Reply*, how do you attempt to escape from this charge of garbling Wilkinson's evidence? Why, by telling us that he was ignorant of some other matters, and by again garbling his evidence, to convict him of having stated an impossibility. With respect to Wilkinson's evidence generally, I assert that all which he stated *positively* would have been confirmed by Mr. Martineau, had confirmation been required; but to shew that he stated an impossibility, you say, in p. 100 of the *Reply*, that he "had measured 33 gallons of fresh oil into an iron vessel, which, according to his own showing would not hold more than 35 gallons, and had contrived to keep the oil *within this vessel* for five or six days, though it was generally kept at a temperature of 400°; and every one who has made experiments on the expansion of oil, must know that 33 gallons of whale oil measured at the usual temperature of the atmosphere in the month of February, (say from 40° to 50°) would, when brought to the temperature of 400° have measured thirty-nine gallons."

Now in order to convict Wilkinson of the impossibility thus described, you have purposely omitted the word "about" used

by him in his evidence, and you have supplied the omission by stating him to have said that he *measured* the oil;—but his words are (*Trial*, p. 147) "I put in about 33 gallons of whale oil." It is evident either that he spoke from memory, or without having measured the oil; and yet you, for the purpose of blasting this man's character, alter his evidence given to the best of his belief, into positive evidence.

In p. 106 of the *Reply*, you say, "Having mentioned the name of Mr. Faraday, it is but justice to that gentleman to state that he had the good sense and the virtue to resist all solicitations to become one of the party," viz., the Associates; I therefore addressed the following letter to Mr. Faraday, and received the annexed answer:

Dear Faraday,

Mr. Parkes, in his "Additional Observations on the Oil Question," has extolled your "good sense and virtue for having resisted all solicitations to become one of the party" who replied to him. I will therefore thank you to inform me, whether, if such solicitations were resisted by you, it arose from any alteration in your opinion as to the danger of heating by means of oil, the nature of the changes which it undergoes by heat, or as to the accuracy of the experiments made by yourself, or conjointly with others?

Yours, very sincerely,

June 3d, 1821. R. PHILLIPS.

Royal Institution, June 5th, 1821.

Dear Phillips,

In answer to your questions I have to state, that I have not been solicited to become one of the party; and that I remember only one occasion on which it was proposed to me to write in answer to Mr. Parkes's observations. I have also to state, that my sole reason for not writing, was disapprobation of controversy, and an opinion that it rarely convinces of error or answers any good purpose; and finally, that my opinion, with regard to the nature of the process, and the changes that take place in the oil, during the heating of it, as well as of the accuracy of the experiments that have been made, and on which that opinion was founded, not only remains unchanged, but is strengthened by my subsequent experience.

I am, dear Sir,

Yours, very truly,

M. FARADAY.

. In p. 108 of your *Reply*, alluding to the Directors of the Globe Insurance Company, you assert that "they not only completely abandoned the idea of danger from the oil apparatus," but that they "actually directed the counsel to declare in court, that they were perfectly satisfied, that this apparatus *lessened* the danger."

What truth there is in this declaration, may be learned from the following extract, from the short-hand writer's notes.

Lord Chief Justice Dallas.—"There is one fact, which one way or another would dispose of this cause at once, and save us hours or possibly days of investigation; that is, whether the oil process increased or decreased the danger of fire, or left it as it was before."

Mr. Serjeant Vaughan.—"We propose to prove that it decreases it, my lord."

Mr. Serjeant Bosanquet.—(for the defendants) "I shall certainly submit that is not in the cause." And afterwards,—"It is admitted that the premises were destroyed by an accidental fire, and the defendants agree not to contend that it was by the oil process. We reserve our own opinion, but we do not contend it in this cause."

In p. 112 of your *Reply*, you describe an experiment, in which, after raising oil to 600° you found it impossible to obtain any inflammable product—nay, at 610° the light was extinguished; this is very singular, considering what you have stated in various parts of your remarks. Thus, in your *Observations*, (p. 346,) you assert, in a tone which will have but few imitators, "I know the care with which I made my own experiments, and how the several experiments which were performed by different means corroborated one another; I am therefore satisfied that the results which I obtained were the true results." Now let us examine this agreement of which you boast; in p. 64 of the printed trial, you say that oil heated to 586° gave out inflammable gas, the flame was not permanent, "but at 600° it continued to burn," again, in the *Reply*, (p. 99, note,) you inform us that "oil vapour is not inflammable at the end of a tube, unless the oil

be heated to the temperature of about 800° of Fahrenheit." So that according to these careful corroborating experiments, "the true results" are that the vapour of oil at 600° is *not* inflammable, and *is* inflammable, and at 610° extinguishes flame. Although I am heartily tired, I cannot help adding one more example of your agreement in opinion with yourself. Alluding to the forcible expulsion of the oil, described by the Associates, you say (*Observations*, p. 342,) "I cannot conceive how the expansion of the vapour could throw out the oil; for, if the vessel could not hold the vapour and the oil, the natural consequence would have been for the vapour to escape through the tube and not the oil." In the *Reply* (p. 92) we are informed that "this vacuity [in the boiler] would be filled with vapour, in consequence of the large quantity of water which is always formed in oil at a high temperature; and this being generated faster than it could be carried off by the tube, would press with a force on the surface of the oil, that would be sufficient to produce all the effects which they have related;" that is, to drive the oil out of the boiler.

In concluding these remarks, I ask the public to consider what reliance can be placed either upon your own experiments, or upon the representations which you have made respecting the experiments of others; what reliance, I ask with confidence, can be placed upon a man, who, pretending to give an impartial account of the evidence offered during an important trial, suppresses the evidence of two gentlemen out of three who spoke to one particular point, because two out of the three differed from him in opinion; who took advantage of a misprint in the trial, to misrepresent the opinion of a juryman, and even suppresses one half of what that juryman said; who states that two calculations of the capacity of a boiler, are erroneous, which are clearly correct; who says that oil does not give out inflammable vapour without depositing charcoal, and without being heated beyond what our thermometers will measure, and yet, when he is told of these facts, denies that any new experiment is stated; who suppresses repeated evidence as to the quantity of oil which a boiler

contained, and in defiance of that evidence states it to have been nearly full; who converts evidence given to the best of belief, into positive evidence, in order to make it contradictory; who states Mr. Faraday to have resisted solicitations which he never heard of; who asserts that the Globe Insurance Company declared their opinion that the oil process lessened the danger, when their counsel declared that they reserved their opinion on the subject; who tells us that oil vapour at 600° is and is not inflammable; and lastly, who asserts that if a vessel contain oil and vapour, and the vapour be generated so fast that the vessel cannot contain both, that the vapour would be expelled and not the oil, but, who afterwards says that oil would be expelled and not the vapour.

Determining not to notice any future remarks of yours, on this subject, I am, yours, &c.,

R. PHILLIPS.

ART. XIV. *Proceedings of the Royal Society.*

The following papers, have been read at the table of the Royal Society, since our last report.

April 5. On the mean density of the earth, by CHARLES HUTTON, L.L.D.

On the separation of iron from other metals, by J. F. W. HERSCHEL, Esq.

12. On the restoration of a portion of the urethra in the perinæum, by H. EARLE, Esq., communicated by the President.

May 3. Observations on the variation of local heat made amongst the Garrow Hills, by D. SCOTT, Esq., in a letter to W. T. BRANDE, Esq., Sec., R.S.

On some subterraneous trees discovered at the foot of the cliffs, about a mile to the eastward of Mundsley, by Lieut. JEFFERSON MILES, R.N., in a letter to W. T. BRANDE, Esq., Sec., R.S.

Case of a diseased enlargement of the glands of the neck, by JOHN HOWSHIP, Esq., communicated by Sir EVERARD HOME, Bart., V.P.R.S.

May 10. Some remarks on Meteorology, by LUKE HOWARD, Esq.

A Calculation of some observations of the Solar Eclipse, on the 7th of September, 1820, by Mr. CHARLES RUMKER, communicated by Dr. THOMAS YOUNG, Sec. R. S. for For. Corres.

24. On the Anatomy of certain parts of the globe of the eye, by ARTHUR JACOB, M.D., communicated by Dr. JAMES MACARTNEY.

Art. XV.—ANALYSIS OF SCIENTIFIC BOOKS.

1. *A Dictionary of Chemistry, on the Basis of Mr. Nicholson's, in which the Principles of the Science are investigated anew, and its Applications to the Phenomena of Nature, Medicine, Mineralogy, Agriculture, and Manufactures, detailed. By* Andrew Ure, M.D., Professor of the Andersonian Institution, *&c. &c. With an Introductory Dissertation, containing instructions for converting the alphabetical Arrangement into a systematic Order of Study.*—London, 1821.

It appears to us, that the misconception of a single word, has frequently led chemical teachers and writers into strange errors of arrangement. The term *Element* in reference to philology, arithmetic, geometry, and music, denotes not only whatever is most elementary in principle, but whatever is simplest in conception, in these sciences. From this two-fold concurrence, we are warranted in employing the letters of the alphabet, numeral notation, general axioms, and the diatonic scale, as the primary objects of contemplation to their students. But *chemical elements* appertain to a far different order of conceptions. Instead of being the objects which naturally present themselves at first to the inquirer, they form, for the most part, the ultimate points of his researches. Their simplicity is but a name relative to the stage of our advancement in the science. The bodies which we at present call chemical elements, are probably all compounds; and are certainly the least easy of apprehension to the learner, because they possess properties the most remote from those of the bodies with which his senses are daily conversant, and are disentangled from combination by processes not a little circuitous and intricate.

On the other hand, as the common and obvious properties of matter constitute the elementary principles of general physics, these justly form the initiatory propositions. This condition of natural philosophy allows the plan of tuition to resemble an architectural process, in which a house of a symmetrical plan is regularly raised from its foundations. But the actual state of chemistry is not susceptible of the same comparison. It may be likened more properly to a tree, whose trunk and main branches are easily traced and delineated, but the efflorescence which terminates the boughs, requires the most delicate microscopic research before we can discover the vital germ, the true element of fructification. Hence we perceive that elementary instruction in chemistry, is not that which treats, first of the elementary or undecompounded bodies; but, that which beginning with the familiar and tangible objects of study,

like the trunk and branches in the preceding comparison, progressively advances to the more recondite. For this reason, we think that one of the earliest subjects to which the teacher ought to direct a pupil's mind, is *water*; first, in its various forms as modified by temperature; and next, as to purity and composition. The disengagement of its two elements from their liquid repose, having familiarized the mind to gaseous existences, he will then readily enter on the chemical examination of the *atmosphere*, and of the curious bodies resulting from the union of its two constituents in successive multiple proportions. An acquaintance with gasometry, and with Gay-Lussac's beautiful doctrine of volumes, both essential to the ulterior investigations, will thus be insensibly formed.

The next transition in this *natural* order of tuition, may be to the well known combustibles, *sulphur* and *charcoal*, and to their several compounds with the bodies previously examined. *Common salt* ought now to be carefully studied, particularly in reference to its constituent *chlorine*. The ores of a few leading metals, with the principles of their reduction, and the metallic oxides, chlorides, and alloys, may now come under review; after which the alkalis, earths, and their saline combinations, with the acids previously examined, viz., the sulphuric, nitric, and carbonic, will be natural objects of research.

By such a course of experimental discipline, the diligent student will acquire, almost imperceptibly, or at least without any perplexity of intellect, a precise conception of the principal objects and methods of chemical research. He may now proceed to examine the various acids and their bases, and the combinations of one or other of these, with the metallic oxides and metals. The salts, and the native mineral and organic productions, will conclude his course. In proportion as he proceeds, the general facts of combination and decomposition, or the laws of chemical affinity, will be developed and fixed in his mind.

Such may be reckoned the best mode of procedure in a course of private study; but transferred to a book, it would evidently produce apparent disorder, though in reality far more conducive to the instruction of the *regular* reader, than those works which place the elementary bodies at the beginning, and the more familiar bodies, or native compounds, towards the end. This dilemma, between what best promotes the symmetry of a printed treatise, and the edification of its reader, is, with regard to arrangement, nearly unavoidable in the present state of chemistry. The perception of this difficulty has led chemists, at different times, to resort, with much advantage, to the dictionary form, or alphabetical order, for describing the objects and phenomena of their science. Macquer's Dictionary was long a popular work all over Europe,

highly creditable to its celebrated author, and was several times republished in the English language. Nicholson constructed, in 1795, on that model, his quarto dictionary, a work distinguished for perspicuity of style, and parallel views of the lately co-reigning chemical hypotheses, the phlogistic and antiphlogistic. In 1808, after having conducted his Philosophical Journal with candour, diligence, and urbanity, he published his octavo dictionary of chemistry, which, though compiled with less diligence and discernment, than his high character as a journalist gave reason to expect, was well received by the public. It presented, at a moderate price, and in a condensed form of typography, a great accumulation of chemical details. Nothing, however, shews more remarkably the slovenliness with which several of the articles were got up, than a comparison of them with the corresponding articles in Messrs. Aikin's quarto dictionary, published the preceding year. In almost every thing belonging to mineral analysis, and particularly to that of the ores, Mr. Nicholson was content to transcribe, or rather to reprint from his former dictionary, the obsolete and defective processes of Cramer, instead of drawing his analytical methods from the more recent and valuable researches of Vauquelin, Klaproth, and Hatchett. Hence, though Nicholson's octavo dictionary, from its price and form, had an extensive sale among chemical students and manufacturers, it never possessed much authority with men of science.

After an interval of twelve years from its publication, in which eventful period, discoveries of greater splendour, variety, and importance, had been added to the science, than during a century before, the proprietor of the copyright of the book took it into his head to print a new edition, and requested Dr. Ure, as we learn from the preface, to superintend its revision for the press. It would appear, that the Doctor had contracted the serious obligation of editorship, for a very trifling sum, without duly considering the great difficulty of revising and printing, within six months, a multifarious work, which required to be, for the greater part, re-written; and that, after the agreement had been made, he was left to struggle through the irksomeness of his task, with no hope of recompense, except the credit of its execution, or the consciousness of deserving well of the chemical world. "The dissertations," says Dr. Ure, "on *Caloric*, *Combustion*, *Dew*, *Distillation*, *Electricity*, *Gas*, *Light*, *Thermometer*, &c., which form a large proportion of the volume, are beyond the letter and spirit of my engagement with the publisher. I receive no remuneration for them, not even at the most moderate rate of literary labour; they are, therefore, voluntary contributions to the chemical student, and have been substituted for what I deemed frivolous and uninteresting details, on some unimportant dye-stuffs, and

articles from old dispensatories, such as althea, chamomile, &c." After this statement, he would be an unreasonable critic who should censure Dr. Ure for having re-printed from the old work several indifferently-written articles, since those which he has himself composed, as denoted by asterisks, would constitute nearly three volumes of the ordinary octavo form.

The memoirs which have been published in the *Philosophical Transactions*, and in this journal, by Dr. Ure, would satisfy the world, that, during his long career in public teaching, he has not been a passive spectator of chemical events, but that he has devoted a large proportion of his time to experimental research, without which discipline, indeed, the demonstrations of a Professor, however showy, will possess neither unity of design, nor authoritative force. He would become merely the retailer of another's wares, of whose value, genuineness, and mode of production, he was incompetent to judge. The dictionary affords abundant evidence that its author does not belong to this class of teachers.

The promise held out in the title-page seems to be honestly fulfilled; for he has, on several important occasions, *investigated anew* the principles of chemical science; and has, in consequence, rectified many errors which had gained currency in our compilations. His general views of chemical theory appear to be, for the most part, much sounder than those which have been re-echoed, with unvarying monotony, in what have been called chemical systems. He has, in particular, bestowed much pains in arranging the valuable facts belonging to the English school of chemistry, which, originating with the faultless memoirs of Cavendish, Hatchett, and Howard, on the Baconian plan of research, has finally, under Dalton, Wollaston, and Sir H. Davy, risen to undisputed pre-eminence among the schools of Europe. The numerous facts inconsistent with the Lavoisierian creed are carefully detailed, and the just inferences drawn from them, both with regard to the theory of acidification and combustion.

The general article Acid, in the dictionary, presents, in the first place, Lavoisier's notion of the origin of acidity, with Berthollet's judicious remarks on it. Dr. Ure then exhibits Sir H. Davy's more just and comprehensive views of acid constitution; and concludes with Dr. Murray's hypothetical modification of these views, which he successfully controverts.

"The more recent investigations of chemists on fluoric, hydriodic, and hydrocyanic acids have brought powerful analogies in support of the chloridic theory, by shewing that hydrogen alone can convert certain undecompounded bases into acids well characterized, without the aid of oxygen. Dr. Murray indeed has endeavoured to revive and new-model the early opinion of Sir H. Davy, concerning the necessity of the presence of water, or its elements, to the constitution of acids. He conceives that many acids are ternary compounds of a radical with oxygen and hydrogen; but that the two latter ingredients do not necessarily exist in them in the state

of water. Oil of vitriol, for instance, in this view, instead of consisting of 81.5 real acid, and 18.5 water in 100 parts, may be regarded as a compound of 32.6 sulphur + 65.2 oxygen + 2.2 hydrogen. When it is saturated with an alkaline base, and exposed to heat, the hydrogen unites to its equivalent quantity of oxygen, to form water which evaporates, and the remaining oxygen and the sulphur combine with the base. But when the acid is made to act on a metal, the oxygen partly unites to it, and hydrogen alone escapes."

"Carbonic acid he (Dr. Murray) admits to be destitute of hydrogen; yet its saturating power is very conspicuous in neutralising dry lime. Now, oxalic acid, by the last analysis of Berzelius, contains no hydrogen. It differs from the carbonic only in the proportion of its two constituents. And oxalic acid is appealed to by Dr. Murray as a proof of the superior acidity bestowed by hydrogen.

"On what grounds he decides carbonic to be a feebler acid than oxalic, it is difficult to see. By Berthollet's test of acidity, the former is more energetic than the latter, in the proportion of 100 to about 58; for these numbers are inversely as the quantity of each requisite to saturate a given base. If he be inclined to reject this rule, and appeal to the decomposition of the carbonates by oxalic acid, as a criterion of relative acid power, let us adduce his own commentary on the statical affinities of Berthollet, where he ascribes such changes not to a superior attraction in the decomposing substance, but to the elastic tendency of that which is evolved. Ammonia separates magnesia from its muriatic solution at common temperatures; at the boiling heat of water, magnesia separates ammonia. Carbonate of ammonia, at temperatures under 230°, precipitates carbonate of lime from the muriate; at higher temperatures, the inverse decomposition takes place with the same ingredients. If the oxalic be a more energetic acid than the carbonic, or rank higher in the scale of acidity, then, on adding to a given weight of liquid muriate of lime, a mixture of oxalate and carbonate of ammonia, each in equivalent quantity to the calcareous salt, oxalate of lime ought alone to be separated. It will be found, on the contrary, by the test of acetic acid, that as much carbonate of lime will precipitate as is sufficient to unsettle these speculations."

Under Acid (Arsenious) we find a very copious account of its poisonous operation on the living body, as also of its tests and antidotes.

"We may here remark, however, that the most elegant mode of using all these precipitation reagents is upon a plane of glass, a mode practised by Dr. Wollaston in general chemical research, to an extent, and with a success, which would be incredible in other hands than his. Concentrate by heat in a capsule the suspected poisonous solution, having previously filtered it if necessary. Indeed, if it be very much diluted with animal or vegetable matters, it is better first of all to evaporate to dryness, and by a few drops of nitric acid to dissipate the organic products. The clear liquid being now placed in the middle of the bit of glass, lines are to be drawn out from it in different directions. To one of these a particle of weak ammoniacal water being applied, the weak nitrate of silver may then be brushed over it with a hair pencil. By placing the glass in different lights, either over white paper or obliquely before the eye, the slightest change of tint will be perceived. The ammoniaco-acetate should be applied to another filament of the drop, deut-acetate of iron to a third, weak ammoniaco-acetate of cobalt to a fourth, sulphuretted water to a fifth, lime-water to a sixth, a drop of violet syrup to a seventh, and the two galvanic wires at the opposite edges of the whole. Thus with one single drop of solution many exact experiments may be made."

A plane of white paper answers extremely well for the same microscopic tests, as recommended by Dr. Paris.

The proportional or atomic weight of alum, or prime equivalent, as Dr. Ure calls it, he deduces from Sir H. Davy's experiments, to be 32, and not 21.36, as assigned by Berzelius,

for Dr. Wollaston's scale. Our author seems successful in reconciling the constitution of alum with the former number.

"It deserves to be remarked (says he), that the analysis of Professor Berzelius agrees with the supposition that alum contains

4 sulphuric acid . . .	=	20.0	34.36
2 alumina	=	6.4	11.00
1 potash	=	6.0	10.30
28 water	=	25.8	44.34
		58.2	100.00

"If we rectify Vauquelin's erroneous estimate of the sulphate of barytes, his analysis will also coincide with the above. Alum, therefore, differs from the simple sulphate of alumina previously described, which consisted of three prime equivalents of acid, and two of earth, merely by its assumption of a prime of sulphate of potash."

Under ALUMINITE, he shews that this mineral

"May be represented very exactly by

2 primes of acid		10.	= 20.
5 ——— alumina . . .		16.	= 32.
21 ——— water		23.6	= 47.2
Foreign matter . .		0.4	= 0.8
		50.0	100.0

"The conversion of the above into alum is easily explained. When the three primes composing bisulphate of potash come into play, they displace precisely three primes (or atoms) of alumina. Two additional primes of water are also introduced at the same time, by the strong affinity of the bisulphate for the particles of that liquid.'

Subjoined to the article ATTRACTION (CHEMICAL) we were glad to see Dr. Young's ingenious tables of affinity, which have been unaccountably omitted in our large systematic works.

CALORIC is a very elaborate article, and, considering the circumstances under which the dictionary was re-written, it is one which will be read with interest. His division of the subject seems to be clear and comprehensive.

"Enough has now been said to shew how little room there is to pronounce dogmatic decisions on the abstract nature of heat. If the essence of the cause be still involved in mystery, many of its properties and effects have been ascertained, and skilfully applied to the cultivation of science and the uses of life.

"We shall consider them in the following order:

"1. Of the measure of temperature.

"2. Of the distribution of heat.

"3. Of the general habitudes of heat with the different forms of matter."

The *distribution* of heat he subdivides into two parts: 1st., the mode of distribution, or the laws of cooling, and the communication of heat, among aëriform, liquid, and solid substances; and, 2dly, the specific heats of different bodies, at the same and at different temperatures. In treating of the third head, the general habitudes of heat with the different forms of matter, he says,

"The effects of heat are either transient or physical; or permanent and chemical, inducing a durable change in the constitution of bodies. The second mode of operation we shall treat of under COMBUSTION. The first falls to be discussed here; and divides itself naturally into the two heads, of changes in the volume of bodies while they retain their form, and changes in the state of bodies."

Several newly constructed and very valuable tables will be found interspersed through this article. The following practical applications of the doctrines previously inculcated, will afford an example of our author's powers of popular description:

"But the most splendid trophy erected to the science of caloric, is the steam-engine of Watt. This illustrious philosopher, from a mistake of his friend Dr. Robison, has been hitherto defrauded of a part of his claims to the admiration and gratitude of mankind. The fundamental researches on the constitution of steam, which formed the solid basis of his gigantic superstructure, though they coincided perfectly with Dr. Black's results, were not drawn from them. In some conversations with which this great ornament and benefactor of his country honoured me, a short period before his death, he described, with delightful *naiveté*, the simple, but decisive, experiments by which he discovered the latent heat of steam. His means and his leisure not then permitting an extensive and complex apparatus, he used apothecaries' phials. With these, he ascertained the two main facts, first, that a cubic inch of water would form about a cubic foot of ordinary steam, or 1728 inches; and that the condensation of that quantity of steam would heat six cubic inches of water from the atmospheric temperature to the boiling point. Hence he saw that six times the difference of temperature, or fully 900° of heat, had been employed in giving elasticity to steam; which must be all abstracted before a complete vacuum could be procured under the piston of the steam-engine. These practical determinations he afterwards found to agree pretty nearly with the observations of Dr. Black. Though Mr. Watt was then known to the Doctor, he was not on those terms of intimacy with him, which he afterwards came to be, nor was he a member of his class.

"Mr. Watt's three capital improvements, which seem to have nearly exhausted the resources of science and art, were the following:—1. The separate condensing chest, immersed in a body of cold water, and connected merely by a slender pipe with the great cylinder, in which the impelling piston moved. On opening a valve or stop-cock of communication, the elastic steam which had heated the pendulous piston, rushed into the distant chest with magical velocity, leaving an almost perfect vacuum in the cylinder, into which the piston was forced by atmospheric pressure. What had appeared impossible to all previous engineers was thus accomplished. A vacuum was formed without cooling the cylinder itself. Thus it remained boiling hot, ready the next instant to receive and maintain the elastic steam. 2. His second grand improvement consisted in closing the cylinder at top, making the piston-rod slide through a stuffing box in the lid, and causing the steam to give the impulsive pressure, instead of the atmosphere. Henceforth the waste of heat was greatly diminished. 3. The final improvement was the double impulse, whereby the power of his engines, which was before so great, was almost at once more than doubled. The counter-weight required in the single stroke engine, to depress the pump-end of the working beam, was now laid aside. He thus freed the machine from a dead weight or drag of many hundred pounds, which had hung upon it from its birth, about seventy years before.

"The application of steam to heat apartments, is another valuable fruit of these studies. Safety, cleanliness, and comfort, thus combine in giving a genial warmth for every purpose of private accommodation, or public manufacture. It has been ascertained, that *one cubic foot* of a boiler will heat about *two thousand feet* of space, in a cotton-mill, whose average heat is from 70° to 80° Fahr. And if we allow 25 cubic feet of a boiler for a horse's power in a steam-engine supplied by it, such a boiler would be adequate to the warming of fifty thousand cubic feet of space. It has been also ascertained that one square foot of surface of steam-pipe is adequate to the warming of two hundred cubic feet of space. This quantity is adapted to a well-finished ordinary brick or stone building. The safety valve on the boiler should be loaded with two pounds and a half for an area of a square inch, as is the rule for Mr. Watt's engines. Cast-iron pipes are preferable to all others, for the diffusion of heat. Freedom of expansion must be allowed, which in cast-iron may be taken at about a tenth of an inch for every ten feet in length. The pipes should be distributed within a few inches of the floor.

"Steam is now used extensively for drying muslin and calicoes. Large cylinders are filled with it, which, diffusing in the apartment a temperature of 100° or 120°, rapidly dry the suspended cloth. Occasionally the cloth is made to glide in a serpentine manner closely round a series of steam cylinders, arranged in parallel rows. It is thus safely and thoroughly dried in the course of a minute. Experience has shewn, that bright-dyed yarns like scarlet, dried in a common *stove* heat of 128°, have their colour darkened, and acquire a harsh feel; while similar hanks, laid on a *steam* pipe heated up to 165°, retain the shade and lustre they possessed in the wetted state. The people who work in steam drying rooms are healthy, those who were formerly employed in the stove-heated apartments, become soon sickly and emaciated. These injurious effects must be ascribed to the action of cast iron, at a high temperature, on the atmosphere."

The true theory of the formation of coal gas is clearly stated in the following short paragraph

"If coal be put into a cold retort, and slowly exposed to heat, its bitumen is merely volatilized in the state of condensible tar, little gas, and that of inferior illuminating power, is produced. This distillation temperature may be estimated at about 600° or 700° F. If the retort be previously brought to a bright cherry-red heat, then the coals, the instant after their introduction, yield a copious supply of good gas and a moderate quantity of tarry and ammoniacal vapour. But when the retort is heated to nearly a white incandescence, the part of the gas richest in light, is attenuated into one of inferior quality, as I have shewn in detailing Berthollet's experiments on CARBURETTED HYDROGEN. A pound of good cannel coal, properly treated in a small apparatus, will yield five cubic feet of gas, equivalent in illuminating power to a moulds candle in the pound. See CANDLE.

We meet with a number of useful rules for conducting chemical computations, in different parts of the dictionary. The reader will find several curious ones in the first article, as well as under GAS. The article *Combustion* may be regarded as presenting Dr Ure's ideas of chemical theory. We shall quote one or two passages from it

"COMBUSTION. The disengagement of heat and light which accompanies chemical combination. It is frequently made to be synonymous with inflammation, a term which might be restricted, however, to that peculiar species of combustion in which gaseous matter is burned. Ignition is the incandescence of a body, produced by extrinsic means, without change of its chemical constitution.

"Becher and Stahl, feeling daily the necessity of fire to human existence, and astonished with the *metamorphoses* which this power seemed to cause charcoal, sulphur, and metals to undergo, came to regard combustion as the single phenomenon of chemistry. Under this impression, Stahl framed his chemical system, the *Theoria Chimiæ Dogmaticæ*, a title characteristic of the dogmatic spirit with which it was inculcated by chemical professors, as the infallible code of their science for almost a century. When the discoveries of Scheele, Cavendish, and Priestley, had fully demonstrated the essential part which air played, in many instances of combustion, the French school made a small modification of the German hypothesis. Instead of supposing, with Stahl, that the heat and light were occasioned by the *emission* of a common inflammable principle from the combustible itself, Lavoisier and his associates dexterously availed themselves of Black's hypothesis of latent heat, and maintained, that the heat and light emanated from the oxygenous air, at the moment of its union or fixation with the inflammable basis. How thoroughly the chemical mind has been perverted by these conjectural notions, all our existing systems of chemistry, with *one* exception, abundantly prove.

"Dr. Robison, in his preface to Black's lectures, after tracing, with perhaps superfluous zeal, the expanded ideas of Lavoisier to the neglected germs of Hooke and Mayhow, says, 'This doctrine concerning combustion, the great, the characteristic phenomenon of chemical nature, has at last

received almost universal adoption, though not till after considerable hesitation and opposition; and it has made a complete revolution in chemical science.' The French theory of chemistry, as it was called, or hypothesis of combustion as it should have been named, was for some time classed in certainty with the theory of gravitation. Alas! it has vanished with the luminous phantoms of the day; but the sound logic, the pure candour, the numerical precision of inference, which characterise Lavoisier's elements, will cause his name to be held in everlasting admiration.

"It was the rival logic of Sir H. Davy, aided by his unrivalled felicity of investigation, which first recalled chemistry from the pleasing labyrinths of fancy, to the more arduous but far more profitable and progressive career of reason. His researches on combustion and flame, already rich in blessings to mankind, would alone place him in the first rank of scientific genius. I shall give a pretty copious account of them, since by some fatality it has happened, that in our best and largest system, where so many pages are devoted to the reveries of ancient chemists, the splendid and useful truths made known by the great chemist of England have been totally overlooked.

"Whenever the chemical forces which determine either combination or decomposition are energetically exercised, the phenomena of combustion, or incandescence with a change of properties, are displayed. The distinction, therefore, between supporters of combustion and combustibles, on which some late systems are arranged, is frivolous and partial. In fact, one substance frequently acts in both capacities, being a supporter *apparently* at one time, and a *combustible* at another. But in both cases the heat and light depend on the same cause, and merely indicate the energy and rapidity with which reciprocal attractions are exerted.

"Thus, sulphuretted hydrogen is a combustible with oxygen and chlorine; a supporter with potassium. Sulphur, with chlorine and oxygen, has been called a combustible basis; with metals it acts the part of a supporter; for incandescence and reciprocal saturation result. In like manner, potassium unites so powerfully with arsenic and tellurium as to produce the phenomena of combustion. Nor can we ascribe the phenomena to extrusion of latent heat, in consequence of condensation of volume. The protoxide of chlorine, a body destitute of any combustible constituent, at the instant of decomposition, evolves light and heat with explosive violence; and its volume becomes one fifth greater. Chloride and iodide of azote, compounds alike destitute of any inflammable matter, according to the ordinary creed, are resolved into their respective elements with tremendous force of inflammation; and the first expands into more than 600 times its bulk. Now, by the prevailing hypothesis of latent heat, instead of heat and light, a prodigious *cold* ought to accompany such an expansion. The chlorates and nitrates, in like manner, treated with charcoal, sulphur, phosphorus, or metals, deflagrate or detonate, while the volume of the combining substances is greatly enlarged. The same thing may be said of the nitrogurets of gold and silver. In truth, the combustion of gunpowder, a phenomenon too familiar to mankind, should have been a bar to the reception of Lavoisier's hypothesis of combustion. The subterfuges which have been adopted, and admitted, in order to reconcile them, are unworthy to be detailed.

"From the preceding facts, it is evident, 1st, That combustion is not necessarily dependent on the agency of oxygen; 2d, That the evolution of the heat, is not to be ascribed simply to a gas parting with its latent store of that ethereal fluid, on its fixation, or combustion; and, 3dly, That 'no peculiar substance or form of matter is necessary for producing the effect, but that it is a *general* result of the actions of any substances possessed of strong chemical attractions, or different electrical relations, and that it takes place in all cases in which an intense and violent motion, can be conceived to be communicated to the corpuscles of bodies.'

"All chemical phenomena indeed may be justly ascribed to motions among the ultimate particles of matter, tending to change the constitution of the mass.

"It was fashionable for awhile, to attribute the caloric evolved in combustion, to a diminished capacity for heat of the resulting substance. Some phenomena, inaccurately observed, gave rise to this generalization. On this subject I shall content myself with stating the conclusions to which MM. Dulong and Petit have come, in consequence of their own recent re-

searches on the laws of heat, and those of Berard and Delaroche. 'We may likewise,' say these able chemists, 'deduce from our researches another very important consequence for the general theory of chemical action; that the quantity of heat developed at the instant of the combination of bodies, has no relation to the capacity of the elements, and that in the greatest number of cases, this loss of heat is not followed by any diminution in the capacity of the compounds formed. Thus, for example, the combination of oxygen and hydrogen, or of sulphur and lead, which produces so great a quantity of heat, occasions no greater alteration in the capacity of water, or of sulphuret of lead, than the combination of oxygen with copper, lead, silver, or of sulphur with carbon, produces in the capacities of the oxides of these metals, or of carburet of sulphur.—We conceive, that the relations which we have pointed out between the peculiar heats of simple bodies, and of those of their compounds, prevent the possibility of supposing, that the heat developed in chemical actions owes its origin merely to the heat produced by changes of state of the supposed combined with the material molecules.'—*Annales de Chimie et de Physique.*

"Mr Dalton, in treating of the constitution of elastic fluids, lays it down as an axiom that every unit of volume of the mixture exercises its mechanical affinity being exercised another remains that the mixture is a mere mixture. Thus also, the extrication of heat in mechanical mixtures is usually referred to the condensation of the gases; the following examples will shew the fallacy of such reasoning. Oxygen and hydrogen mixed explode by the electric spark, and the volume of the mixture is diminished at the instant of combination; but the heat evolved is common to both gases, and is produced as much when the oxygen is expanded to three, four, or five volumes, as when it is confined to one. Nitrous oxide is formed with about 110 hundredths of its bulk of oxygen. The following experiment of Mr Gay-Lussac is equally unfavourable to the same purpose. Two saturated solutions of muriate of ammonia, at the temperature of 61° and of the density 1·3, were mixed with water in the proportion of 14·01 to 3·76. The temperature of the mixture rose but the density at 61° was 1·19 while the mean density was only 1·01 2. On adding water to the preceding mixture, in the proportion of 3·61 to 39·26, the temperature sank 1°, while the density continued 0·00 by the mean. Other saline solutions presented the same result, though none to so great a degree.

"That the internal motions which accompany the change in the *mode* of combination, independent of change of *form* or action by the exclusion of heat and light, is evident from the following observations of Berzelius. In the year 1811, when he was occupied with examining the combinations of antimony, he discovered, accidentally, that several of these intimonates, when they began to grow red hot, exhibit a sudden appearance of fire, and then the temperature again sinks to that of the surrounding combustibles. He made numerous experiments to elucidate the nature of this appearance, and ascertained that the weight of the oxygen was not altered, and that the appearance took place without the presence of oxygen. Before the appearance of fire, the oxides are very easily decomposed, but afterwards they are attacked neither by acids nor alkalies;—a proof that their constituents are now held together by a stronger affinity, or that they are more intimately combined. Since that time he has observed these appearances in many other bodies, as, for example, in green oxide of chromium, the oxides of tantalum and rhodium.—See Chromium.

"Mr Edmund Davy found, that when a neutral solution of platinum was precipitated by hydro-sulphuret of potash, and the precipitate dried in air deprived of oxygen, a black compound was obtained, which when heated out of the contact of air, gave out sulphur, and some sulphuretted hydrogen gas, while a combustion similar to that in the formation of the metallic sulphurets appeared, and common sulphuret of platinum remained behind. When we heat the oxide of rhodium, obtained from the soda-muriate, water first comes over, and on increasing the temperature, combustion takes place, oxygen gas is suddenly disengaged, and a suboxide of rhodium remains behind. The two last cases are analogous to that of the

protoxide of chlorine, the *euchlorine* of Sir H. Davy. Gadolinite, the siliciate of yttria, was first observed by Dr. Wollaston to display a similar lively incandescence. The variety of this mineral, with a glassy fracture, answers better than the splintery variety. It is to be heated before the blow-pipe, so that the whole piece becomes equally hot. At a red-heat it catches fire. The colour becomes greenish-grey, and the solubility in acids is destroyed. Two small pieces of gadolinite, one of which had been heated to redness, were put in aqua regia; the first was dissolved in a few hours; the second was not attacked in two months. Finally, Sir H. Davy observed a similar phenomenon on heating hydrate of zirconia.

"The *verbal* hypothesis of thermoxygen by Brugnatelli, with Dr. Thomson's supporters, partial supporters, and semicombustion, need not detain us a moment from the substantial facts, the noble truths, first revealed by Sir H. Davy, concerning the mysterious process of combustion. Of the researches which brought them to light it has been said, without any hyperbole, that 'if Bacon were to revisit the earth, this is exactly such a case as we should choose to place before him, in order to give him, in a small compass, an idea of the advancement which philosophy has made since the time, when he had pointed out to her the route which she ought to pursue.'

"The coal mines of England, alike essential to the comfort of her population and her financial resources, had become infested with fire-damp, or inflammable air, to such a degree as to render the mutilation and destruction of the miners, by frequent and tremendous explosions, subjects of sympathy and dismay to the whole nation. By a late explosion in one of the Newcastle collieries, no less than one hundred and one persons perished in an instant; and the misery heaped on their forlorn families, consisting of more than three hundred persons, is inconceivable. To subdue this gigantic power was the task which Sir H. Davy assigned to himself; and which, had his genius been baffled, the kingdom could scarcely hope to see achieved by another. But the stubborn forces of nature can only be conquered, as Lord Bacon justly pointed out, by examining them in the nascent state, and subjecting them to experimental interrogation, under every diversity of circumstance and form. It was this investigation which first laid open the hitherto unseen and inaccessible sanctuary of *Fire*."

We recommend the whole article to the diligent perusal of our readers. He has transplanted with fidelity the beautiful and invaluable facts, first disclosed to the world in Sir H. Davy's papers on flame, published in the *Philosophical Transactions*. Dr. Ure arranges the phenomena of combustion under six heads:—

"1*st*, The temperature necessary to inflame different bodies. 2*d*, The nature of flame, and the relation between the light and heat which compose it. 3*d*, The heat disengaged by different combustibles in burning. 4*th*, The causes which modify and extinguish combustion, and of the safe-lamp. 5*th*, Invisible combustion. 6*th*, Practical inferences.

Our author has announced in the introduction, a systematic work on chemistry.

"If the public," says he, "after this larger specimen of my chemical studies, shall deem me qualified for the task, I may promise its completion within a year from this date. The work will be comprised in four octavo volumes, and will contain the results of numerous investigations into the various objects of practical chemistry, joined to a systematic view of its principles. By several simple instruments, tables, and rules of calculation, chemical analysis, the highest and most intricate part of the science, may, I apprehend, be, in many cases, brought within the reach of the busy manufacturer; while, by the same means, such accuracy and despatch may be ensured, as to render the analysis of saline mixtures, complex minerals, and mineral waters, the work of an hour or two; the proportions of the constituents being determined, to one part in the thousand."

The success which the present work has already had, will, we hope, encourage Dr. Ure to bring forth, with all diligence, a work so much wanted at the present moment, as that above-described. From the specimens he has exhibited in the Dictionary, we are satisfied that it will contain a faithful exposition of the known facts, with useful rules for simplifying chemical practice.

The Dictionary would have been improved, for occasional consultation, by page numerals, for want of which, there is a difficulty of referring to specified passages, in such extensive dissertations as his articles ATTRACTION, CALORIC, COMBUSTION, ELECTRICITY, GAS, EQUIVALENTS CHEMICAL, &c. The view of what is usually called the atomic theory, which he has given under the last-named article, seems to us the most complete and most philosophical hitherto offered to the public, and presents some valuable rules for computing from analysis, the proportional weight, or prime equivalent of the various simple and compound bodies. O.

ii. *The Elements of Chemical Science. By* J. GORHAM, M.D., *Member of the American Academy, and Professor of Chemistry in Harvard University, Cambridge.* 2 *vols.* 8vo. *Boston,* 1819 *and* 1820.

This is the first original book on Chemistry published in the United States, and merely as such, deserves the notice of our chemical readers; it contains nothing original either in experiment or observation, neither does it profess originality, but it has the merit of perspicuous arrangement and candid narrative.

The first volume includes a succinct account of the doctrines of attraction, heat, light, and electricity, of the chemical properties of the supporters of combustion, and of the simple inflammable unmetallic bases, and concludes with a general view of the process of combustion, and of the analogies between the simple substances and some of their compounds, with remarks on some points of chemical theory. The preliminary part of this volume is a judicious compilation of facts from the best English works; but, in the concluding chapters, Dr. Gorham enters upon general views, and becomes more tangible to the critic. In his history of combustion, the author has scarcely done justice to Jean Rey who preceded Hooke and Mayow, and whose experiments tended to establish the influence of air in that process, and to lead to views analogous to those subsequently adopted by his eminent successors; nor has he given as much room to the experiments and speculations of Hooke as they deserve, while he has detailed, with tiresome minuteness, the exploded theory of Lavoisier, and has even condescended to notice and transcribe, not, it is true, without

some animadversion, the Thomsonian theory of semi-combustion, upon which we have formerly expressed our opinion. In this part of his book Dr. Gorham has also wasted some pages in detailing the absurd speculations respecting the absolute zero, placed by Dr. Irvine at 700°., and by Mr. Dalton at 6150°. below 0, this small discrepance being of little importance, it would seem, in such inquiries. There is also much that might have been omitted in relation to the capacities of bodies for heat; but we can forgive these exuberances, as they lead to a tolerable compendium of Sir H. Davy's researches on flame, and to an exposition of those inimitable investigations which ended in his discovery of the safety lamp.

In Dr. Gorham's chapter on some points of chemical theory, he adverts to the imperfections of all systematic arrangements of the subjects of Chemistry, and to the difficulties which beset the writer in concisely setting forth his materials without frequent repetition, or more blamable omission of facts small in themselves but important in association. "If," says he, "we commence with the principles of the science, its laws can be demonstrated only by a reference to the mutual actions of bodies still unknown; and if individual substances be first described, the general terms which have been appropriated to classes of facts must be employed, and frequently but imperfectly explained." Of these difficulties, and of many others, those who are at all conversant with chemical writers, must be amply aware, and, in the present state of the science, we see little probability of their removal, or even of their material diminution. Taking all things into the account, we are of opinion that Dr. Gorham has himself followed one of the least exceptionable plans of arrangement; namely, that which, after having discussed the general laws of chemical changes, proceeds to the history of elementary bodies, and of their mutual combinations in morganic nature, and, ultimately, to their complex arrangements in the animal and vegetable world. We are quite aware that this plan is not very philosophical, but upon such a subject we willingly sacrifice logical accuracy to perspicuous detail, and consider that as the best arrangement, which most easily enables the student to retain, compare, and apply the infinitely numerous, diversified, and scattered facts of this endless branch of natural knowledge.

In adverting to the analogies between the elementary substances, Dr. Gorham has struck out nothing new, and has not been peculiarly happy in retailing the opinions of others; chemical writers indeed generally misemploy their own and their readers' time, in entering upon the abstract philosophy of their science, and would fill their paper more advantageously in extending practical details, and describing the minutiæ of manipulation. Could we see a System of Chemistry from the pen

of Dr. Wollaston, or of Sir Humphry Davy, we should indeed expect to be instructed and edified by their incursions into the truly philosophical and speculative regions of their science; the former would surprise us by the profound and accurate solidity of his judgment in regard to theoretical points; and the latter would enchain our attention by the brilliancy of his generalizations, and the happy talent which he so eminently possesses, of seizing upon remote analogies and bringing them to bear upon new investigations and discoveries. But as we fear that neither Dr. Wollaston nor Sir H. Davy, who already sit "enthroned in the uppermost chambers" will ever condescend to the drudgery of compilation which has not inaptly been compared to the labour of the anvil and the forge, we must rest content with, and should indeed feel grateful to, those who employ that measure of time and talent which they possess, in arranging and collecting the insulated and scattered facts of chemistry into a tangible aggregate, provided they perform their task with judgment and candour, and not in that sour and distorting vein of peevish petulance for which we have lately had occasion to reprimand a writer, whose general information and indefatigable diligence promised at one time to render him eminent amongst British Systematists. But, to be brief, we would have chemical writers bestow more time upon the real business of the laboratory, and less upon matters of opinion and speculation; not in detailing the figures of retorts and receivers, nor in descanting upon the art of filling soap bubbles with hydrogen gas, but in describing faithfully and minutely the various obstacles that oppose the student's progress in the common processes of experiment, as well as in the more refined and difficult branches of analysis. We are no admirers of Scientific Catechisms, nor do we profess profound reverence for Messrs. Longmans' manufactory of Philosophical Conversations; but we venture to suggest that even from these humble sources, for it is said there are sermons even in stones, some hints might be derived, useful to the systematic writer; at least, he might learn from them the mode of addressing beginners in the study, which though he is too apt to forget it, constitutes a main part of his calling as an instructor.

But, to return to Dr. Gorham; the second volume includes the history of the metals and of organic substances. As our author has omitted all mineralogical and geological details, he might have greatly improved his introductory chapter on the metals, if he had sketched their natural history; he might also have introduced in this place, some few historical particulars respecting them, which are interesting and important to the student, and which we have looked for in vain, in other parts of his book; with these exceptions, however, Dr. G. has given a good general sketch of the chemical habitudes of this class of bodies.

In considering them individually he has adopted the arrangement of Thenard, which, with all its faults, is perhaps, as little open to objection as any other which could be suggested; excepting that we should deem it improved by placing the division which treats of bodies regarded analogically as metallic oxides, last instead of first.

To give our readers some notion of Dr. Gorham's individual treatment of the metals, we shall select one of the most important, namely, Iron; the general order of description is similar in all of them. The first paragraphs relate to the importance of this metal, to its general diffusion, and its mechanical properties; its two oxides are next described, and the difficulty adverted to of reconciling their composition with the atomic theory. The chlorides, carburets, phosphuret, and sulphurets of iron are then treated of, and to these succeeds the history of its salts, in the order following: 1. Chlorate. 2. Muriate. 3. Nitrate. 4. Carbonate. 5. Phosphate. 6. Sulphate. 7. Ferrocyanate. The last paragraph of the section on iron describes its alloys. Such is the general arrangement adopted under the head of each of the metals, and it is in our opinion infinitely preferable to that disjointed plan, generally followed by our own authors, of considering the metals in the abstract in one chapter, their salts in a second, their combinations with oxygen in a third, and so forth; the perplexity and confusion of which, if not self evident, may be amply judged of by reference to Dr. Thomson's system, and to M. Thenard's *Traité*. While however we applaud our author's plan, we cannot congratulate him on the happiness of its execution; his details are meagre and unsatisfactory; we are told nothing of the ores of the metals; of the means of reducing them; of the methods of analyzing their combinations, of their uses in the arts; and of many other things which Dr. Gorham might have picked out of the works of Hatchett, Klaproth, and other standard authorities, and which would greatly have contributed to the value of his work, more especially considered as a text book for students. The speculations of Dr. Berzelius, and the hypotheses of Dr. Murray, are very well as speculations and hypotheses, but they should not have been suffered to usurp the place of genuine philosophy.

The epitome of Vegetable Chemistry is divided into three chapters; the first is subdivided into twenty sections, giving an account of the proximate principles of plants; the second chapter contains a brief view of the structure and chemical physiology of vegetables, which would have more aptly preceded the former; and the third is entitled, "Of the Spontaneous Decomposition of Vegetables," and includes the phenomena and products of fermentation.

The fluids and solids of the animal body, and the changes

that attend their spontaneous decomposition, are the subjects of the two concluding chapters; to the latter, "*mineral waters*" are tacked on as a kind of outrider; how these are connected with, or related to toasted cheese and adipocere, we cannot even guess; the printer, probably, is to blame; Dr. Gorham, however, gives almost faulty sketch of the analysis to be adopted in ascertaining their constituents; and what is less pardonable, he refers to threadbare and insufficient authorities for further details; Phillips, Marcet, and Klaproth, are the sources to which he should have directed his readers.

Having already said that Dr. Gorham's book contains nothing either new or original, we have not thought it necessary to canvass the theories which he has adopted; of the arrangement we have already spoken in terms of sufficient approbation; but, having also briefly adverted to some of its defects, we doubt not that in a future edition we shall see considerable improvement. Unfortunately every page of this work shows that the author is not at home in the laboratory; although therefore his reading is extensive, and has made him well acquainted with the labours of others, there is a want of that free and easy description which we meet with in the writings of really practical chemists, and which makes the reader, as it were, an assistant and participator in the processes that are brought before him. Dr. Gorham's style and language, though without elegance, are sufficiently correct and unobjectionable, but for the reasons we have just stated, his book is dry and uninteresting to any except the mere tyro; he has every where done ample justice to the British school of Chemistry, and in the introduction to the first volume, has given a very creditable sketch of the causes which have influenced the recent progress of the science, and which have tended to annihilate the visionary and speculative generalizations of Lavoisier and his associates.

I.

Art. XVI. ASTRONOMICAL AND NAUTICAL COLLECTIONS. No. VI.

i. *A Postscript on Atmospherical Refraction. By* Thomas Young, M.D., F.R.S. *From the* Philosophical Transactions *for* 1819. *With a parenthetical Correction.*

1. A simple and convenient method of calculating the precise magnitude of the atmospherical refraction, in the neighbourhood of the horizon, has generally been considered as almost unattainable; and Dr. Brinkley has even been disposed to assert the "impossibility of investigating an exact formula," [that should represent all its variations], notwithstanding the "striking specimens of mathematical skill, which," as he justly observes, "have been exhibited in the inquiry." We shall find, however, that the principal difficulties may be evaded, if not overcome, by some very easy expedients.

2. The distance from the centre of the earth being represented by x, and the weight of the superincumbent column by y, the actual density may be called z, and the element of y will vary as the element of x and as the density conjointly; consequently, $dy = -mzdx$; the constant quantity m being the reciprocal of the modulus of elasticity. The refractive density may be called $1 + pz$, p being a very small fraction; and it is easy to see that the perpendicular u, falling on the direction of the light, will always vary inversely as the refractive density, since that perpendicular continually represents the sines of the consecutive angles, belonging to each of the concentric surfaces at which the refraction may be supposed to take place (Nat. Phil. II. p. 81:) and $u = \frac{s}{1+pz}$, s being a constant quantity. The angular refraction at each point will obviously be directly as the elementary change of this perpendicular, and inversely as the distance v from the point of incidence; whence the fluxion of the refraction will be $\frac{du}{v} = dr$, as is already well known.

3. For the fluent of this expression, which cannot be directly

integrated, we may obtain a converging series by means of the Taylorian theorem; but we must make the fluxion of the refraction constant, and that of the density variable; so that the equation will be $u = \frac{du}{dr} \cdot r + \frac{d^2u}{dr^2} \cdot \frac{r^2}{2} + \frac{d^3u}{dr^3} \cdot \frac{r^3}{2.3} + \ldots$, u being the initial value of u, when $r = 0$. Now the whole variation, of which u is capable, while z decreases from 1 to 0, extends from $\frac{s}{1+p}$ to s; or, since p is very small, from $s - ps$ to s; and dr being $= \frac{du}{v}$, we have the equation $ps = vr + \frac{dv}{dr} \cdot \frac{r^2}{2} + \ldots$

But $v = \sqrt{(x^2 - u^2)}$, $dv = \frac{x dx - u du}{v}$, and $\frac{dv}{dr} = \frac{x}{v} \cdot \frac{dx}{dr} - u$, and dx being $= -\frac{dy}{mz}$, and $du = -psdz$, $\frac{dx}{dr} = \frac{v}{mpsz} \cdot \frac{dy}{dz}$.

4. We must now determine the value of the density z, which, when the temperature is uniform, becomes simply $= y$; but for which we must find some other function of y, including the variation of temperature; and we may adopt, for this purpose, the hypothesis lately advanced by Professor Leslie, in the article Climate of the Encyclopædia Britannica, and suppose the density to be augmented, by the effect of cold, in the proportion of 1 to $1 + n\left(\frac{1}{z} - z\right)$, n being somewhat less than $\frac{1}{10}$; and since the density is as the pressure and the comparative specific gravity conjointly, we have $z = y\left(1 + n\left[\frac{1}{z} - z\right]\right)$, $\frac{z}{y} = 1 + \frac{n}{z} - nz$, $d\frac{z}{y} = \frac{dz}{y} - \frac{zdy}{yy} = -\frac{ndz}{zz} - ndz$, and $\frac{dy}{dz} = \frac{y}{z} + \frac{nny}{z^3} + \frac{nyy}{z}$; consequently $\frac{dx}{dr} = \frac{v}{mpsz}\left(\frac{y}{z} + \frac{nyy}{z^3} + \frac{nny}{z}\right)$, $\left(\frac{dy}{ydr} = -\frac{mz}{y} \cdot \frac{dx}{dr} = -\frac{v}{psz} - \frac{nvy}{psz^3} - \frac{nvy}{psz}\right.$, and not "$-\frac{v}{psz} - \frac{2nvy}{psz^3} - \frac{2nvy}{psz}$," which stands in the original paper, in manifest defiance of *the first rules of arithmetic*: indeed the *very first* rule of all is forgotten, for the paragraphs are *numbered* 1, 2, 3, 4, 6 . . . ! But

instead of retracing the steps of the calculation with these corrections only, it will be more satisfactory to extend the general theorem somewhat further, without confining it to a particular law of temperature.

5. For this purpose we may make $\frac{dy}{dz} = \zeta$, $\frac{d\zeta}{dr} = \zeta'$, $\frac{d\zeta'}{dr} = \zeta'$, and we shall have, for computing the coefficients of the series $vr + \frac{dv}{dr}\cdot\frac{rr}{2} + \ldots$, the values

$$dr = \frac{du}{v};$$

$$\frac{du}{dr} = v;$$

$$\frac{dx}{dr} = \frac{\zeta v}{mns}$$

$$\frac{dz}{dr} = \frac{-du}{psdr} = \frac{}{ps}$$

$$\frac{dy}{dr} = \frac{\zeta dz}{dr} = \frac{-\zeta v}{ps},$$

$$\frac{dv}{dr} = \frac{\zeta x}{mpsz} - u;$$

$$d\frac{dv}{dr} = d\zeta.\frac{x}{mpsz} + dx.\frac{\zeta}{mpsz} - dz.\frac{\zeta x}{mpsz} - dz\frac{\zeta x}{mpsz^2} - vdr; \text{ and}$$

$$\frac{ddv}{dr^2} = \frac{\zeta' x}{mpsz} + \frac{\zeta^2 v}{m^2p^2s^2z^2} + \frac{\zeta x v}{mp^2s^2z^2} - v.$$ Now since m is about 766, it is obvious that the second term, containing its square, may be neglected in comparison with the third, since the other quantities concerned in these terms can never differ materially from each other: for the same reason the term v may be omitted, as not being divided by p, and u may be considered as equal to s, and its fluxion neglected, as well as that of x, which may be called $= 1$; and we may proceed to take the fluxion of

$$\frac{ddv}{dr^2} = \frac{\zeta' x}{mpsz} + \frac{\zeta x v}{mp^2s^2z^2} = \frac{\zeta'}{mpsz} + \frac{\zeta v}{mp^2s^2z^2}; \text{ whence } d\frac{ddv}{dr^2} = \frac{d\zeta'}{mpsz} - \frac{\zeta' dz}{mpsz^2} + \frac{\zeta dv + v d\zeta}{mp^2s^2z^2} - \frac{2\zeta v dz}{mp^2s^2z^4}; \text{ and } \frac{d^3v}{dr^3} = \frac{\zeta''}{mpsz} + \frac{\zeta' v}{mp^2s^2z^2} + \frac{\zeta}{m^2p^2s^2z^2}.\left(\frac{\zeta}{mpsz} - s\right) + \frac{\zeta' v}{mp^2s^2z^2} + \frac{2\zeta v^2}{mp^2s^2z}.$$ It will

now be convenient to divide ζ'' into the two portions $\zeta''_{,}$ and $\zeta''_{,,}v^2$, in order to obtain that part of the sixth term which is independent of v; the fourth will then become $\frac{d^3v}{dr^3} = \frac{\zeta''_{,}}{mpsz} + \frac{\zeta}{mp^2s^2z^2}\cdot\left(\frac{\zeta}{mpsz} - s\right) + \left(\frac{\zeta''_{,,}}{mpsz} + \frac{2\zeta'}{vmp^2s^2z^2} + \frac{2\zeta}{mp^3s^2z^3}\right)v^2$. The whole of the fluxion of the former part will contain v, which will disappear again in the next term, being changed into dv, and the v^2 of the second part will become $2\frac{dv^2}{dr^2}$ in the sixth term. We shall therefore have, for the case of the horizontal refraction when $z=1$ and $s=1$, $\frac{d^5\gamma}{dr^5} = \left(\frac{d\zeta''_{,}}{mpdt} - \frac{\zeta''_{,}}{mp}\cdot\frac{dz}{dt} + \frac{\zeta'}{mp^2}\cdot\frac{dv}{dt} - \frac{2\zeta dz}{mp^2dt}\right)\frac{dv}{dr} + \frac{\zeta}{mp^2}\cdot\frac{ddt}{dt^2}\left)\frac{dv}{vdt} + 2\frac{dv^2}{dr^3}\left(\frac{\zeta''_{,}}{mp} + \frac{2\zeta'}{vmp} + \frac{2\zeta}{mp^2}\right)\right.$

It is obvious, that since $\frac{ddv}{dr^2} = \left(\frac{\zeta}{v}\cdot\frac{1}{mpsz} + \frac{\zeta}{mp^2s^2z}\right)v$, the quantity ζ', must be derived from it by taking the fluxion with respect to v only, and must be equal to $\frac{ddt}{dr^2}\cdot\frac{dv}{vdt}$, which is the product of the second and third coefficients. The fluxion of this quantity, $d\zeta''$, is also capable of a simpler expression; for since ζ' will in general be divisible by v, $\zeta'_{,} = \frac{d^2\zeta'}{dv}\cdot\frac{dv}{dt} = \frac{\zeta}{v}\frac{dv}{dr}$, and $d\frac{\zeta'}{v} = \frac{d\zeta'}{v} - \frac{\zeta dv}{vv}$; whence $\frac{d\zeta''_{,}}{dr} = \frac{d\zeta'}{vdt}\cdot\frac{dv}{dr} - \frac{\zeta'}{vv}\cdot\frac{dv^2}{dr^2} + \frac{\zeta'}{v}\frac{d^2v}{dr^2} = \frac{\zeta''}{v}\cdot\frac{dv}{dr} - \frac{\zeta''}{v}\frac{dv}{dr} + \frac{\zeta}{v}\cdot\frac{d^2v}{dr^2} = \frac{\zeta''_{,,}v^2}{v}\cdot\frac{dv}{dt} + \frac{\zeta'}{v}\cdot\frac{d^2v}{dr^2} = \zeta''_{,}v\cdot\frac{dv}{dr} + \frac{\zeta}{v}\frac{d^2v}{dr^2}$. Consequently

$$\frac{d^5v}{dr^5} = \left(\frac{\zeta''_{,,}}{mp}\cdot\frac{dv}{dr} + \frac{\zeta'}{v^2mp}\cdot\frac{d^2v}{dr^2} + \frac{\zeta'}{vmp^2}\cdot\frac{dv}{dr} + \frac{\zeta'}{vmp^2}\cdot\frac{dv}{dr} + \frac{2\zeta}{mp^3}\cdot\frac{dv}{dr} + \frac{\zeta}{vmp^2}\cdot\frac{ddv}{dr^2}\right)\frac{dv}{dr} + \frac{dv^2}{dr^2}\left(\frac{2\zeta''_{,,}}{mp} + \frac{4\zeta'}{vmp^2} + \frac{4\zeta}{mp^3}\right)$$
$$= \frac{dv^3}{dr^3}\left(\frac{3\zeta}{mp} + \frac{6\zeta'}{vmp^2} + \frac{6\zeta}{mp^3}\right) + \frac{dv}{dr}\left(\frac{\zeta'}{v} + \frac{\zeta}{p}\right)\frac{ddv}{vmpdr^2}$$

6. We may next proceed to substitute, in these general ex-

pressions, the values derived from the various laws which may be supposed to govern the variations of temperature: observing first, that in general

$m = 766, p = .0002825 = \frac{1}{3540}$; whence

$\frac{1}{mp} = 4.621$, $\frac{1}{m^2p^2} = 21.3536$, $\frac{1}{mp^3} = 16358$, $\frac{1}{m^2p^4} = 72052$, $\frac{1}{mp^5} = 57\,907\,320$.

7. (A) If the temperature were uniform, we should have $y = z, dy = dz, \zeta = 1, \zeta' = 0, \zeta'' = 0$, and $\frac{dv}{dr} = \frac{1}{mps} - s = \frac{4.621}{s} - s$; and when $s = 1$, 3.621

$\frac{d^2v}{dr^2} = \frac{v}{mps}$

$\frac{d^3v}{dr^3} = \frac{1}{mps}\left(\frac{1}{mps} - s\right) + \frac{2v^2}{mps}$; or if $v = 0$, 16358×3.621

$\frac{d^4v}{dr^4} = \left(\frac{1}{mp} - 1\right)^2 \frac{6}{mp^3} + \left(\frac{1}{mp} - 1\right) \frac{1}{m^2p^4} = 3.621\ (6 \times 57907320 \times 3.621 + 16358^2) = 5524050000$; $\frac{1}{720}$ of which is 7672300. Hence, for $s = 1$, we have the equation $.0002825 = 1.8105\ r^2 + 2467\ r^4 + 7672300\ r^6 + \ldots$, in which, if we put $r^2 = .000130$, we shall have $.0002825 = .0002939 + \ldots$; which is too much: then taking $r^2 = .000120$, we have $.0002825 = .00021726 + .00003552 + .00001325 + [.00001647]$: and this is somewhat too great a remainder; for the quotients of the terms being 6, 3..., the remainder ought not to exceed the last term; so that r^2 must be about .000121, and $r = .0110$, or $37'\,50''$, which is too great by about one ninth. By the assistance of this series we might easily compute the refraction upon the hypothesis of Professor Bessel, who supposes the variation of density to follow the same law as if the temperature were uniform, but alters the value of m, so as to accommodate it to the actual magnitude of the refraction in low altitudes.

(B) In Professr Leslie's hypothesis, we have

$n = \frac{45}{500} = .09$

$\zeta = \frac{y}{z} + \frac{nyy}{z} + \frac{nyy}{z^3}$; the initial value $\zeta^{\nu} = 1 + 2n = 1.18$

$$\zeta' = \frac{v}{ps}\left(\frac{y}{z^2} + \frac{nyy}{z^3} + \frac{3nyy}{z^4}\right) - \frac{v\zeta}{ps}\left(\frac{1}{z} + \frac{2ny}{z} + \frac{2ny}{z^3}\right)$$

$$\zeta'^{\nu}\frac{v}{ps}(1+4n)(1-\zeta) = \frac{-v}{ps}(2n + 8n^2)$$

$$\frac{d^2v^{\nu}}{dr^2} = \frac{1 + 2n - (2n + 8n^2)}{mp^2s^2}\, v = \frac{1-8n}{mp^2s^2}\, v.$$

$$\zeta'' = \frac{d^2\zeta'}{dv}\cdot\frac{dv}{dr} + \frac{d^2\zeta'}{dy}\cdot\frac{dy}{dr} + \frac{d^2\zeta'}{dz}\cdot\frac{dz}{dr} + \frac{d\zeta'}{d\zeta}\cdot\frac{d\zeta}{dr} = \zeta'' + \zeta' v^2$$

$$\zeta''_{,} = \frac{d^2\zeta'}{dv}\cdot\frac{dv}{dr} = \frac{\zeta'}{v}\cdot\frac{dv}{dr} = \frac{\zeta}{v}\left(\frac{1+2n}{mps} - s\right)$$

$$\zeta''_{,,} v^3 = \frac{d^2\zeta'}{dv}\cdot\frac{-\zeta v}{ps} + \frac{d^2\zeta}{dz}\cdot\frac{-v}{ps} + \frac{d^2\zeta}{d\zeta}\cdot\zeta$$

$$\zeta''_{,,}\, v^{3\nu} = \left\{\frac{v}{ps}(1+8n) - \frac{\zeta v}{ps}(4n)\right\}\frac{-\zeta v}{ps} - \frac{v}{ps}\left\{\frac{-v}{ps}(2+14n) + \frac{v\zeta}{ps}(1+8n)\right\} + \frac{v^2}{p^2s^2}(1+4n)(2n+8n^2) = \frac{v^2}{p^2s^2}\cdot\{-\zeta(1+8n) + \zeta^2(4n) + 2 + 14n - \zeta(1+8n) + 2n + 16n + 32n^2\}$$

$$= \frac{v^2}{p^2s^2}(-1-8n-2n-16n^2+4n+16n+16n^2+2+14n-1-8n-2n-16n^2+2n+16n^2+32n^2)$$

$$= \frac{v^2}{p^2s^2}\cdot(48n^2)$$

$$\frac{d^3v^{\nu}}{dr^3} = \frac{1}{mp^2s^2}\left\{-(2n+8n^2)\left(\frac{1+2n}{mps} - s\right) + \zeta\left(\frac{1+2n}{mps} - s\right)\right\} + \frac{v^3}{mp^3s^3}(48n^2 - (4n+16n^2) + 2 + 4n)$$

$$= \frac{1-8n^2}{mp^2s^2}\left(\frac{1+2n}{mps} - s\right) + \frac{2+16n^2+48n^2}{mp^2s^2}\, v^{3\nu}$$

$$\frac{d^4v^{\nu}}{dr^4} = \left(\frac{1+2n}{mp} - 1\right)\cdot\frac{1}{mp^2}(144n^2 - 12n - 48n^2 + 6 + 12n) + \left(\frac{1+2n}{mp} - 1\right)\cdot\frac{1-8n^2}{m^2p^2}\left(\frac{1+2n}{p} - \frac{2n+8n^2}{p}\right) = \left(\frac{1+2n}{mp} - 1\right)\cdot\frac{1}{mp}(6 - 48n^2 + 144n^2) + \left(\frac{1+2n}{mp} - 1\right)\left(\frac{1-8n^2}{mp^2}\right)^2$$

We have then, for the case of horizontal refraction, $\frac{dv}{dr} = 4.453 = 2 \times 2.2265$; $\frac{d^3v}{dr^3} = \frac{.9352}{mp^2} \times 4.453 = 68112 = 24 \times 2838$, and $\frac{d^5v}{dr^5} = (4.453)^2 \times 57907320 \times 5.7162 + 4.453 \times 15296^2 = 7657200\,000 = 720 \times 10635000$: consequently, $.0002825 = 2.2265\, r^2 + 2838\, r^4 + 10635000\, r^6$; now if $r^2 = .0001$, we have $.0002825 = .00022265 + .00002838 + .000010635$ [$+ .000020935$]: consequently, .0001 is too little for r^2, and we may try .00011, giving $.0002825 = .00024491 + .00003434 + .00001420$ [$- .00001095$]. But in order to keep up the probable sequence of the progression, the remainder should be about equal to the last term, or about .000011, and .0000209 should have been diminished by about .00001 instead of .0000318; so that we must take .000103 as the true value of r^2 on this hypothesis, and $r = 34'.53''$, which is *exactly*] too great by about 1′; a difference by far too considerable to be attributed to the errors of observation only; and we must infer, that the law of temperature, obtained from the height of the line of congelation, is not correctly true, if applied to elevations remote from the earth's surface. [If indeed this law were fully established, and capable of being applied, with any little modification, to the exact computation of the refraction, it would be necessary, for the lowest altitudes, either to compute a greater number of the fluxional coefficients, or to divide the refraction into two or more parts, and determine the successive changes of density required for each of them. We should also have] for finding, on this hypothesis, the height x, corresponding to the pressure y and the density z, the expression $mx - m = 1 - \frac{y}{z} + \frac{n}{q}$ hl $\frac{2zz + qy(1-z)}{2zz - qy(1-z)}$; y being $= \frac{zz}{z + n - nzz}$, and $q^2 = 1 + 4n^2$[: and the actual state of the atmosphere would probably be very well represented by this formula, taking $n = .1$ or .11, rather than .09.

(C)]Professor Bessel's hypothesis is also found to make the horizontal refraction too great. Mr. Laplace's formula, which

affords a very correct determination of the refraction, is said to agree sufficiently well with direct observation also; but, in fact, this formula gives a depression considerably greater than was observed by Gay Lussac, in the only case which is adduced in its support; and the progressive depression follows a law which appears to be opposite to that of nature, the temperature varying less rapidly at greater than at smaller heights, while the observations of Humboldt and others seem to prove that in nature they vary more rapidly. Notwithstanding, therefore, the ingenuity, and even utility of Mr. Laplace's formula, it can only be considered as an optical hypothesis, and we are equally at liberty to employ any other hypothesis which represents the results with equal accuracy; or even to correct our formulas by comparison with astronomical observations only, without assigning the precise law of temperature implied by them.

[D. We may compute the effect of a temperature supposed to vary uniformly with the height, by making $z=y(1+tx-t)$, or $=yx^t$, we have then $\frac{z}{y}=1+tx-t$, or x^t, and $\mathrm{d}\frac{z}{y}=\frac{\mathrm{d}z}{y}-\frac{z\mathrm{d}y}{yy}$ $=t\mathrm{d}z$, or $=tx^{t-1}\mathrm{d}x$, which are initially the same. But $t\mathrm{d}x=\frac{-t\mathrm{d}y}{mz}$, and $\frac{\mathrm{d}z}{y}=\frac{z\mathrm{d}y}{yy}-\frac{t\mathrm{d}y}{mz}$, whence $\frac{\mathrm{d}z}{\mathrm{d}y}=\frac{z}{y}-\frac{ty}{mz}=\frac{mzz-tyy}{myz}$, and $\frac{\mathrm{d}y}{\mathrm{d}z}=\zeta=\frac{myz}{mzz-tyy}$; consequently $\mathrm{d}\zeta=\frac{my\mathrm{d}z+mz\mathrm{d}y}{mzz-tyy}-2myz.\frac{mz\mathrm{d}z-ty\mathrm{d}y}{(mzz-tyy)^2}$; and initially $\zeta=\frac{m}{m-t}$, and $\zeta'=\mathrm{d}\frac{\mathrm{d}\zeta}{\mathrm{d}x}=\left\{\frac{m+m\zeta}{m-t}-2m\frac{m-t\zeta}{(m-t)^2}\right\}\frac{-v}{ps}=\left\{\zeta+\zeta^2-2\zeta^2+\frac{2t\zeta^2}{m-t}\right\}\frac{-v}{ps}=\left\{\zeta-\zeta^2+2\zeta^2.\frac{t}{m-t}\right\}\frac{-v}{ps}=\left\{\zeta-\zeta^2+2\zeta^2(\zeta-1)\right\}\frac{-v}{ps}=\left\{(2\zeta^2-\zeta)(\zeta-1)\right\}\frac{-v}{ps}=\zeta(\zeta-1).(2\zeta-1)\frac{-v}{ps}$. Now, if we suppose the temperature to vary 1° in 300 feet, we have $\frac{1}{500}\cdot\frac{1}{300}=\frac{1}{150000}$, for the variation of density depending on temperature in $\frac{1}{20900000}$ of the earth's radius x; hence t should be 139, and ζ

$= \frac{766}{766-139} = 1.26$, whence $\frac{\zeta}{mp} = 5.822$. while the phenomena of refraction require this quantity to be about 6. Thus, in Bradley's approximation, we first take $r = \frac{ps}{v}$, and then $r = pta\,(ZD - \frac{3ps}{v}) = p\left(\frac{s}{v} - \frac{3ps}{v}(1+\frac{ss}{vv})\right)$ very nearly, or $r = \frac{ps}{v} - \frac{3p^2s}{v} - \frac{3p^3s^3}{vv}$, and $vr = ps - \frac{3r^2}{s} - 3r^2s$, or, while s remains small, $ps = vr + 3\frac{r^2}{s}$, which is sufficiently accurate near the zenith. If we make $\frac{\zeta}{mp} = 6$, we shall have $\zeta = 1.3$, and $t = 176$, which is equivalent to a depression of a degree of Fahrenheit in 227 feet: we shall then have, for ζ', $-1.3 \times .3 \times 1.6\,\frac{v}{ps} = 24 - .624\,\frac{v}{ps}$, and $\frac{d^2v}{dr^2} = (1.3 - .624)\,\frac{v}{mp^2s^2} = .676\,\frac{v}{mp^2s^2} = .676 \times 16358$, and $\frac{1}{8}$ of this, or 1854, is the coefficient of the third term. With the same value of ζ, taking $n = .15$, this coefficient would become, upon a hypothesis similar to Professor Leslie's, 2236.

8. It is not possible, in the present state of our knowledge of the subject, to determine, from observation, either the refraction with sufficient accuracy to enable us to compute from it the law of the variation of temperature, or the variation of temperature with sufficient accuracy for computing the refraction. Considering, indeed, how improbable it is that the upper regions of the atmosphere should be of the same temperature as the surface of the hills on the same general level, we could scarcely expect the agreement to be more complete than these computations make it; and it is perfectly possible either that t may be as great at 176, or that n may be .15: but we cannot determine from the observed refraction which of the laws of variation is capable of representing it with the greatest accuracy: much less should we be justified in believing, because Mr. Laplace's formula happens to represent the refraction very accurately,

that the temperature varies the less rapidly as we ascend higher. It is, however, perfectly justifiable, for the purposes of astronomy; to adopt the form of the equation which is shown by these examples to be converging, and to correct the coefficients by an immediate comparison with observation; and in this manner it has been found that the formula employed in the *Nautical Almanac* is abundantly sufficient for the purposes to which it is applied. This formula is $.0002825 = v\frac{r}{s} + (2.47 + .5v^2)\frac{r^2}{s^2} + 3600\, v\, \frac{r^3}{s^3} + 3600\, (1.235 + .25\, v^2)\frac{r^4}{s^4}$; its results are almost identical with those of the French tables, except in the immediate neighbourhood of the horizon. But the effect of a difference of temperature, at the place of observation, is not so correctly represented by any of the tables commonly employed, and requires to be separately examined.]

9. The terrestrial refraction may be most easily determined by an immediate comparison with the angle subtended at the earth's centre, the fluxion of which is $\frac{ad.r}{vx}$, and $\frac{ad.r}{exdr}$ is initially the first part of the coefficient of the second term of the series already obtained, and is equal to [about] 6; so that this angle, while it remains small, is six times the refraction: commonly, however, the refraction in the neighbourhood of the earth's surface is somewhat less than in this proportion.

10. The effects of barometrical and thermometrical changes may be deduced from the fluxion of the equation, if we make m, p, and n, or rather t, vary: and for this purpose it will be convenient to employ the form $ps = vr + \left(\frac{1}{2(m-t)p^s} - \frac{s}{2}\right) r^2$, the value of the fraction, if we neglect the subsequent terms, becoming 3.41; and this expression is sufficiently accurate for calculating the whole refraction, except for altitudes of a few degrees. Now the fluxion of $p = v\frac{r}{s} + \left(\frac{1}{2(m-t)p} - \frac{ss}{2}\right)\frac{rr}{ss}$, which we may call $p = v\frac{r}{s} + \left(\frac{1}{w} - \frac{ss}{2}\right)\frac{rr}{ss}$, is $dp = \left(\frac{v}{s} +$

$\left(\frac{1}{w}-\frac{ss}{2}\right)\frac{2r}{ss}\,dr-\frac{rr}{ssw}\left(\frac{dm-dt}{m-t}+\frac{dp}{p}\right)$, the coefficient of dr being equal to $\frac{2p}{r}\frac{v}{-s}$; and $\left(2p-\frac{rv}{s}\right)\frac{dr}{r}=\left(p+\frac{rr}{ssw}\right)\frac{dp}{p}+\frac{rr}{ssw}\left(\frac{dm-dt}{m-t}\right)$; $\frac{1}{w}$ being 3.4.1, and $m-t$, on this supposition, 519. The proportional variation of p, or $\frac{dp}{p}$, will be $\frac{1}{500}$ for every degree that the thermometer varies from 50°; and $\frac{dm}{m}$ being also $\frac{1}{500}$, $\frac{dm}{m-t}$ will be $\frac{766}{519\times500}=.003$. The variation of t can only be determined from conjecture; but supposing the alteration of temperature to cease at the height of about 4 miles, it must increase, with every degree that the thermometer rises at the earth's surface, about $\frac{1}{120}$, and $\frac{dt}{t}$ being $\frac{1}{120}$, $\frac{dt}{m-t}$ will be $\frac{247}{519\times120}=.004$. The alterations of the barometer will affect p only, $\frac{dp}{p}$ being $\frac{1}{30}$ for every inch above or below 30. It is evident, since $m=\frac{39.58\times5280\times12}{13.57\,hd}$, h being the height of the barometer, and d the bulk of air compared to that of water, that m must diminish, as well as p, when the temperature increases; and the correction for t being subtractive, the three variations will co-operate in their effects; but the proportion will be somewhat different from that of the simple densities. If we preferred the expression derived from Professor Leslie's hypothesis, we should merely have to substitute $\frac{2dn}{1+2n}$ for $\frac{dt}{m-t}$, and the variation depending on the *law* of temperature would become about $\frac{2}{3}$ as great. It must, however be limited to such changes as affect the lower regions of the atmosphere only, its "argument" being the deviation from the mean temperature of the latitude; but even in this form it cannot be satisfactorily applied to the observations at present existing; although it appears to be amply sufficient

to explain the irregularities of terrestrial refraction, as well as the uncommon increase of horizontal refraction in very cold countries: and we may even derive from all these considerations a correction of at least half a second, or perhaps of a whole second, for the sun's altitude at the winter solstice, tending to remove the discordance, which has so often been found, in the results of some of the most accurate observations of the obliquity of the ecliptic.

ii. *Extracts from two Papers on Refraction, by the Rev.* John Brinkley, D.D., *published in the Transactions of the Royal Irish Academy.*

I. Read *May*, 1811.

12. As it is of considerable importance, particularly with a view of comparing observations made in different places, that the same refractions should be generally used, no objection, I apprehend, can be made to the general adoption as far as about 80° of the French refractions, which are now so well known.

13. Perhaps the following tables, "deduced from the above formula," may be considered rather more convenient in many instances than the French tables; they will certainly furnish a useful check. The advantage they afford is derived from the facility with which the computation can be made by help of tables of logarithms and of logarithmic tangents to four or five places of figures, such as are in the "Tables requisite to be used with the *Nautical Ephemeris*." By these the logarithmic tangent of the zenith distance can be taken out at once, and the inconvenience of proportioning for the minutes of zenith distance avoided, which is greater than the new inconvenience occasioned by the second table. Hence the tables here given may be considered more convenient for observations of the sun, moon, and planets.

II. Read *January*, 1820.

Although observations of zenith distances, when the object is near the horizon, are frequently affected by irregularities of refraction, that seem not capable of being reduced to any law, yet there is reason to suppose that the effect of these irregularities will disappear in a mean of a great number of observations; and thus a mean refraction for any altitude may be obtained, depending only on the mean zenith distance, and the corresponding heights of the barometer and thermometer. The investigation of the law of these *regular* refractions, as they may be called, has much engaged the attention of astronomers.

This inquiry has led to the extremely complex but elegant mathematical researches of Kramp, Laplace, and Bessel. Their investigations are nearly related to each other. Dr. Young has also recently, by an entirely different method, and with great analytical skill, obtained an equation expressing the relation between the refractive force of air and the refraction at any zenith distance.....

It is the object of this paper to deduce, by help of a modification of the result of the hypothesis of a density decreasing uniformly, by an extremely simple investigation, the refraction, at any low altitude *corresponding to any heights of the barometer and thermometer.* The tables thence resulting, for zenith distances, between 80° and the horizon, will, I conceive, be found as convenient as can be desired. They scarcely yield in simplicity to the French tables, and enable us to obtain the quantity of refraction, as changed by the weight and temperature of the atmosphere, in which, near the horizon, the French tables appear entirely to fail.

The first tables in which this has been attended to, as far as the horizon, if I mistake not, were those of Mr. Bessel.

In our ignorance of the law of variation of density, we can only verify any hypothesis that we adopt, by a comparison of its results with those obtained by direct observation. In this way, by help of Dr. Bradley's observations, Mr. Bessel has

obtained a modification of the law of uniform temperature, that will give the refractions, to within about three degrees of the horizon, with great exactness. Dr. Young has, by "adopting" a law of variation of temperature advanced by Professor Leslie, obtained an equation for refraction, the solution of which gives the refractions with considerable exactness as far as the horizon.

The following method is derived from the formula, obtained in the hypothesis of a density decreasing uniformly. However great, within the usual limits, we may suppose the change of density at the surface to be, there is no reason to suppose a material change in the *law* of density in the atmosphere. (Note. This reasoning may be fallacious, and it appears to be very desirable, that the facts should be ascertained by a sufficient number of observations, at given zenith distances near the horizon.) At present we have not sufficient observations to determine, whether the actual variations of refractions at low altitudes are most conformable to the theory of Mr. Bessel, to that of Dr. Young, or to that above given

TABLE I.

Z. D.	Log.	Diff. for 1.	Z. D.	Log.	Diff. for 1
° ′			° ′		
80.0	1.3605	6.90	87.20	1.7837	14.55
81.0	1.4031	7.62	87.40	1.8128	14.65
82.0	1.4488	8.21	88.0	1.8121	15.50
83.0	1.4981	9.07	88.20	1.8731	16.05
84.0	1.5524	10.13	88.40	1.9052	16.75
85.0	1.6132	11.00	89.0	1.9387	17.25
85.30	1.6462	11.37	89.20	1 9732	18.00
86.0	1.6803	12.07	89.40	2.0092	18.60
86.30	1.7165	13.23	90.0	2.0464	
87.0	1.7562	13.75			

TABLE II.

Far. Therm.	Logarithms	Far. Therm.	Logarithms	Far. Therm.	Logarithms
°		°		°	
10	.3283	34	.3048	58	.2827
11	.3273	35	.3039	59	.2818
12	.3263	86	.3030	60	.2809
13	.3253	37	.3020	61	.2800
14	.3243	38	.3011	62	.2791
15	.3233	39	.3001	63	.2782
16	.3223	40	.2992	64	.2773
17	.3213	41	.2983	65	.2764
18	.3203	42	.2974	66	.2755
19	.3193	43	.2965	67	.2746
20	.3183	44	.2956	68	.2737
21	.3173	45	.2946	69	.2728
22	.3163	46	.2937	70	.2720
23	.3154	47	.2928	71	.2711
24	.3144	48	.2919	72	.2703
25	.3134	49	.2910	73	.2694
26	.3124	50	.2900	74	.2685
27	.3114	51	.2891	75	.2677
28	.3105	52	.2881	76	.2668
29	.3095	53	.2872	77	.2660
30	.3086	54	.2863	78	.2652
31	.3076	55	.2854	79	.2644
32	.3067	56	.2845	80	.2636
33	.3058	57	.2836	81	.2627

TABLE III.

Therm. near Barom.	Logarithms
°	
20	.2913
30	.2909
40	.2904
50	.2900
60	.2896
70	.2891
80	.2887

USE OF THE TABLES.

Log. A. in minutes = Tab. I. + (A. C.) Tab. II. + Tab. III.

Log. B. in minutes = Tab. I. + Tab. II. + Log. bar. + 7.2773.

Log. Refr. in seconds = Tab. II. + Log. bar. + log. tan. (zen. dist. − A + B).

EXAMPLE.

App. Z. D.=87° 42′ 10″. Bar. 29.50. Therm. 35°.

Tab. I.	87° 42′17	1.8160		Tab. I.	1.8160
Tab. II.	35° A. C.	9.6961		Tab. II.	0.3039
Tab. III.		0.2906		Log. Bar.	1.4698
A	63′,49	1.8027		Const.	7.2773
				B 7′,36	0.8670
				87°. 42,17	
				87 . 49,53	
				63,49	
		11.2481	Tang.	86 . 46,04	(Z.D−A+B
Tab. II.		0.3039			
Log. Bar.		1.4698			
Refr. 1051 ,5		3.0218			
= 17′ ,31′5					

[*By the Nautical Almanac.*

Alt. 2° 17′ 50′

2° 20′	R.	17′. 0″	Diff. 1′ 4,1	B. 35	Th. 2,8
		8,9	2′,10′+8,9	50,−17,5	15°+42,0
		42			
		17.50,9			
		17,5			
		17.33,4			

: and if 48°, instead of 50°, were the standard temperature of the table, it would be 17′. 27 .8.]

The mean of forty-two observations of α Lyrae S. P. made at the Observatory of Trinity College, Dublin, (mean of bar. 29,50, and mean of therm. 35°) gave 17′ 26″,5 [: 5″ less than Dr. Brinkley's table; 7″ less than the N. A.].

If Table 2, Vol. XII., be added to them, they will serve for computing the refraction from the zenith to the horizon.

TABLE 2. Vol. XII.

Z. D.	BAROMETER 28,50	29,00	29,50	30,00	30,50
°	″	′	″	″	″
80	10,5	10,7	10,9	11,1	11,4
79	8,1	8,3	8,5	8,7	8,9
78	6,3	6,4	6,6	6,7	6,9
77	5,1	5,2	5,3	5,4	5,6
76	4,1	4,2	4,3	4,4	4,5
75	3,1	3,4	3,5	3,6	3,7
74	3,0	3,0	3,1	3,1	3,2
73	2,5	2,5	2,6	2,6	2,6
72	2,1	2,1	2,2	2,2	2,2
71	1,8	1,8	1,9	1,9	1,9
70	1,5	1,5	1,5	1,6	1,6
69	1,3	1,3	1,3	1,4	1,4
68	1,2	1,2	1,2	1,2	1,2
67	1,0				1,0
66	0,9				0,9
65	0,8				0,8
64	0,7				0,7
63	0,6				0,6
62	0,6				0,6
61	0,5				0,5
60	0,5				0,5
58	0,4				0,4
56	0,3				0,3
54	0,3				0,3
52	0,2				0,2
50	0,2				0,2
45	0,2				0,2
40	0,1				0,1
30	0,0				0,0
0	0,0				0,0

[Tab. II. + log. var. + log. tan. Z.D. = log. R′. R′ − Tab. 2. = Refr.]

EXAMPLE. Z. D. 71° 26′. Bar. 29.76 i. Ther. 43°.

Log. Tab. II.	0.2965	Appr. Refr.	175″,4
Log. bar.	1.4736	Tab. 2.	2 ,0
Log. tan. 71° 26	0.4738	Refr.	173 ,4 = 2′53″,4
Log. appr. R. 175″,4	2.2439		

[The same Example by the Nautical Almanac.]

Alt. 18° 34′

Alt. 19°	Refr. 2′.47″,7	Diff. Alt. ,16	B. 5,61	Th. ,34		
	+ 5,2	−26′ = +4,16	.24,	−1,34	7°,	+2,38
	2′.52″,9					1,34
	2.53,4					1,01
Difference	,5.]					

iii. *Observations on* M. Delambre's *Remarks, relative to the Problem of finding the Latitude from two Altitudes, and the Time between. By the* Rev. John Brinkley, D.D., Professor of Astronomy in the University of Dublin.

In giving, in the last number of the Astronomical and Nautical Collections, M. Delambre's method of finding the latitude from two altitudes of the sun and the time between, a remark of his was inserted, containing a fundamental objection against the method of Douwes. It appears, however, that M. Delambre, in his *Nouvelles Réflexions, Conn. des Tems*, 1822, p. 316, has pursued an erroneous line of reasoning, a circumstance rare indeed as to that learned and illustrious astronomer. That objection to the indirect method of solving the problem is not founded.

M. Delambre has the equation (p. 317), $\frac{a}{c} = \cos. H - \tan. \psi \sin. H$, ψ being a small arc. He assumes a value for H, and computes both ψ and $(H + \psi)$ from this same equation, and finds $(H + \psi) - \psi = H$. This surely could not be otherwise. It is singular that it escaped M. Delambre, that the interval between the observations disappears from his equation.

Another point of view shows, that what he has done is not relative to the method of Douwes. It is not an impossible supposition to make the declinations exactly equal, and then his method of computation, page 321, concludes nothing, instead of becoming the method of Douwes.

The usual objections to the method of Douwes are, 1. Not allowing for the change of declination. This can occasion no

error of consequence to the navigator. But in fact, if great accuracy be desired, the change of declination may be easily allowed for in practising the method of Douwes, as stated below. 2. That, except under great limitations, the latitude computed may be further from the truth than that by the reckoning. But Dr. Brinkley's method entirely obviates this objection. His method is intended to correct and extend the results obtained by the original method of Douwes, even for cases where it would otherwise be quite useless. 3. The length of the computation has also been objected to, but unless it be repeated two or three times, it is shorter than the direct can be made, and it possesses no ambiguity embarrassing to those not conversant in spherical trigonometry. By Dr. Brinkley's method it rarely indeed happens that two operations are necessary.

Dr. Brinkley has given the following method of allowing for the change of declination.

Having computed the middle time by the method in the requisite tables, or by the common log. tables, add to it half the interval to get the time furthest from noon. *Or* use the estimated time when the observation furthest from noon was made; add together (three places of log. are sufficient), the sine of time furthest from noon, the secant of the altitude belonging to this time, and the cosine of the lat. by account; look for the sum among the log. sines, and take out the corresponding cosine, which is to be added to the log. of the change of declination in minutes. The sum is log. of the correction of the altitude furthest from noon. This is to be added to that altitude when the sun at the other observation is nearer the elevated pole; otherwise subtracted. The altitude so corrected is to be used instead of that observed, and the declination to be used is that at the observation nearest noon. The computed latitude found is to be corrected by Dr. Brinkley's rules in the *Nautical Almanac* 1822.

It will rarely occur that the time is not known with sufficient exactness for this correction of the altitude made to obviate the effect of the change of declination. Thus the correction will be easily had for the direct method.

M. Delambre's example, thus computed, will afford an instance of the indirect method leading to a true result, when exactly computed; contrary to the opinion of M. Delambre.

Example.

1st. Ob. PM. alt. 42° 14′,1 } Interval Decl. 8°.15′ N. Lat. by reckoning
2 Ob. PM. alt. 16 5, 8 } 3h=45° Increase of declin.=3′ 49°.45′ N.

Sec. 8°.15′	10.00152			nat sin. 42 14,1	67217		log. 3 0.477
Sec. 49.45	10.18089			16 5,8	27726		9.875
	4.68807				39491	log. 4.59649	2,3 0.352
A	4.88438					0.41716	16 5,8
AC sin 22° 30′	0.41716					A 4.88438	
39555 log	4.59720			med. time	52°.15′	sin 9.89803	16 3,5
med. t. 82°22′,5 sin	9.89874				22. 30		
22 30				d. lur. from noon	71.15	sin 9.984	
					16. 6	sec 10.017	
t.n. noon 29 52,5		net sin	4214,1 67217	net sin	48.45	cos 9.819	
11 56,2	sin. 9.41119	net sin	16 3,5 27062			sin 9.820	
	2		39555			cos 9.975	
	8.82234						
A	4 88138						
8670 log	3,93800		cot 48°45	9.943	tan 52 ?.22′	10.113	
67217			tan 8 15	9.161	cot. 11 .56	10.571	
75887 sin 49°.21′,8			2)19.104			2)20.087	
40 38, 2			P sin	9.552	Q tan	10 343	
8 15			+ L cos	9.970	sec tan	10.293	
lat. compd. 48 53, 2			— T	10.146		150	
lat. by acc. 48 45			— S tan	10.110	11°.50′ sin	9.411	
						9.851	
+ D 8, 2	log 0.911		— C sec	10.216		20.000	
	9.558		2		— T	10.146	
Corr. lat. — 3, 0	0.482			0.432			
4853, 2			A.C.	9.568			
Lat. 48.50, 2							

Had the estimated time been used, the enclosed part would have been unnecessary.

Taking 48° 55 for the lat. by account, the second supposition of Delambre, the computed lat. will be 48° 47′2

48 55

— D 7,8 log. 0.892
9.508
0.400

Corr. lat. + 2 ,9
48 47,2

Lat. 48 50,1

Supposing lat. by reckoning 47°.50′ This lat. is inexact only by 1′, although the lat. by account was inexact by 1°.
Lat. computed will be 49 23,6

1 33,6 =
+ D 93,6 log 1.971
9.568

Corr. lat. 34,6 1.539
49 23,6

Lat. 48 49,0

Vindication of the Connaissance des Tems, for 1812.

It appears from an Article in the *Annales de Chimie* for April, that the error of the Table of Corrections of the places of the stars, in the *Connaissance des Tems for* 1812, consists only in the omission of the character ⊙ at the head of the second column. This omission had led two astronomers of considerable reputation in London to point out the whole table to the Editor of these Collections as erroneous; and he is obliged to confess, that although he suspected the nature of the error, he had NOT the sagacity to discover how simply it might be *remedied*, as perhaps he ought to have done.

He had himself been put to great trouble and inconvenience for want of the errata page of the *Connaissance des Tems for* 1823, having received the volume without it: and he thought it due to the Editors to endeavour to supply the deficiency of their Booksellers or their Binders: never imagining that they could have supposed him so mean spirited, as to mention the circumstance from jealousy or ill nature, or that they could have attributed to him the silly vanity of seeking to claim reputation from having been the humble instrument of correcting a few errors of the press. That he was not deficient in sincere respect for the author of the table, or in gratitude for the labours of the French Astronomers, is sufficiently demonstrated by his remarks subjoined to the Lunar Observations computed and compared: and the many marks of friendship and kindness, which he has received from the Editors of the *Annales de Chimie*, have rendered it impossible that he should voluntarily have made any observations, that he could have supposed likely to wound their feelings unnecessarily. He might indeed have fancied, that he had some little reason to complain, that no acknowledgment was made, in the errata page in question, of the source from which it had been derived: and still more that no return had been made for the communication, by a private indication of a similar nature. He has now, however, for the first time, to acknowledge a favour of this kind, in a public denunciation of no less than "60 errors" at once; to which he must himself add, extempore, 180

more, of equal magnitude, beginning with the year 1821; the English computers having *always* neglected to attend to the observation of Mr. Burckhardt, contained in a note at the end of his Tables, that the Supplement of the node is to be diminished by 7′ whenever it is to be inserted in an Almanac. It may, however, be remarked, that this omission can never have a *sensible* effect in any computations, for which the mean place of the node, as set down in the Almanac, is employed; and that both these misconceptions might have been easily avoided, if the learned author of the tables had condescended to give a single example, of the manner in which a computer is to proceed, in employing every part of them. But it must be confessed, that it is difficult for a real mathematician to be aware of all the precautions, that are required, for avoiding the occurrence of errors of this sort in the hands of mere mechanical labourers.

v. *The Force of Magnetism, compared with the Dip. Extracted from* Captain SABINE'S *Appendix to* Captain PARRY'S *Journal. 4to. London*, 1821, p. cxxxviii.

"Having detailed the Observations on the intensity of the Magnetic Force, it may not be uninteresting briefly to examine, how far the results are consistent with the ratio in which it was expected that the magnetic force would be found to vary under different dips of the needle.

"In the Rules and Tables for clearing the Compass from the regular Effect of the Ship's Attraction, printed in 1819 by order of the Commissioners of Longitude, and published, with some alterations and additions, in the *Journal of the Royal Institution* for October, 1820, the magnetic force in the direction of the dipping needle is considered to vary, inversely, as the square root of four diminished by three times the square of the sine of the dip; and the force acting on a needle limited to a horizontal motion, inversely, as the square root of three increased by the square of the secant of the dip.

"The Observations at Melville Island are entitled to principal

consideration, as having been made under more favourable circumstances than were presented by the other opportunities of the voyage; they are, therefore, to be compared with those which were made in England.

"The dip in London being 70° 33'.3, and at Winter Harbour 88° 43.5, the force in the direction of the dipping needle should increase by calculation in the ratio of 1.153 to 1.

"The time of vibration of Mr. Browne's dipping needle decreased, between London and Winter Harbour, in the proportion of 481 to 446, and consequently the force appeared to have increased in the ratio of 1.163 to 1.

"The dip at Sheerness being 69° 55', and at Winter Harbour 88° 43'.5, the magnetic force should increase by calculation as 1.163 to 1; but the force acting on the horizontal needle should be diminished in the proportion of 13.275 to 1.

"The times of vibration of the three horizontal needles increased between Sheerness and Winter Harbour, in arcs from 7 to 14 degrees, respectively, as follow: No. 1 as 339.7 to 94.5; No. 2 as 327.4 to 90; and No. 3 as 316.1 to 85; consequently, the force acting on them appeared to have diminished by No. 1 as 12.93 to 1; by No. 2 as 13.23 to 1; and by No. 3 as 13.83 to 1; the mean being as 13.33 to 1; differing but $\frac{1}{70}$ from the result of the calculation.

"This is, perhaps, a nearer agreement with the theory than there was reason to have expected, considering how much the unavoidable causes of uncertainty in such experiments are augmented in high magnetic latitudes.

"The results on the 26th of June, and on the 23d and 24th of July, 1819, compared with the observations in England, will also be found to agree as well with the theory as it is reasonable to expect in experiments, where neither time nor circumstances admitted the adoption of the precautions, requisite to ensure the utmost accuracy of which they are capable; and where, perhaps, the moving of the ice during the observations may have introduced an additional error which no care could guard against. By the experiments of the 26th of June, the force had diminished by Needle No. 2, as 2.815 to 1, and by No. 3

as 2.86 to 1; and supposing the Dip to have been 83° 04′, as was found by rather an indifferent observation, the theory would require a diminution of 2.5 to 1. By the experiments of July the force had diminished by Needle No. 2, as 3.28 to 1, and by No. 3, as 3.198 to 1; the calculated diminution being 3.12 to 1."

It does not appear that this connexion between the dip and the magnetic force was ever before theoretically laid down, or experimentally established; it is, however, only an application of the system of Aepinus and Coulomb to a hypothesis respecting the magnetism of the earth, which is perhaps partly original, but which is the most simple and natural that could be assumed.

A strange assertion has lately been made, in a periodical publication, respecting the author of the paper in question, which is, that he must have been ignorant of the existence of a plane, in which a mass of iron, rendered magnetic by its temporary situation with respect to the earth only, produces no effect in the needle of a compass placed near it.

It is impossible that such an assertion could have been made by one who had properly studied the grounds of the modern theory of magnetism, as laid down by Aepinus, by Dr. Robison, by Haüy, by Biot, or by any other good elementary writer, and who had read the paper with any thing like attention.

The most superficial consideration of the nature of induced magnetism will show, that a mass of soft or conducting iron must become "a terrella," or earth in miniature, by the action of the earth's magnetism; and that as such, it must have its magnetic equator, on which the direction of the magnetic force is parallel to that of its magnetic axis, and consequently to the magnetic axis of the earth; and that when the needle is situated in the plane of this equator, it cannot be disturbed by the magnetism of the terrella, which must act in the same line as the force of the earth itself, though in a contrary direction.

That Mr. Lecount should have fancied this a new discovery is not at all surprising; and it does him great credit to have

pursued his investigation under many disadvantages; but it is much more remarkable that a literary man, who has written a great deal, and who has probably read a little, should have been so completely uninformed.

If the author of the paper published in these selections had been ignorant of the fact, he could never have remarked, in the 11th section, that "a horizontal bar of soft iron will lose its effect on the needle in four positions, at right angles to each other; and a bar so inclined as to become perpendicular to the dipping needle in the plane of the meridian, will lose its effect in two opposite positions in that plane only;" nor could he have obtained the result now so strikingly verified by Captain Sabine, if he had been so ignorant of the theory as the Reviewer's assertion implies.

It is true, that, if the account of Captain Flinders's observations is correct, the table given in this paper is not applicable to such a ship as Captain Flinders's, in the neighbourhood of the equator, nor in the southern hemisphere; and that if the effect which was simply called *regular* in the first *unpublished* impression of the paper, had been considered by the seamen intrusted with it for trial, as the principal or only effect, though the author distinctly pointed out another effect, as frequently occurring, under the name of the *irregular* attraction, they might have been misled by the incautious application of the table to all cases indiscriminately.

It is also true, that Mr. Barlow and Mr. Lecount have ascertained, that cast iron is capable of producing more distinct effects, by its induced magnetism, than might have been expected by those who understood the term of *soft* iron, as synonymous with conducting, in too literal a sense: and if Mr. Barlow had claimed this as a discovery, and if he had also claimed the merit of having first experimentally ascertained the truth of his very ingenious friend Charles Bonnycastle's theoretical assertion, that the induced magnetism of a shell ought to be equal to that of a solid sphere; it would have become the duty of those, who sat in judgment upon his papers, to examine how far these discoveries were actually altogether

original, and how far they deserved to be announced to the world as such: but if such a publication could not be encouraged without admitting the originality of other facts, which they supposed to be known to every student of natural philosophy, they had surely some reason to hesitate, before they gave it their unqualified approbation.

vi. *Third Report of the Commissioners appointed by His Majesty to consider the subject of Weights and Measures.*

MAY IT PLEASE YOUR MAJESTY,

WE, the commissioners appointed by your Majesty, for the purpose of considering the subject of weights and measures, have now completed the examination of the standards which we have thought it necessary to compare. The measurements which we have lately performed, upon the apparatus employed by the late Sir George Shuckburgh Evelyn, have enabled us to determine, with sufficient precision, the weight of a given bulk of water, with a view to the fixing the magnitude of the standard of weight, that of length being already determined by the experiments related in our former Reports, and we have found by the computations, which will be detailed in the Appendix, that the weight of a cubic inch of distilled water, at 62° of Fahrenheit, is 252.72 grains of the Parliamentary standard pound of 1758, supposing it to be weighed in a vacuum.

We beg leave therefore finally to recommend with all humility, to your Majesty, the adoption of the regulations and modifications suggested in our former Reports; which are principally these

1.—That the parliamentary standard yard, made by Bird in 1760, be henceforwards considered as the authentic legal standard of the British empire; and that it be identified by declaring, that 39.1393 inches of this standard, at the temperature of 62° of Fahrenheit, have been found equal to the length of a pendulum supposed to vibrate seconds in London, on the level of the sea, and in a vacuum.

2.—That the parliamentary standard troy pound, according to the two pound weight made in 1758, remain unaltered, and that 7000 troy grains be declared to constitute an avoirdupois pound, the cubic inch of distilled water being found to weigh at 62°, in a vacuum, 252.72 parliamentary grains.

3.—That the ale and corn gallon be restored to their original equality, by taking for the statutable common gallon of the British empire, a mean value, such that a gallon of common water may weigh 10 pounds avoirdupois in ordinary circumstances, its content being nearly 277.3 cubic inches; and that correct standards of this imperial gallon, and of the bushel, peck,

quart and pint derived from it, and of their parts, be procured without delay for the Exchequer, and for such other offices in your Majesty's dominions, as may be judged most convenient for the ready use of your Majesty's subjects.

4.—Whether any further legislative enactments are required, for enforcing a uniformity of practice throughout the British empire, we do not feel ourselves competent to determine. But it appears to us, that nothing would be more conducive to the attainment of this end, than to increase, as far as possible, the facility of a ready recurrence to the legal standards, which we apprehend to be in a great measure attainable by the means that we have recommended: it would also, in all probability, be of advantage to give a greater degree of publicity to the Appendix of our last Report, containing a comparison of the customary measures employed throughout the country.

5.—We are not aware that any further services remain for us to perform in the execution of the commands laid upon us by Your Majesty's commission; but, if any superintendence of the regulations to be adopted were thought necessary, we should still be ready to undertake such inspections and examinations, as might be required for the complete attainment of the objects in question.

London,
31 *March*, 1831.

(Signed) George Clerk.
Davies Gilbert.
W. H. Wollaston.
Thomas Young.
Henry Kater.

APPENDIX.

The commissioners having been furnished, by the kindness of the Honourable Charles C. C. Jenkinson, with the apparatus employed by the late Sir George Shuckburgh Evelyn, in the determination of the magnitude of the standard weights, and there being some doubt of the perfect accuracy of his method of measuring the capacity of the bodies employed, it was judged necessary to repeat that measurement with greater precautions; and the results of Captain Kater's experiments have afforded some slight corrections of the capacities in question.

The sides of Sir George Shuckburgh's cube were found by Captain Kater equal to 4.98911, 4.98934, and 4.98935 inches; the diameter of the cylinder 3.99713, and and its length 5.99600 inches; and the diameter of the sphere 6.00759 inches. Hence the content of the cube appears to be 194.1969 inches; that of the cylinder 75.2398; and that of the sphere 113.5264 inches of Bird's parliamentary standard of 1760, recommended in the last Report of the commissioners, or of the standard made by Troughton for Sir George Shuckburgh.

The difference of the weight of the cube in air at 62°, with the barometer at 29.0, and in water at 60.2°, was 31381.79 grains; and adding to this the weight of an equal bulk of the air at 62°, which is $\frac{1}{773}\cdot\frac{29}{30}$ of that of the water, or 36.26 grains, and subtracting from it $\frac{1}{770}$ of this, or 4.26 grains, the buoyancy of the brass weights, we obtain 31413.79 grains for the weight of the cube of water in a vacuum at 60.2°. Now this cube is less than the supposed measure at the standard temperature of 62°, in the ratio of 1 to 1.0000567, on account of the contraction of the brass; and the water is denser than at the standard temperature, according to Mr. Gilpin's experiments, in the ratio of .99998 to .99981, or of 1.00017 to 1, the whole correction, for the difference of 1.8°, being .0001133, or 3.55 grains, making 31410.24. for the weight of the cube of water in a vacuum at 62°; which, divided by 124.1969, gives 252.907 for the weight of a cubic inch in Sir George Shuckburgh's grains.

In the same manner we obtain for the cylinder, which was weighed in air under the same circumstances, and in water at 60.5°, the difference being 19006.83 grains, the correction $\frac{1}{773}\cdot\frac{27}{30}\cdot\frac{7}{8}$ for the effect of buoyancy amounting to 19.43 grains, and for the difference of temperature of the water and brass conjointly, the densities being .999955 and .999910, the correction .000145 − .000047 = .000098, or 1.86 grains leaving + 17.57 grains for the whole correction of the weight, as reduced to a vacuum at 62°, and making it 19024.40, which divided by 75.2398, the content of the cylinder, affords us 252.851, for the cubic inch in a vacuum at 62°.

The sphere was weighed in air at 67°, the barometer standing at 29.71, the correction for buoyancy is here $\frac{7}{8}\cdot\frac{29}{30}\cdot\frac{1}{773}$, or for 28673.51 grains, 29.79; while the temperature of 66° requires, for the difference between the expansion of brass and water, the addition of .00042 − .000126, or .000294 of the whole, that is + 8.43 grains, making the whole correction 38.15, and the weight in a vacuum 28711.66; which, divided by 113.5264, gives us 252.907, for the cubic inch in a vacuum.

The mean of these three measures is 252.888, giving for the three errors +.019, −.037, and +.019; and this mean, reduced to the parliamentary standard, makes 252.722 grains, for the cubic inch of distilled water at 62°, weighed in a vacuum, or 252.456 in air, under the common circumstances of the atmosphere, when weights of brass are employed. In a vacuum at the maximum of density, that is at 39°, the weight of a true cubic inch will be 253 grains, and of a cubic decimetre 15440*. The proposed Imperial Gallon, of ten pounds, or 70000 grains, of water, will contain very nearly 277.3 cubic inches, under common circumstances.

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*It appears, however, from an official Report, obligingly communicated to us by Dr Kelly, that the actual standard chiliogramme has been found to contain only 15433 English grains

Art. XVII. *Miscellaneous Intelligence.*

I. Mechanical Science.

§ The Arts, Agricultural Economy, &c.

1. *Improvement of Oil Lamps.*—MM. Arago and Fresnel have lately applied the principle of Count Rumford's concentric or co-lateral meshes to the improvement of lamps, intended either for light-houses or theatres, or for other uses where a strong bright clear light is wanted. In order to obviate the difficulty which was formerly found to arise from the carbonization of the wick by the great heat occasioned at the summit of the burner, the oil was made to flow over at the mesh, in the manner proposed and adopted by M. Carcel; and in thus keeping the flame at the top of the wick, a full, clear and steady combustion was obtained. Many circumstances require attention in order that these lamps produce their best effect; as the space between the meshes, the size of the air canals, the height of the wick, the magnitude of the reservoir, &c. When perfect, the experiments made with them, though they seemed to indicate a slight degree of saving in the oil required to produce a certain quantity of light with lamps having two wicks, yet they did not, as a general result, with three and four wicks, justify the opinions of Count Rumford; and the quantity of light produced, was about the same as what would have been given by the same quantity of oil burned in other economical methods. The principal advantage is the power of concentrating all the light into one focus, so that when advantageously placed, as on the centre of a light-house furnished with lens, the greatest quantity may pass from one point through the lenses. The light of these lamps is very regular; for, after twelve or thirteen hours' burning, it does not diminish more than one-fifth,—at least such was the result with a four-wicked lamp placed in the focus of a large lens. The quantity of oil allowed to flow over at the wick, should be at least equal to that which is burned. It is not apparently injured, and is to be returned into the reservoir. In place of the apparatus employed by M. Carcel, to make the oil rise to the wick, MM. Arago and Fresnel placed the reservoir of oil above the height of the burner; and then, by an open moveable tube, which passed into it from the air, the level up to which the oil was required to flow was easily regulated.—*Annales de Chimie*, xvi, p. 377.

2. *Coal-Oil Parish-Lamps.*—It is now some time since the volatile oil, obtained by distilling coal and coal-tar, has been applied in place of animal oil, in producing light. Large quantities of this fluid are prepared at once from the coal in

Scotland, and much is also obtained by distilling coal-tar. When pure, it is limpid and colourless, and closely resembles, if it be not identical with, naphtha. A large district about Fitzroy-square and Charlotte-street has been lighted by this fluid, burned in lamps particularly constructed for it by Major Cochrane; they are patent, as well also as the application of the oil to this purpose. The flame in these lamps is very short, but extremely bright, and certainly far surpasses a common street gas flame in that respect, if it does not also an Argand burner supplied by coal-gas. It has happened now and then, when the wick has been too high, and the oil used has been obtained from coal-tar, that the flame has smoked, the wick become charred, and at times so much vapour has collected in the lamp as at last to explode and burst it to pieces; but this has not happened with the Scotch oil. The lamps in the district before mentioned, have now been in use for a considerable time, and are found to be attended with perfect success.

3. *Lithography.*—A society has been formed at Munich for the imitation of oriental MSS.; the object is by means of lithography to multiply copies of the best works which are extant in the Turkish, Arabic, Persian, and Tartar tongues, and to dispose of them in the East, by the port of Trieste. The cabals of those, whose business it is to write MSS., and the different ornaments with which the Turks and Arabs adorn their writings, have been obstacles to this design hitherto; but by the aid of Lithography, the difficulty it is thought may be overcome. Thus the cheapness of that mode of engraving will contribute to spread to an unlimited extent, the treasures of the best writers of the East.

A lithographic establishment has also been formed in London, for the purpose of facilitating the progress of this branch of art, at No. 1, Wellington-street. Series of the impressions taken from copies of the pictures in the Munich gallery are to be seen there, and give an idea of the powers of the art, far beyond what could possibly be imagined by those who know of it only from description. It contains also a large deposit of Foreign and British Materials, for the prosecution of this pursuit, and many of the finest results that have been produced by it.

4. *On the Potash to be obtained from the Stalks of Potatoes.*

[In a Letter to the Editor.]

Dear Sir,

In *Tillock's Journal* for November 1817, there is inserted an article on the manufacture of potash from the stalks of potatoes. The experiments are said to have been executed first in France, and the results are all given in weight, from

that of the green vegetable to the incinaration of the mixed saline mass, commonly known in our markets by the name of pearlash. The "immense advantages," however, which are to be derived from this practice, seem to depend on a fallacy which is of such a nature, that it appears rather to be the consequences of a direct mis-statement than of any conceivable error. The French statement is indeed followed by a comparative experiment made in Ireland by Mr. Rice, the results of which are far different, and such as to confirm the suspicion here stated. According to the French experiment, the produce of potash per acre, is above 2,000 lbs.; in Mr. Rice's, it is only 201¼ lbs. It is true that owing to differences in the method of burning vegetables, whether terrestrial or marine, they are found to yield very different proportions of alkali, from circumstances respecting the peculiar nature of these substances, the mode of their existence in vegetables, and the changes they undergo in the fire, with which we are not yet acquainted. But as the method of drying and burning are fully described in the original experiment and appear to have been accurately followed in Ireland, this cannot account for the extraordinary differences in the results respectively obtained.

Being desirous of further verifying the French statement, which, certainly, if correct, offered no small temptation to agriculturists, I requested my friend Sir John Hay, Bart., to make a large experiment for that end, on his farm near Peebles; and as it was executed with that accuracy which characterizes his whole system of agriculture, his well-known reputation will afford sufficient proof that it was worthy of reliance. I ought, however, to add, that lest any thing should arise to throw a doubt on the event, the directions given in the narrative of the French experiment, were implicitly followed in every particular, from the cutting to the burning of the plant, and that the ashes were weighed, lixiviated, and examined by myself.

The result of two trials on two separate acres, follow; and the Scotch acre, it must be remembered, is one seventh larger than the English. It is presumed that in the original statement the measures were reduced to the English acre. The first acre was a rich loamy soil at King's Meadows; the potatoes were drilled, and produced a good crop. They were cut, as directed, immediately after flowering, left ten days to dry, and burnt in a pit. the produce was 222 lbs. of ashes, and, on lixiviation and drying, these yielded 55 lbs. of impure potash, or mixed salts.

The second acre was a clayey wet soil, with a retentive bottom; but the crop, which was also drilled, was considered moderate. These stems were treated in the same manner; but the burning had been more complete, as the ashes contained less charcoal than the preceding. They only weighed 112 lbs., and produced 28 lbs. of impure potash.

Thus it appears that, in the Irish experiment, the potash procured was scarcely one-twentieth part of that which was said to have been obtained in the French; while, in the two experiments made in Scotland, the produce, in one instance, was only the half, and, in another, only the quarter of that which was obtained from the trial made in Ireland.

It is probable that the Irish acre, if computed according to the lazy-bed system of cultivation, contained more plants than the drilled Scotch acre; and thus the differences of produce between Mr. Rice's acre, and that at King's Meadows, will not be very difficult to reconcile; while the scantiness of the second crop, tried in Scotland, may also suffice to account for the still greater diminution of the alkaline product in that case. Both these trials therefore sufficiently confirm each other; particularly when we further consider the differences in the proportion of saline ingredients which the same plant exhibits in different situations and circumstances, and those further differences in the alkaline product which arise from variations in the treatment previous to burning and during combustion. The French experiment, however, leaves them at an incalculable and incredible distance.

Taking a mean result then from the experiments made in Ireland and Scotland, or even admitting the former to afford a better standard, there is evidently no temptation for agriculturists to repeat these trials with a view to profit. It appears on analysis, that the dry saline residuum, here called in compliance with the French statement, impure potash, does not contain above ten per cent of pure alkali, the remainder consisting of muriate of potash and other ingredients; and it is evident that a much larger quantity would not repay the expenses incurred in the operation,

I am yours, &c.,
J. Mac Culloch.

5. *Apple Bread.*—M. Duduit de Maizieres, a French officer of the king's household, has invented, and practised with great success, a method of making bread with common apples very far superior to potato bread. After having boiled one-third of peeled apples, he bruised them while quite warm into two-thirds of flour, including the proper quantity of yeast, and kneaded the whole without water, the juice of the fruit being quite sufficient. When this mixture had acquired the consistency of paste he put it into a vessel, in which he allowed it to rise for about twelve hours. By this process he obtained a very excellent bread, full of eyes and extremely palatable and light.—*New Monthly Mag.*

6. *New Musical Instrument.*—A musical instrument is now in

London, called the Terpodion. It is the invention of M. Buschman, who has lately brought it from the continent. Its effect is striking, and astonishing, for it combines the sweetness of the flute and clarionet, with the energy of the horn and bassoon, and yields a full and rich harmony, resembling an orchestra of wind instruments. The sounds may be continued at pleasure with any degree of strength, and the action of the instrument resembles that of the ediphone, but it is described by the inventor as consisting entirely of wood, and it is understood that the sounds are produced by the vibration of wooden staves; its construction is said to be cheap, and its state unalterable by the weather.

7. *Large reflecting Telescope.*—Mr. J. Ramage of Aberdeen has constructed a twenty-five feet reflecting telescope, the largest except that of Sir W. Herschel ever made. The speculum is twenty-five feet focal length and fifteen inches diameter, the power from 50 to 1,500, and the mechanism by which the observer and instrument are moved, is simple and well contrived.

8. *Iron Bridges.*—Carthage bridge on the Genessee river in the state of New York, fell to pieces on the second of May (1820?). It was a single arch of iron, and for its extent and height stood unrivalled in America or in Europe. The arch consisted of nine ribs, its chord 352 feet, and height of the railing above the water 200 feet; the length of the floor 714 feet.—*New Monthly Magazine.*

9. *Prize Question.*—The following question has been proposed by the Society of Sciences and Arts of Utrecht. "What relation is there between speculative philosophy and mathematics? Why are mathematics necessary to philosophy, excluding their application to physics? and what means does speculative philosophy offer for the extensive and ultimate perfection of pure mathematics?"

II. Chemical Science.

§ Chemistry.

1. *Permeability of Iron to Tin.*—Mr. Smithson describes, in the *Annals of Philosophy*, vol. i., p. 276, an instance where tin had been forced through the pores of cast iron. It is adduced, in support of the opinion, that the capillary copper in the slag of the Hartz, has been formed by being pressed through minute pores. "For some purposes of the arts, Mr. Clement formed a cylinder of copper, and, to give it strength, introduced into it a hollow cylinder, or tube of cast iron. To complete the

union of these two cylinders, some melted tin was run between them. With the exact particulars of this construction, I am not acquainted; but the material circumstance is that, during the cooling of the heated mass, a portion of the melted tin was forced, by the alteration of volume of the cylinders, through the substance of the cast-iron cylinder, and issued over its internal surface in the state of fibres, which were curled and twisted in various directions. Such was the tenuity of these fibres of tin, that little tufts of them applied to the flame of a candle, took fire and burned like cotton."

Mr. Smithson suggests, that perhaps this penetration of solid matter, by other solids or fluids, by great pressure, may have useful applications, and produce some compounds very advantageous in the arts.

2. *Granulation of Copper.*—The following singular circumstance is related by Mr. W. Keates, of the Cheadle copper-works, to Mr. Phillips. "I send you some globules of copper, quite hollow, and so light as to swim on water; the history of which is as follows: one of our refining furnaces contained about 20 cwt. of melted copper, which was to be laded into blocks; but the refining process had not been carried far enough, so that when the men came to lade it out into the moulds, they found it to be impracticable, in consequence of its emitting such a great quantity of sulphurous acid vapour. They were therefore obliged to put it into a cistern of water, to granulate it; but by this operation, instead of the copper assuming the form of solid grains, the whole of it became in the form sent to you, and floated on the water like so many corks. What is the most probable explanation of this phenomenon? One of our refining men, during forty years' experience in the business, has never seen any thing similar." Mr. Phillips adds, that the globules of copper sent to him were light, and that, though they had lost the power of floating on water, they floated in sulphuric acid.—*Annals of Philosophy*, vol. i. p. 470.

3. *Selenium.*—In consequence of the repairing of the leaden chamber of the sulphuric acid works, at Gripsholm, a quantity of a substance has been collected, consisting principally of sulphur, impregnated with selenium. This has been forwarded by Professor Berzelius, to Mr. Allen, of Plough-court, Lombard-street; and is to be sold in small quantities, for the benefit of the proprietors. A translation of the process recommended by Professor Berzelius, for the separation of the selenium is given with the substance.

4. *Chromic and Sulphuric Acids.*—When sulphuric acid is boiled on chromate of lead or baryta in excess, the chromic

acid obtained is not pure, but contains sulphuric acid. The liquid containing the two acids, when successively evaporated, entirely crystallizes in small quadrangular prisms of a deep red colour. If the heat and concentration be carried too far, oxygen is disengaged, and sulphate of green oxide of chromium found. These crystals are deliquescent, and contain one atom of each of the acids. To analyze them, they were boiled with a mixture of muriatic acid and alcohol, so as to convert the chromic acid into green oxide; then dividing the liquid into two parts, one was precipitated by muriate of baryta, to give the sulphuric acid; and the other by ammonia, for the oxide of chrome, and consequently the chromic acid.

Alcohol easily dissolves this substance, and, if strong, so rapid a decomposition is produced, as to resemble an explosion. The chromic acid becomes oxide of chromium, and a particular ethereal odour is produced. Having ascertained that the same odour was produced by treating peroxide of manganese with alcohol and sulphuric acid, I collected some of this ethereal fluid by distillation, and rectified it on lime to separate water, and on chloride of calcium to separate alcohol. It was then of an acrid burning taste, and very penetrating odour, resembling sulphuric ether. When mixed with water, it separated into a stratum of sulphuric ether, and a white transparent light oil, identical with the sweet oil of wine. The mixture of alcohol, sulphuric acid, and black oxide of manganese, that had been used, contained much sulphate of manganese, but no hyposulphuric acid.

Hence, in treating alcohol by chromic and sulphuric acid, or by the latter and peroxide of manganese, it appears to undergo the same alteration, as by sulphuric acid alone. Sulphuric ether and sweet oil of wine are formed by means of the oxygen of the chromic acid, or of the peroxide of manganese. The sulphuric acid suffers no alteration, but its presence is necessary to determine the decomposition of the alcohol and the partial deoxidation of the chromic acid, or peroxide, in consequence of its affinity for the oxides of chromium and manganese. I do not doubt but that it might be replaced by many other acids.

M. Gay Lussac, to whom these experiments are due, then notices, that Scheele and Dobereiner had noticed effects relative to this subject: Scheele remarked the ethereal smell, &c., produced by the action of peroxide of manganese, sulphuric acid and alcohol, and distilling slowly; and Dobereiner had observed a similar odour in a mixture of chromate of potassa, sulphuric acid, and alcohol.—*Annales de Chimie*, vol. xvi. p. 103.

5. Native Carbonate of Magnesia.—Dr. Henry has published an account of a native carbonate of magnesia, brought from the

East Indies by Mr. Babington. It occurs massive; its colour is snow white, with the exception of a few small dots and stripes of ochre-yellow; its fracture is small conchoidal, passing into uneven; it gives sparks with steel, is not easily scraped by a knife, but is not hard enough to scratch fluor spar; its fragments are sharp-edged. Internally it has no lustre; it is very slightly transparent, and that only at the edges. Its specific gravity is 2.5615; its locality is not known.

It dissolves in acids slowly when cold, even though powdered; but heat quickens the solution, and its accompanying effervescence. When analyzed, 100 grains gave

Magnesia	46
Carbonic acid	51
Insoluble matter . . .	1.5
Water	0.5
Loss	1.

so that it is nearly a pure carbonate of magnesia.

Dr. Henry, at the conclusion of this analysis, expresses his doubts of the existence of a true bi-carbonate of magnesia. They are founded on the analysis of a salt prepared by himself, by mixing dilute solution of sulphate of magnesia with a solution of carbonate of soda, charged with carbonic acid; they consisted of 29 base, 30 acid, and 41 water.—*Annals of Philosophy*, i. p. 254.

6. *On Compounds of Sulphur with Cyanogen, &c.*—M. Berzelius, in pursuance of his researches on the compounds of cyanogen, (p. 205,) has lately examined the sulphuretted compounds of cyanogen, and added much to our knowledge of them. He concludes that the substance, as prepared by M. Grotthus or M. Vogel, (*i. e.*, by fusing sulphur with ferro prussiate of potassa, dissolving, filtering, and drying,) is a sulpho-cyanuret of potassium; and though he has not been able to separate the sulpho-cyanogen or sulphuret of cyanogen from the base, so as to have it in a separate state, yet he deduces its composition from experiments, as being 1 atom cyanogen, and 2 atoms of sulphur, or

Carbon	20.63	2 atoms	150.66
Azote	24.28	1 atom	177.26
Sulphur	55.09	2 atoms	402.32
	100.		730.24

The sulpho-cyanuret of potassium is composed of

Potassium	40.15	1 atom . .	.	979.83
Azote	14.53	2 atoms	354.52	
Carbon	12 35	4 atoms	301.32	1460.48
Sulphur	32.97	4 atoms	804.64	
	100.			2440.31

The sulphuretted hydro-cyanic acid is composed of

Hydrogen	1.68	2 atoms	12.44
Nitrogen	23.85	1 atom	177.26
Carbon	20.30	2 atoms	150.66
Sulphur	54.17	2 atoms	402.32
	100.		772.68

In considering the nature of these substances, M. Berzelius seems inclined to admit that view of them and their nature which is analogous to the chlorine theory, as also this theory itself, and goes a considerable way towards answering some of those objections which have been raised at different times to it.

On substituting selenium for sulphur in these and analogous experiments, results which might have been expected from the analogy of the two bodies took place; on heating it with the ferro-prussiate of potassa, a selenio-cyanuret of potassa was formed perfectly analogous to the sulpho-cyanuret.—*Ann. de Chim.* xvi. 23.

7. *Sub-sulphate of Alumina and Potassa.*—This salt has lately been examined, and its constitution made out by M. Anatole Riffault. It was prepared by adding potassa to a boiling solution of alum until nearly saturated; a precipitate fell, which, when well washed and dried at a heat below 212°, was the substance in question. On analysis, it gave sulphuric acid 36.187; alumina 35.165; potassa 10.824; water 17.824; which accords with

1 atom sulphate of potassa	109.10	19.654
3 atoms subsulphate of alumina	344.84	52.116
9 atoms water . . .	101.18	18.230

Such being the nature of this salt, and such its accordance with the atomic theory, attempts were made to produce a similar compound of sulphuric acid and alumine with ammonia. This was obtained by pouring solution of ammonia into a boiling solution of ammoniacal alum. The precipitate, when well washed and dried, was analyzed, and found to consist of sulphuric acid 38.248, and of alumina 37.851 *per cent.*, which accords very nearly with the full or theoretical number.

4 atoms sulphuric acid . .	200.46	38.724
9 atoms alumina . . .	194.49	37.572
1 atom ammonia . . .	21.55	4.164
9 atoms water . . .	101.18	19 540
	517.69	100

Ann. de Chimie. xvi. p. 355.

8. *Analysis of Verdigris.*—Mr. Phillips has lately analyzed the acetate of copper by the following process. A given weight

was decomposed by being boiled with excess of hydrate of lime, which threw down all the oxide of copper, and formed an acetate of lime; this was filtered, and carbonic acid passed through the solution to separate any free lime dissolved in it; it was then heated, that the excess of carbonic acid and carbonate of lime might separate, and was then neutral acetate of lime, containing the acetic acid of the verdigris. The lime was then thrown down by carbonate of soda, and the carbonate of lime ascertained, and the quantity of acetic acid in the verdigris deduced from its quantity; 63 of carbonate lime being considered as equivalent to 63.96 of acetic acid. The quantity of oxide of copper in the salt was ascertained by precipitating the salt with potassa, and drying the precipitate. The water was deduced from the loss of weight. In this way 100 of acetate of copper appeared to be composed of

Acetic acid	49.2
Peroxide of copper . . .	39.2
Water	11.6

so that admitting peroxide of copper to be a compound of two atoms oxygen 20, and one atom copper 80, the atomic constitution of verdigris will be

	By Theory.	By Experiment.
2 atoms of acetic acid	127.92	128.84
1 atom of peroxide of copper	100.	102.65
3 atoms of water . .	33.96	30.39
	261.88	261.88

Mr. Phillips, in addition to Dr. Thomson's arguments for concluding the sulphate of copper to be a bi-salt, states, that if finely-divided carbonate of lime be added to a solution of it, an effervescence takes place, and an insoluble sulphate of copper falls. The same effect is produced by the soluble sulphate, nitrate, and acetate of copper, and hence Mr. P. is inclined to conclude that these also are bi-salts.—*Annals of Philosophy*, i. 417.

9. *Analysis of Gun-Powder.*—The usual mode of analyzing gun-powder is to take a given weight of it well dried, to dissolve out the nitre, to evaporate the solution and get the weight of nitre, and to get the weight of charcoal and sulphur by drying them together and weighing them. Another portion of gun-powder is then taken and heated with as much potassa and a little water, which dissolves out the nitre and sulphur, leaving the charcoal, which, being washed and dried, is weighed. The sulphur is estimated by the weight wanted to complete the sum of the weight of the nitre and charcoal.

Another mode has been adopted in the laboratory of the direction of powders, which is shortly as follows. A certain

portion of the powder is first dried and the loss of water ascertained; the nitre is then obtained by washing the powder, evaporating the solution, and fusing the nitre. To obtain the sulphur, 5 grains of powder, 5 of sub-carbonate of potassa, 5 of nitre, and 20 of common salt, all of them free from sulphuric acid, are mixed very intimately, and placed on the fire in a platinum vessel. The sulphur burns slowly, and the mass becomes white. It is then dissolved, and the solution being saturated with nitric or muriatic acid, the sulphuric acid is precipitated by muriate of baryta, the quantity of sulphate of baryta ascertained, and the sulphur estimated from its weight. The sulphate of baryta may either be collected on a filter and weighed, or it may be estimated by using a solution of muriate of baryta of known strength, adding it carefully until precipitation ceases; the quantity of solution used, gives by a direct proportion the quantity of sulphur in the gun-powder. The charcoal is estimated from the loss of weight.

Potassa, with some precaution, may be used for the sub-carbonate, and a capsule, a matrass, or even a tube of glass, may be used instead of the platinum capsule.—*Annales de Chimie*, xvi. p. 437.

10. *On firing Gun-Powder by Electricity*, by M. Leuthwaite.

Dear Sir,—I have been induced to try a few experiments on the firing of gun-powder by means of the electrical shock, with the view of ascertaining the effect produced by making various fluids part of the conducting chain for the discharge. This inflammation has generally been considered as difficult to be produced. Probably these observations may tend to lessen the difficulty, and illustrate, at the same time, the conducting power of fluids for electricity.

The jar made use of contained one square foot of coated surface, and when charged with sufficient intensity to raise the quadrant electrometer to 90°, always discharged itself spontaneously.

The glass tube used was six inches long, the bore $\frac{3}{10}$ of an inch in diameter. A cork was fitted into each end, through which a brass wire was introduced, and the capacity of the tube was filled with different fluids.

I first ascertained from the mean of several experiments, that the gun-powder could not be fired through water, when the quadrant electrometer stood at a less number of degrees than 60, at which it always fired.

I then filled the same tube with sulphuric ether, and found from the mean of several experiments, that it would not fire the powder at a less number of degrees than 60. But when the tube was filled with alcohol, it always fired the powder at 30°.

The tube was, lastly, filled alternately with sulphuric and muriatic acid, and a discharge made through them when the electrometer indicated 80° of intensity, at which the gunpowder would not take fire.

Should you consider any of the foregoing experiments of importance, I shall leave you to make what use of them you may think proper. I am, &c.

Rotherhithe, 24th May, 1821. J. LEUTHWAITE.

11. *Use of Chromate of Lead as a Dye.*—The following observations on this subject are by M. Berthier: "The chromate of lead applies very well on stuffs, and I have many times repeated the experiment. The following are some remarks I have made.

"With subacetate of lead and neutral chromate of potassa, only an orange colour is obtained, not very agreeable; but if the stuffs thus dyed be dipped in acetic acid, they almost immediately acquire a very fine and brilliant yellow lemon colour. On using the neutral acetate of lead in place of the subacetate, a fine gold colour is immediately obtained, with the chromate of potassa, but acetic acid cannot give it the yellow lemon colour.

"These colours are absolutely unalterable by soap and water when cold; at boiling temperatures they fade a little, without any change of tint, but vinegar restores their first brilliancy.

"Ammonia makes them of a red orange colour; acetic acid restores them to their primitive state. When chromate of lead is treated by ammonia, it may be made to pass through a great variety of shades from orange to the red of the finest minium. The ammonia in these cases dissolves chromic acid, and leaves a red chromate. The action of acid, either nitric or acetic, is to dissolve oxide of lead from this chromate, and leave the clear yellow compound.

"Stuffs dyed by the chromate of lead, have their colours immediately and completely destroyed by the subacetate of soda and by muriatic acid, even when cold."—*Annales des Mines*, vi. 137.

12. *Porcelain Glaze.*—Mr. Rose, of Coalport, Shropshire, has sent a communication to the Society of Arts, in which a new glaze for porcelain is described. The object was to produce a glaze containing no lead, so that colours afterwards laid on to, and burned into, it, should not be altered by that metal. The principal ingredient in it is feldspar, of a somewhat compact texture, and a pale flesh red-colour, which forms veins in a slaty rock, adjoining to the town of Welchpool, in Montgomeryshire. This material being freed from all adhering pieces of slate and quartz, is ground to a fine powder; and, being thus prepared, 27 parts are mixed with 18 of borax, 4 of

Lynn sand, 3 of nitre, 3 of soda, and 3 of Cornwall china clay. This mixture is melted into a frit, and is then ground to a fine powder, 3 parts of calcined borax being previously added.

Some specimens furnished by Mr. Rose, were placed in the hands of Mr. Muss and other artists, to be submitted to experiments. When placed in heats much higher than they would be subjected to in the fair course of enamelling, the glaze remained firm and perfectly uniform, without any specks or splits having been produced on its surface; the colours, even the pinks and chrome greens, coming out remarkably well upon it, and none of them chipping off, as is frequently the case with the colours of the French porcelain.—*Transactions of the Society of Arts*, 1820.

13. *On preventing the Ravages of Moths in woollen Cloth.*—The discovery of an easy and effectual method of preventing the destruction of woollen fabrics and furs by moths, has long been a subject of research, and it still stands, I believe, among the list of premiums in the promises of the Society of Arts. Although the process here in question is known to many individuals, it is not yet known to the public at large, and your Journal offers the means of diffusing it.

The discovery, although accidental, is due to the officers of Artillery at Woolwich, employed in the inspection of clothing returned from Spain. It was observed, that in casks where all other woollen substances were totally destroyed, those cloths that had been rendered water-proof by the common well known processes, remain untouched. Attention having thus been excited to this circumstance, other similar mixed packages were examined, and the results were found to be invariable.

This process has the advantage of being cheap, easy of application, and permanent; since the chemical change produced by it in the surface of the woollen fibre, is not liable to be affected by time. If, in the case of military stores, no other good result were to follow the use of the water-proof process, this would be a sufficient reason for its universal adoption. The effect of all the odorous bodies commonly used for this purpose is transitory, as they evaporate in the course of time; but the aluminous soap which becomes united to the animal fibre in the water-proof process, seems to disgust this destructive larva so as effectually to prevent it, like some dyes, from attempting to devour the wool or other animal hairs, which are its natural food.

There seems no reason why this process should not be adopted in furs for the same purpose; since great losses are occasionally sustained by their destruction. It might with equal ease be applied to them; and as it does not appear to produce any effect on the appearance of the woollen sub-

stances to which it is applied, it would probably cause no change in the brilliancy or beauty of those substances, so justly valued for their utility and beauty, and so difficult to preserve without the most watchful attention.

J. M.

14. *Decomposition of Blood.*—M. Vauquelin had occasion to observe the changes produced in five years, in the fluid obtained by washing coagulated bullocks' blood. It appears at first to have contained the serum, and a considerable portion of the colouring matter of the blood, and the results at the end of the time mentioned were·

1. A large quantity of carbonic acid.
2. A large quantity of sulphuretted hydrogen.
3. A large quantity of acetic acid.
4. Ammonia which saturated these acids.
5. An acid and very fetid volatile oil, saturating part of the ammonia. These substances did not exist previously in the blood.
6. It appears that the fixed fatty matter which was found, had existed in the blood previously, as a similar matter was found in recent blood.
7. That the albumen was almost entirely decomposed, slight traces only remaining, and so altered in its nature that it rather resembled glue than albumen.
8. That the colouring matter remained entirely unchanged.
9. That the blood did not appear to contain phosphorus.

M. Vauquelin remarks that the quantity of sulphur in blood is much larger than is generally imagined, amounting to two grammes (about 30 grains) in a litre (2¼ pints). This sulphur had separated spontaneously from the fluid, and formed a ring just above its surface on the glass.—*Ann. de Chim.* xvi. p. 363.

15. *Diod grùfol.*—A liquor is brewed from the berries of the mountain ash, in North Wales, called diod grùfol, by only crushing and putting water to them. After standing for a fortnight it is fit for use, its flavour somewhat resembles perry.

16. *Formation of Alcohol, by fluoboric Gas.*—Some very interesting experiments are detailed in a short paper published in the *Annales de Chimie*, xvi. p. 72, on the action of fluoboric gas on alcohol. They are by M. Desfosses of Besançon. The gas was sent into a portion of alcohol, which became very ethereal in odour, and very acid, even so as to fume. The fluid was distilled, and then rectified, first from potassa and afterwards from chloride of calcium. The ether thus obtained, was entirely analogous to sulphuric ether. It burnt like it, and gave no acid fumes. The specific gravity was .75, being rather greater than that of pure ether, but it had not been washed so

as to separate the alcohol from it. When decomposed in a tube heated red, it gave much carburetted hydrogen; but the water through which the gas had passed, was not at all acid, and did not effect either lime-water or acetate of lead. Hence it appears, that the ether should be ranged with those produced by sulphuric, phosphoric, and arsenic acids.

To ascertain what the phenomena of this change were, alcohol was saturated with fluoboric gas; after some time it became turbid, and a black powder like charcoal was deposited. It was distilled in an apparatus which would collect the gas; none came over during the time that ether was passing, but at the end of the distillation a few bubbles were liberated, which, when well washed, troubled lime-water, and were inflammable, they were therefore mixtures of carburetted hydrogen and carbonic acid; and these gases from other experiments appear to be produced by the action of the gas on the alcohol, and not by free sulphuric acid. Though the evaporation was carried very far, no sweet oil of wine was formed. From hence it follows, says the author, that first, an ether analogous to sulphuric ether, may be obtained by the action of fluoboric gas on alcohol; and, secondly, that the etherification probably takes place in consequence of the affinity of fluoboric acid for water, that it produces no sweet oil of wine, and that the acid does not appear altered in its nature.—*Annales de Chimie*, xvi. p. 72.

17. *On the Ripening of Fruits.*—In consequence of a prize question set forth by the Academy of Sciences, for the year 1821, three papers were received on the ripening of fruits, their effect on the air, &c. Of these, one written by M. Berard of Montpellier, gained the prize, and it has since been published in the French Journals, *Annales de Chimie*, xvi. p. 152, 225. The memoir is long, and cannot well be abridged, but the author has himself given a summary at the end of his paper of which the following is a translation:

Fruit does not act like leaves on the air. The result of its action as well in light, as in darkness, is at every instant of its formation, a loss of carbon by the fruit, which combines with the oxygen of the air, and forms carbonic acid. This loss of carbon is essential to the ripening of the fruit, for when the fruit is placed in an atmosphere deprived of oxygen, this function becomes suspended, the ripening is stopped, and if the fruit remains attached to the tree, it dries up and dies.

A fruit which happens naturally to be enclosed in a shell may nevertheless ripen, because the membrane which forms the husk is permeable to the air. The communication between the external and internal air is so free that the two portions

are always of uniform composition, so that when the air thus contained is analyzed, it is always found to be of the same composition as atmospheric air.

When fruits separated from the tree, but capable of completing their own ripening, are placed in media free from oxygen, they do not ripen: the power, however, is only suspended, and may be re-established by placing the fruit in an atmosphere capable of taking carbon from it. But if the fruit remain too long in the first situation, although it preserves the same external appearance nearly, it has entirely lost the power of ripening.

Hence it results, that most fruits and especially those that do not require to remain on the tree, may be preserved for some time, and the pleasure they afford us thus prolonged. The most simple process consists in placing at the bottom of a bottle, a paste formed of lime, sulphate of iron, and water, and afterwards to introduce the said fruit, it having been pulled a few days before it would have been ripe. These fruits are to be kept from the bottom of the bottle, and as much as possible from each other, and the bottle to be closed by a cork and cement. The fruits are thus placed in an atmosphere free from oxygen, and may be preserved for a longer or shorter time according to their nature; peaches, prunes, and apricots from twenty days to a month; pears, and apples for three months. If they are withdrawn after this time, and exposed to the air, they ripen extremely well; but if the times mentioned are much exceeded they undergo a particular alteration, and will not ripen at all.

Ripe fruit exposed to the air rots and decays. In this case it first changes the oxygen of the surrounding air into carbonic acid, and then liberates from itself a large quantity of the same acid gas. It appears that the presence of oxygen gas is necessary to the rotting or decay of fruits, when it is absent, a different change takes place.

When the fruit cannot ripen except on the tree, its ripening is not produced by a chemical change of the substances it contained whilst still green, but by the change of new substances furnished to it by the tree, and when it appears to lose the acid taste it had in its unripe state, it is because that taste is hidden by the large quantity of sugar it receives in ripening.

In the fruits which ripen off the tree, the quantity of sugar is also found considerably to increase; and in this case, it must be formed at the expense of the substances previously in the fruit. Gum and lignin are the only principles, the proportion of which diminish at the same time; it is therefore natural to conclude, that it is the portions of these substances which have disappeared, that have been converted into sugar: and as the

lignin contains most carbon, it is natural to suppose it is from it the oxygen takes the carbon to form carbonic acid, that change so indispensable to ripening*.

Finally, the alteration the lignin suffers in the ripening, continues during the decay of the fruit. It becomes brown, and its decomposition occasions the formation of much carbonic acid. Sugar is also decomposed at this time, and it is to its disappearance, that the peculiar taste of decayed fruits is to be attributed. The sugar in its decomposition also gives rise, no doubt to the formation of carbonic acid.

18. *Crystallization of Sugar.*—M. H. Braconnot has pointed out the strong powers of crystallization in the sugar of barley, and has shewn a remarkable instance in which that highly-important arrangement of matter can take place without liquidity. This sugar, when newly prepared, is perfectly transparent, and of a very brittle and vitreous fracture, offering at this time no traces of crystallization; but when left to itself for some days, its surface becomes dull and crystalline, and the effect continues to increase until the whole of the sugar has crystallized. It has then lost some of its transparency, and is seen to consist of many rounded groups of needle-like crystals, which are most generally separated from each other by empty spaces. The sugar, thus crystallized, is much more brittle than before; its fracture presents a number of acicular diverging crystals, united in bundles terminated by the interstices, especially when this arrangement has been produced at temperatures below the common temperature. When held in the mouth, it does not take on a bright smooth surface, but becomes rough, and by care small crystals may be separated, which by the microscope appear to be flat tetrahedral crystals. It was at first supposed that this substance had attracted water from the atmosphere, and in that way been enabled to have such motion produced among its particles as to allow of crystallization; but when placed in a close vessel with chloride of lime, when it lost $\frac{1}{100}$ of its weight, still it crystallized as in the open air. It also crystallized when immersed in oil of turpentine.

Confectioners know, and have long feared, the effects of this crystallization; they considered it as a degradation of the sugar in its nature, and search continually for means to pre-

* M. Berard in a note says, it is difficult to suppose that in those fruits that ripen early on the tree, all the sugar should be sent into the fruit from the plant; it is more probable that the fruit acts on the air, and forms sugar like the other fruits, but not in sufficient quantity, and that therefore, it is necessary recourse should be had to the tree, to complete its ripening.

vent it but apparently without success.—*Annales de Chimie*, xvi. p. 427.

19. *Cathartine, the active Principle of Senna.*—MM. J. L. Lassaigne and H. Fenuelle, in examining senna, obtained from it a particular principle called by them cathartine. A decoction of the leaves was made, and, after being filtered, was precipitated by acetate of lead. The precipitate collected was diffused through water, and sulphuretted hydrogen passed through it. The liquor filtered was evaporated to dryness, and digested in alcohol, and the alcohol solution then evaporated to dryness. It contained acetate of potassa, which was separated by alcohol acidulated by sulphuric acid; then filtering to separate the sulphate of potassa insoluble in this fluid, precipitating the excess of sulphuric acid by acetate of lead, decomposing this latter salt by sulphuretted hydrogen, filtering again and evaporating to dryness, a substance was obtained, which was considered the purgative principle of senna.

This substance is uncrystallizable, of a reddish yellow colour, of a particular smell, a bitter and nauseous taste. It is soluble in alcohol and water in all proportions; insoluble in ether. Its extract becomes moist in the air. It purges in very small doses.—*Annales de Chimie*, xvi. p. 20.

20. *Piperin, or the active Principle of Pepper.*—Piperin is a new vegetable principle, extracted from black pepper, by M. Pelletier. To obtain it, black pepper was digested in alcohol repeatedly, and the solution evaporated, until a fatty resinous matter was left. This, on being washed in warm water, was left of a good green colour, and had a hot and burning taste; it dissolved readily in alcohol, and less readily in sulphuric ether; concentrtaed sulphuric acid gave it a fine scarlet colour. A solution of this substance in hot alcohol, being left for some days, deposited a number of small crystals. These were purified by repeated solution and crystallization in alcohol and ether, and from the mother liquors, fresh portions were obtained, which, on purification, were like the first. It is to be remarked, that the pepper taste they possessed when impure, gradually left them as they became more and more pure; so that the white crystals scarcely had any taste, while it seemed to accumulate in the fatty matter, as the crystalline portion was separated from it: and also, that the purer the crystals, the finer the tint produced in them by sulphuric acid. The fatty matter left, also reddened by sulphuric acid; but it is a question whether it would do so when pure.

The crystalline matter forms colourless four-sided prisms, with single inclined terminations; they have scarcely any

taste. Boiling water dissolves a small portion; but it is insoluble in cold water; it is very soluble in alcohol, less so in ether; it is soluble in acetic acid, and crystallizes from it in feathery crystals. Weak sulphuric, nitric, and muriatic acids, do not dissolve or act on it; the strong acids decompose it. Strong sulphuric acid gives it a blood-red colour, which disappears on adding water; the substance does not seem altered if the acid has not remained long on it. Muriatic acid acts in the same way, producing a deep yellow colour. Nitric acid makes it greenish yellow, orange, and then red; the ultimate action of the acid produces oxalic acid, and yellow bitter principle. It melts at about 212°. Destructive distillation converts it into water, acetic acid, oil, and carburetted hydrogen, gas: no ammonia is formed. Oxide of copper converts it into carbonic acid and water. After comparing this substance with other vegetable principles, particularly with resins, M. Pelletier concludes by considering it a peculiar substance, and names it *Piperin*.

The fatty matter left, after extracting the piperin, is solid, at a temperature near 32°; but liquefies at a slight heat. It has an extremely bitter and acrid taste; it is very slightly volatile, and tends rather to decompose, than rise in vapour; that which passes over is not so piquant and acrid, as the undistilled part, but is more balsamic. It dissolves easily in alcohol and ether, and unites to fatty bodies; and, with the exception of its taste, does not differ from them. From the result of the distillation, it may be considered as composed of two oils: one volatile and balsamic; the other more fixed, and containing the acridness of the pepper.

Finally, M. Pelletier finds in pepper—1, Pipirin; 2, a very acrid concrete oil; 3, a volatile balsamic oil; 4, a gummy coloured matter; 5, an extractive principle; 6, malic and tartaric acids; 7, starch; 8, bassorine; 9, lignin: 10, earthy and alkaline salts. He concludes, also, there is no vegetable alkali in pepper; that the crystalline substance of pepper is a peculiar body; that pepper owes its taste to an oil but little volatile; and that a strong similarity exists between common pepper and cubebs, as illustrated by the analysis of M. Vauquelin, of the latter compared with the former.—*Annales de Chimie*, xvi. p.337.

21. *On Phosphorescence.*—The phenomena of phosphorescence, produced by exposure of bodies to light, have been very attentively observed lately by M. Heinrich, of Ratisbonne, who has made some new and interesting observations on them. The precautions taken by the observer were to remain, previous to the observation, thirty or forty minutes in a perfectly dark place; to expose the substances, the powers of which were to be observed for not more than ten seconds to the light of a

clear day; to keep them out of the rays of the sun, lest they should become heated; and to observe them in the same dark chamber to which he had previously retired. The observations were made in two different seasons, the summer and the winter. The following are the general results, but the account is very much compressed from M. Heinrich's paper.

Among natural bodies some are phosphorescent, some not phosphorescent. Among the most phosphorescent are some diamonds, but not all, though no apparent difference in their external appearances could be perceived; some remained luminous from five seconds to one hour. The different effects of the coloured rays were remarkable; a good diamond exposed in the blue rays acquired a durable phosphorescence, but did not become luminous at all in the red rays. All the fluor spars were highly phosphorescent, and also all the carbonates of lime.

M. Heinrich observes that the phosphorescence of calcareous combinations varies with the acid in combination. Thus the fluates were very phosphorescent, some of them remaining luminous for one hour. The carbonates follow; they are distinguished by the clear and white light they emit, which is such in some specimens as to enable a person to read by it, but it does not continue above thirty or forty-five seconds. The sulphates shine for a short time, but very faintly; the phosphates are still less favourable for the phenomena of phosphorescence by irradiation.

The calcareous combinations are succeeded by heavy spar or sulphate, and carbonate of baryta. Pure siliceous, aluminous and magnesian earths are not phosphorescent, though many of their native combinations are feebly so.

With regard to saline minerals, M. Heinrich considers their phosphorescent powers to be determined by the acid and base in combination. With the exception of amber and the diamond, no inflammable fossil becomes phosphorescent by irradiation. None of the metals are phosphorescent; metallic salts are moderately, and metallic oxides feebly, phosphorescent.

Vegetable substances are but bad phosphori. The wood of hot countries is better than that of our climate; the white hazel-tree shines brightly—an old sugar cane, dates, and the inner part of the cocoa nut, become finely phosphorescent. Cotton is very bad; dried plants are in general very feebly luminous; vegetable substances bleached are infinitely superior to the same substance not bleached; this is particularly observable in linen, paper, &c. Animal substances, containing carbonate of lime, as egg-shells, shells, corals, &c., are more phosphorescent than those containing phosphate of lime.

After the facts, of which the above are the results, follow various interesting observations: thus it is remarked that the

duration of the light differs very much; the diamond and fluor shine for above an hour, no other fossil for more than a minute. Vividness and duration are in no relation to each other. With the exception of the diamond, the light of fossils is always white, whether they be illuminated by coloured rays or not. Sun-light is more effectual in producing the phenomena than day-light, but too long an exposure is disadvantageous. White bodies act more powerfully than coloured, and light coloured more than dark coloured. Bodies that are shining continue to shine when immersed in water. Difference of temperature has but little effect; ice is phosphorescent, at the same time it is observed, that heat augments the intensity and diminishes the duration of the phosphorescence; cold has a contrary effect. Pure water and transparent fluids do not shine. The light appears to penetrate the substances considerably; for when shining, if a grove be cut a line deep in the substance, it will be as luminous at the bottom of it as at the surface; finally, polishing injures, and in some cases destroys, the phosphorescent power.

M. Heinrich then speaks of artificial phosphori, and describes the process for preparing the Bolognian compound, and those of Canton, Baldwin, &c. Among animal and vegetable substances, many not luminous at first, became so by being cooled or heated; some of these are the flesh of birds; dried tendons, burnt bones and horns, yolk of eggs, toasted cheese, roasted coffee, chestnuts, pease, &c.

M. Heinrich considers these phenomena of phosphorescence to be occasioned by the mere restitution or emission of the light absorbed by the phosphori during exposure to external sources. Among those which are most difficult to explain are the preservation of the light, by enveloping the luminous body perfectly; and its increased emission by the application of heat. If a diamond, the fluor spar of Siberia, or chlorophane, be exposed to light for some minutes, and be then covered with black wax, ink, or any substance, which will perfectly exclude air and light, on removing the envelope, after some days, the body will still be found emitting light. This fact was known to Kircher and Beccaria. The second fact is the following: When fluor spar which has been exposed to light has ceased to become luminous, it may be made to emit light by merely warming it with the breath or the hand. This effect may be obtained many times successively after only one irradiation, especially if, at each time, the warmth be a little increased; at last, warming ceases to produce the effect, but the simple exposure of the spar to the sun enables it anew to present the same appearances as before.—*Bib. Univ.* xv. p. 247.

22. *To the* Editor *of the Journal of Science.*

Sir,

In page xiii of the Introduction to the Dictionary of Che-

mistry lately published, I have alluded to Dr. Henry in terms which have occasioned a private correspondence between that gentlemen and me, the result of which we are desirous of making public in your Journal.

In the beginning of August 1816, I transmitted to him an Essay on Alkalimetry and Acidimetry, accompanied by a letter, in which I begged him to favour me with his opinion of its merits, cautioning him, meanwhile, not to communicate its contents to any person. In the 8th edition of his Elements, which appeared in 1818, he published a plan of alkalimetry and acidimetry modified from that described in my Essay*. This struck me at the time as an unwarranted use of my communication; and declining to correspond with him on the subject, I resolved to seize the first favourable opportunity to reclaim my rights. Under this feeling I wrote the paragraph in the Introduction to the Dictionary.

Dr. Henry thus writes me on the 12th of April 1821, "I assure you that I had not at the time of publishing my book, nor can I now recall, the remembrance of any injunction of secrecy, respecting your alkalimeter; I conceived I had so expressed myself at page 512, vol. ii. of my Elements, as unequivocally to give to you the credit of inventing an instrument on the principle of directly, and without calculation, indicating the *per centage* of alkali in any specimen; and that I pretend to nothing more than the modification of your method which is described in my book."

Under these circumstances, I am satisfied that Dr. Henry had no intention to appropriate to himself the credit of my invention; but I sincerely regret that, before promulgating the modification of my method, he had not consulted me on the subject. This would have prevented all chance of misunderstanding between me and Dr. Henry, whose accomplishments as a gentleman and a chemist, I have been accustomed to admire. The readers of the Dictionary will perceive under the articles Calculi, Coal-gas, Gas, Salt, &c., that I have not suffered temper to influence my judgment, but have done merited honour to the Doctor's researches on every scientific occasion.

I have the honour to be, Sir,
Your most obedient servant,

Glasgow, April 15, 1821. Andrew Ure.

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* "It has been very properly objected to it [the alkalimeter of Descroisilles] by Dr. Ure, of Glasgow, (in an Essay on Alkalimetry, which he was so good, about two years ago, as to communicate to me in manuscript, and which I believe he has not yet published,) that these degrees, being entirely arbitrary, do not denote the value of alkalis in language universally intelligible; and he has proposed an instrument which shall at once, and without calculation, declare the true proportion of alkali in 100 parts of any specimen. The principal deviation in the following rules from the method of Dr Ure, is," &c. &c.

23 *Singular Property of Boracic Acid.*—I mentioned in the 6th vol. of this *Journal*, p. 152 (1819), the property possessed by boracic acid in all states of dilution, of reddening turmeric paper in the manner of an alkali. Since then the attention of M. Desfosses has been drawn to the action of boracic acid on this colouring matter (*Annales de Chimie*, xvi. p. 75.), apparently without a knowledge of the previous remark; and he has shewn that a mixture of boracic, with other acids, reddens turmeric very deeply, and that turmeric, when acted on by this mixture of acids, has its nature altered, for it approaches somewhat to turnsole, and is rendered blue by alkalies.

There is something so curious in this action of boracic acid, on turmeric, that I am tempted to offer a few more results on the subject.

Turmeric paper dipped into a solution of pure boracic acid very speedily receives a slight tint of brownish red, which, when the paper is dry, is very marked, and resembles that produced by a weak alkali. In this state the properties of the colouring matter are entirely different to what they were before: sulphuric, nitric, muriatic, and phosphoric acids, even when very dilute, produce a bright red colour on this paper, and a strong solution of oxalic acid also reddens it. Alkalies on the contrary make it blue, gradually passing to shades of purplish blue, yellowish red, &c. As long as the acids or alkalies remain on the paper, if not so strong as to destroy the colouring matter, the new colour remains, but a slight washing removes them, and then the boracic acid tint returns, and the paper has its first peculiar properties. When altered by muriatic acid, or ammonia, the mere volatilization restores the paper to its first state; with the ammonia the restoration is very ready and perfect; with the acid, it is longer and not so complete. If the paper reddened by boracic acid be heated, the yellow of the turmeric is almost restored, and then it takes from acids a weaker red tinge, and from alkalies a more purplish colour than before.

Turmeric, thus altered by boracic acid, is readily restored to its original state by washing; altered turmeric paper when put in water for two or three hours resumes its original properties, and acts as at first in testing the alkalies.

When the altered paper is placed in sun-light a few days, the colour is soon destroyed as with turmeric alone, and then neither acid nor alkali will affect it.

When turmeric paper is dipped into neutral or slightly alkaline borate of ammonia, it soon becomes of the red tint produced by boracic acid, and is, in every respect, as if altered by boracic acid alone; when this paper is made blue by ammonia, the ammonia easily washes out, and the blue tint disappears, and

afterwards the boracic acid or borates will wash out and leave the paper as at first.

Borax itself at first reddens turmeric paper because of the excess of alkali, but as the colouring matter becomes altered by the presence of the boracic acid, the tint becomes of a dirty blue colour, and then the paper is changed by acids or alkalies, just as if it had been altered by boracic acid.

Hence it is probable that the neutral borates have the same power as the boracic acid, of altering the colouring matter of turmeric, for it is not probable there should be an actual separation of the elements of the salts by it, especially as they both wash out from it and leave it unaltered.

Hence also both acid and alkaline borates redden turmeric.

M. Dessosses, proposes this effect of boracic acid as a test for its presence; for a very small quantity of it mixed with other acid has the power of reddening turmeric paper in consequence of these changes.

M. F.

III. Natural History.

Geology, Medicine, &c.

1. *Further Remarks on the Resemblance between certain Varieties of Granite and of Trap.*—J. Mac Culloch.

In a former number of this *Journal* (Vol. X. p. 29.), I gave a detailed account of some interesting facts occurring in Aberdeenshire, respecting the resemblance of certain portions of the granite of that country to some of the members of the trap family. It was there shown, that, in this district, specimens could be procured from the fundamental granite, and connected with the most common varieties by transition, resembling many of the latest greenstones, and even the basalt of most recent origin which is superincumbent on the latest stratified rocks. The series of specimens formed from these places, is not to be distinguished from a common series of basalt and greenstone; but the interesting conclusions to which this fact gives rise, respecting the similarity of origin in these two families, so far removed in position, need not be repeated, as they were sufficiently pointed out in the paper to which I have alluded.

As the instances which I there quoted may, however, seem to require confirmation, particularly to those geologists who are unwilling to abandon the notions in which they have been educated, it will not be useless to point out another set of similar facts, equally open to investigation, and equally con-

firming the views formerly held out. Setting aside this minor consideration, it is always useful to accumulate examples of any geological fact; particularly of such as, from their novelty, or from their disagreement with former observations, are often, for a considerable time, received with doubt or incredulity. To multiply the places of access to such appearances is also useful; and I can only regret that I have not here to refer to a country more accessible, instead of being, as it is, more remote than Aberdeenshire.

Granite of various characters occurs in different parts of the Shetland islands; where it displays, in a degree of profusion not to be equalled through the whole of Scotland, all these phenomena attending veins, and accompanying its contact with the stratified rocks, which are, deservedly, objects of so much attention to geologists, and which serve to throw so much light on the nature and origin of this substance. But the most entire and extensive tract is found in North Maven, extending over a space which it would be useless to describe in words; as, without a map, no definite idea could be conveyed of it. There are, at least, two very distinct varieties in this district; and, it is not difficult to discover that they are of different eras; since veins of the one variety are found to penetrate into the other, whereas the reverse never takes place.

It is in one of these that the varieties, analogous to those of Aberdeenshire, formerly described, are found; and they present a similar series of graduating specimens; the whole being evidently inferior to gneiss and the other primary strata of that district, and, in many places, graduating into undisputed varieties of the most ordinary granite. To detail the aspects of these specimens, would be merely to repeat that which was said in the former communication on this subject. I shall therefore merely add, that from the ordinary syenitic granite, consisting of hornblende, quartz, and feldspar, with or without mica, a regular series may be traced, passing through numerous modifications of greenstone, not differing from those of the trap family, down to a perfect basalt.

For the information of those who may be inclined to visit the ground in question, I may add that the most convenient situations for examining these appearances in detail, are in the neighbourhood of Hillswick.

2. *On the Deposition of Carbonate of Lime in Wood.*—It is well known that siliceous earth is deposited in many vegetables, particularly in the grasses, in the bark of the *Calamus Rotang*, and in that of *Equisetum hyemale*. The deposit known by the name of *Tabasheer*, is a particularly conspicuous example of this nature. The deposition of carbonate of lime is a more rare occurrence; yet it is found in many pears, and is very re-

markable in the bark or on the surface of *chara vulgaris*. Having accidentally observed one instance of this nature in a very unexpected situation, I thought it deserving of record as adding another illustration of a remarkable fact in vegetable physiology.

A few years ago, when some uneasiness was produced by the rapid consumption of oak in the navy, commissions were sent to various places to procure such woods as appeared to be adapted for ship-building. Among others; many specimens were brought from Sierra Leone in Africa; and, from these, the singular wood under review was selected. Unfortunately, no description of these trees was furnished; so that it is impossible to conjecture to what genus the specimen in question belongs, or whether indeed it belongs to any known genus.

The size of the timber, which is probably still lying in Deptford yard, proves, at least, that it is a large tree. The colour of the wood is that of mahogany, which it much resembles on a general view; being, at the same time, equally hard. But the longitudinal split shows a larger fibrous structure, and, in the transverse smooth section, the grain is coarser, from the large size of the vessels which form the interesting part of this wood.

These vessels, or tubes, are so numerous that they amount to 1,600 in the square inch. Their form is very irregular; seldom round, occasionally oval, but more commonly of a long irregular shape. Sometimes also, two or more ovals are connected by a narrow line. These vacuities in the wood are filled with a yellow carbonate of lime, which bears slight marks of irregular crystallization. But they are not always entirely filled; the wider ones being perforated by a circular tube running through them, and surrounded by the calcareous matter. These orifices are of such a size as just to admit the point of a human hair; it frequently happens that two or more are contained in one of the deposits of the carbonate.

I need scarcely add that the application of an acid excites an active effervescence over the whole section of the wood.

3. *Breaking out of a Spring.*—A remarkable phenomenon occurred at Bishop Monckton, near Ripon, on April 18th, on the estate belonging to Mr. Sharnock. About two o'clock in the afternoon the attention of a person in that gentleman's service was attracted by a rumbling noise which apparently proceeded from the stack yard, distant thirty yards from the house. He supposed it to proceed from children throwing stones against the doors and wall; but on looking up the avenue, formed by a row of stacks, and leading to the house, he observed a small portion of the ground in motion, which, after continuing in a considerable state of agitation for some minutes, suddenly presented an opening of about a foot square, whence issued a great

body of water. Returning with violence it soon enlarged the cavity, and in its progress carried down with it a portion of the surrounding earth several feet in extent, which was buried in the abyss below; the water continued to ebb and flow more or less at intervals during the day. Mr. Charnock plumbed this subterranean pit in the evening, and found it fifty-eight feet in depth; the water has now subsided and remains settled within two yards of the top.—*Gentleman's Magazine*, May, 461.

4. *New Volcano.*—A new volcano has burst out in the highest summit of a ridge of mountains near Leiria, in Portugal. This extraordinary phenomenon occurred at the period of the high rise of the Douro, mentioned in most of the Journals. The volcano was in full action when the latest accounts came away, but had happily taken a direction which threatened to do little damage. The country is steril, and is that through which Wellington passed in pursuit of Massena.—*Gentleman's Magazine*, April.

5. *Hartshorn, its use in Intoxication.*—Dr. Porter, a German physician, states that he has found the spirit of hartshorn (in the dose of a small tea-spoonful in a glass of water,) to counteract the inebriating effects of fermented liquors and spirits.

6. *Scarlet Fever.*—It is announced in the *Journal de Medicina Pratique*, of Berlin, that the Belladonna is a preservative against this fever. The fact was first discovered at Leipsic, but it has lately been confirmed by several experiments.

7. *Iodine, on its application as a Medicine.*—An abstract was given at page 191, vol. x. of this *Journal*, from a paper, by Dr. Coindet, of Switzerland, on the application of iodine to the dissipation of the goitre. In consequence of the importance of any effectual remedy for this disease, in a country where it is so frequent, much attention has been drawn towards Dr. Coindet's discovery, and considerable opposition made to it. It happens also that from the number of cases in which it has been applied, much information, with regard to the general medicinal effects of this substance, has been obtained. These, with other reasons, have induced Dr. Coindet to publish a second paper on the subject, which, as it contains some very interesting matter necessary to be known before the publication of the remedy can be said to be completed, we are induced to abstract at this time, though from the rarity of the disease in this country, it has not that high interest here it possesses in that part of the world.

After having dwelt upon the necessity in every case of using prudence in the administration of a powerful medicine, especially

when that medicine is new, and its action but little understood; Dr. Coindet mentions the circumstance that at Geneva alone 140 ounces of iodine have been sold since he first made known its use in this disease; consequently, that above 1,000 persons have used it; and he remarks that fewer accidents have happened in the application of this quantity than happens in a similar application of almost any powerful medicine.

As the iodine in different states will act differently as a medicine, Dr. Coindet states. that of all the preparations he prefers the iodurettcd hydriodate of potassa. This is prepared by dissolving thirty-six grains of the hydriodate, and ten grains of iodine in one ounce of distilled water; from six to ten drops in half a glass of water, sweetened, is given three times a day, diminishing or increasing the dose according to the effects.

Dr. Coindet prepares the hydriodate of potassa by saturating potassa with hydriodic acid. The acid he prepares previously by passing sulphuretted hydrogen gas through water holding iodine in suspension, or through a solution of iodine in alcohol. The sulphur is then filtered out, and the liquor heated to drive off the free sulphuretted hydrogen. A much simpler mode of preparing the hydriodate would be to saturate a strong solution of potassa with iodine, evaporate to dryness, and fuse the salt out of contact with air in a covered platinum crucible or glass flask, until the portion of iodate formed is decomposed and converted into iodide; the whole is then iodide of potassium, and only requires to be dissolved in water to form the hydriodate of potassa.

Whilst attentively observing the action of this substance on the animal economy, it soon appeared that if given in excess, it seemed to saturate the body, and then produced particular symptoms, which Dr. Coindet calls iodic. This never happens before an effect has taken place on the goitre; and, as the farther addition and action of iodine, beyond the dissipation of the mass is injurious, a stop is immediately put to its administration when these effects appear. After eight or ten days its use is resumed, and continued until the symptoms are again observed, when it is discontinued and again resumed after an interval of time, which is to be more or less according to the state of the patient, and the effect of the medicine on him.

The iodic symptoms when strong are as follows: accelerated pulse, palpitation, frequent dry cough, want of sleep, rapid loss of flesh and strength; with some, there is produced only a swelling of the legs, or tremblings, or a painful hardness of the goitre, sometimes diminished breasts, continued increase of appetite, and in all that Dr. Coindet had seen a very rapid diminution and disappearance of the goitre.

At those times Dr. Coindet forbade iodine and prescribed milk, especially that of asses, warm baths, valerian, kino, car-

bonate of ammonia, preparations of opium, and other antispasmodics. In painful hardness of the goitre; leeches, and emollient fomentations.

The rapid disappearance of the goitre, which accompanies these symptoms, shews them to be occasioned by an excess of iodine: from eight to ten weeks is considered the mean time of proper treatment.

The iodine should not be administered indiscriminately in all cases of goitre: some are inflammatory, and some are accompanied by a bilious disposition of the body; in these cases, leeches should be applied on the goitre, and medicines administered as the case requires, before the iodine be given. If similar symptoms arise during the application of iodine, then those indications should be attended to, and proper medicines given with the iodine.

Iodine should never be employed in cases where the patient is of a gross disposition, or tending to menorrhagia, or in cases where diseases of the breast threaten to, or have commenced, or in slow fevers. It should also be refused to persons who are nervous, delicate, and of a feeble constitution.

Dr. Coindet then states his reasons for believing that iodine may be usefully employed in cases of amenorrhœa, in chronic diseases of the uterus, of indolent tumours of the lymphatic glands of the breast, cases of scrophula without fever, and where the enlarged glands of the neck are indolent; and concludes by expressing a strong wish that no person will resort to this remedy without the advice and observation of a physician.—*Bib. Univ.* xvi. p. 140.

8. *Medical and Physiological Prize Questions.*—1. The Royal Academy of Sciences, at Paris, have proposed the following prize question for 1823: "To determine, by precise experiments, the causes, either chemical or physiological, of animal heat." It is particularly required that the heat emitted by a healthy animal in a given time be ascertained, as well also as the quantity of carbonic acid produced in respiration, and that the heat thus produced, be compared with that occasioned by the formation of as much carbonic acid from the combustion of carbon. The prize will be a gold medal of 3,000 francs value. The memoirs are to be sent in before July 1, 1823.

2. The following prize subject has been announced by the Royal Academy of Sciences, at Paris, for competition during the years 1821 and 1822: "To trace the gradual developement of the aquatic Triton, or Salamander, through its different degrees from the egg to the perfect animal, and to describe the internal changes which it experiences, but principally in regard to osteogany and the distribution of the vessels." The prize is

a gold medal of 300 francs value; to be adjudged in March, 1822. The essays to be sent in by January 1, 1822.

3. The *Société Medicale d'Emulation* proposes the following prize question: the memoirs, written in French or Latin, are to be sent before August 31, 1822, to the *Secrétaire-général*, at Paris. The value of the prize is 500 francs.

"What are the disposition and structure of the system of organs called the nervous ganglions of the organic life, sympathetic nerve, great intercostal, trisplanchnique, &c.?"

"What are the functions of this system of organs?"

"And, as far as is known, what are the diseases in which it is essentially affected?"

4. The Helvetic Society of Natural Sciences have proposed the following prize question for the years 1822 and 1823: To collect exact and well-observed facts, on the increase and diminution of the glaciers in the different parts of the Alps, on the deterioration or amelioration of their pasturages, and on the former and present state of the forests." It will be sufficient if the authors treat only of a determined part of the Alps. Memoirs to be given in before the 1st of January 1823. Prize 300 livres.

5. The Society of Sciences and Arts of Utrecht has announced the following question for competition: "Are there characteristic signs sufficient to distinguish always with certainty, the true cancers from other maladies which resemble it? If so, what are these signs? Ought this malady to be considered as the effect of an indisposition of the whole body, or as only local? If it is to be considered as an indisposition of the whole body, can external remedies, whether amputation, or the remedy applied by the religious of the convent of Rus, or the corrosive remedies, especially arsenic, contribute to the cure or the alleviation of the malady? or ought they to be considered as all equally hurtful? When the malady has not yet the characteristic signs of true cancer, but when there is reason to fear it may become so, and when it may as yet be considered as a local evil, what external remedies may then be applied with sound hope of success? and what are those which should be considered as hurtful?"

6. Another question, by the same body, is as follows: "Can we, by surveying any particular part of the body of an animal that we have not had an opportunity of observing in life, conclude, with certainty, what use it made of that part; so that we may look on this principle of final causes, not only as an useful principle, but as always a sure guide in the natural history of the animal kingdom?"

IV. General Literature.

1. *Origin of Vegetables.*—Turnips and carrots are thought indigenous roots of France; our cauliflowers came from Cyprus; our artichokes from Sicily; lettuce from Cos, a name corrupted into *Gause*; shallots, or eschallots, from Ascalon; the cherry and filbert are from Pontus; the citron from Media; the chestnut from Castana, in Asia Minor; the peach and the walnut from Persia; the plum from Syria; the pomegranate from Cyprus; the quince from Sidon; the olive and fig from Greece, as are the best apples and pears, though also found wild in France, and even here; the apricot is from Armenia.—*New Monthly Magazine*, iii. p. 235.

2. *New Longitude Act.*—By this act the 58th of the late king is amended. The rewards are, 5000*l.* to any subject of Great Britain who shall reach the longitude of 130° from Greenwich within the arctic circle; 15,000*l.* (further) for the north-west passage into the Pacific; 1,000*l.* for 83° of north latitude; and a like sum for 85°, 87°, 88°, and 89° respectively. It is assumed in the preamble, that no ship has gone beyond 81° of north latitude, nor 113° of west longitude.

3. *Roman Mint.*—A considerable quantity of clay moulds, or matrices, for the coining of Roman money, have been lately turned up at Lingwell-Yate, near Wakefield. Thoresby, in his *Ducatus*, mentions a quantity of similar moulds, found at the same place in 1697. Several crucibles, for melting the metal, were found at the same time; and in some of the moulds, there are coins yet remaining. Specimens have been sent by a gentleman at Wakefield, to the Society of Antiquaries, and to the British Museum, in hopes of their decision whether this place was the resort of coiners, or the real mint belonging to the Roman station in its immediate vicinity.—*New Monthly Magazine.*

4. *Ancient Roman Altar.*—A Roman altar was dug up in April, by Mr. S. Faulkner, gardener, in a place called Darvell's field, situate between the Tarvin and Whitchurch roads, in Boughton, near Chester. It is formed of red granite, and is in excellent preservation: its height is nearly four feet; its two fronts, on each of which is the same inscription, are eighteen inches across; and the two sides, quite plain, are about twelve inches each. On the top is a kind of shallow basin, supported by two volutes. The pedestal is a square piece of red sandstone, about twenty inches wide, and six thick, and was found at a small distance from the altar. The inscription is, "*Nymphis et Fontibus Legione Vicesima valente victrici;*" thus Englished: "The twentieth Legion, the power-

ful, the victorious; to the Nymphs and Fountains." There is no particular spring now known very near the spot, where the altar was found; but there are some within five minutes' walk of the place, and the district abounds with them. It is supposed this curious piece of antiquity was thrown down, and buried, at the time the Romans took their last leave of Britain, which was about the year 448 of the Christian era.—*New Monthly Magazine.*

5. *Druidical Sepulchre.*—Ten sepulchral urns were lately found about a foot below the surface on the grounds of Llys D'unfarm, the property of Joseph Huddart, Esq., near the Roman military communication between the tumulus at Llocheddier and that of Dolbarmaon, in Caernarvonshire. The urns occupied a circular space about five yards in diameter, which seemed to have been surrounded by a stone wall. They lay in a straight line, and were filled with bones and ashes; the first containing a small piece of copper. Each urn was protected by four upright stones in a rectangular form, with a flat stone on the top, and a few handfuls of pure gravel underneath. They crumbled into ashes when the ploughman attempted to remove them, and not a fragment above the size of a square inch could be found a few days after the discovery. From there being several druidical remains in the neighbourhood, it is supposed to have been a place of sepulchre consecrated by the Druids. A great part of the sepulchre still remains untouched.—*Monthly Mag. May* 1821.

6. *Literary Notices.*—i. The first volume of Mr. A. P. Thomson's *Lectures on Botany* is almost ready for publication. It will contain the descriptive anatomy and physiology of those organs which are necessary for the growth and preservation of the plant as an individual; and will be illustrated by more than one hundred wood-cuts and ten copper-plates. It is intended to form the first part of a complete system of Elementary Botany.

ii. Next month will be published, *A Treatise of the Principles of Bridges by Suspension*, with reference to the *Catenary*, and exemplified by the Cable Bridge now in progress over the Strait of Menai. In it the properties of the catenary will be fully investigated, and those of arches and piers will be derived from the motion of a projectile. It will contain practical tables, a table of the dimensions of a catenary, and tables o the principal chain, rope, stone, wood, and iron bridges; with the dimensions of them erected in different countries.

iii. Mr. Gideon Mantell, F. L. S. is about publishing in one volume, royal quarto, (illustrated by numerous engravings), the *Fossils of the South Downs; or, Outlines of the Geology of the South-eastern Division of Sussex.*

ART XVIII METEOROLOGICAL DIARY for the Months of March April and May 1821, kept at EARL-SPENCER's Seat at Althorp, in Northamptonshire The Thermometer is hanging in a North-eastern Aspect, about five feet from the ground, and a foot from the wall

For March, 1821

		Thermometer		Barometer		Wind	
		Low	High	Morn	Eve	Morn	Eve
Thursday	1	37	41	29 19	9 8	SW	[illegible]
Friday	2	31	45	29 18	29 16	W	[illegible]
Saturday	3	41	51	29 [illegible]	29 [illegible]	[illegible]	[illegible]
Sunday	4	32	44	29 [illegible]	29 [illegible]	[illegible]	[illegible]
Monday	5	28	43	29 [illegible]	29 30	[illegible]	[illegible]
Tuesday	6	28	42	29 [illegible]	29 30	[illegible]	[illegible]
Wednesday	7	28	42	29 [illegible]	29 [illegible]	[illegible]	[illegible]
Thursday	8	28	42	29 [illegible]	29 [illegible]	[illegible]	[illegible]
Friday	9	28	42	29 [illegible]	29 [illegible]	[illegible]	[illegible]
Saturday	10	28	42	29 [illegible]	29 [illegible]	[illegible]	[illegible]
Sunday	11	28	42	29 [illegible]	29 [illegible]	[illegible]	[illegible]
Monday	12	28	42	29 [illegible]	29 74	[illegible]	[illegible]
Tuesday	13	28	42	29 [illegible]	29 [illegible]	[illegible]	[illegible]
Wednesday	14	28	42	29 [illegible]	30 02	[illegible]	[illegible]
Thursday	15	28	42	30 [illegible]	30 10	[illegible]	[illegible]
Friday	16	28	42	30 [illegible]	30 [illegible]	[illegible]	[illegible]
Saturday	17	28	42	30 [illegible]	29 [illegible]	[illegible]	[illegible]
Sunday	18	28	42	29 [illegible]	29 [illegible]	[illegible]	[illegible]
Monday	19	28	42	29 [illegible]	29 [illegible]	[illegible]	[illegible]
Tuesday	20	28	42	29 [illegible]	29 [illegible]	[illegible]	[illegible]
Wednesday	21	28	42	29 [illegible]	29 [illegible]	[illegible]	[illegible]
Thursday	22	28	42	29 [illegible]	29 [illegible]	[illegible]	[illegible]
Friday	23	28	42	29 [illegible]	29 [illegible]	[illegible]	[illegible]
Saturday	24	28	42	29 [illegible]	29 41	[illegible]	[illegible]
Sunday	25	28	42	29 [illegible]	29 32	[illegible]	[illegible]
Monday	26	28	42	29 [illegible]	29 [illegible]	[illegible]	[illegible]
Tuesday	27	28	42	29 [illegible]	29 [illegible]	[illegible]	[illegible]
Wednesday	28	28	42	29 [illegible]	29 [illegible]	[illegible]	[illegible]
Thursday	29	28	42	29 [illegible]	29 [illegible]	[illegible]	[illegible]
Friday	30	28	42	29 [illegible]	29 [illegible]	[illegible]	[illegible]
Saturday	31	28	42	29 [illegible]	29 [illegible]	[illegible]	[illegible]

For April 1821

		Thermometer		Barometer		Wind	
		Low	High	Morn	Eve	Morn	Eve
[illegible]	1	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	2	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	3	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	4	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
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[illegible]	7	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
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For May, 1821

		Thermometer		Barometer		Wind	
		Low	High	Morn	Eve	Morn	Eve
[illegible]	1	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	2	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	3	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
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[illegible]	31	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

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