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OF

THE QUARTERLY JOURNAL,

N°. XIX.

TO CORRESPONDENTS.

The Editor regrets that Mr. G. Mansel's Paper reached him too late for publication in this Number.

We presume that our Correspondent J. F. D. allows his communication to stand over till next Number.

The Letter, signed V., has been received.

The Editor begs to apologize to Mr. Watts for the term "illiberal," used in the Index of Volume VIII. of this Journal: it escaped his observation till Mr. W. pointed it out.

An additional Plate will be given in No. XX.

The Chemical Lectures and Demonstrations in the Laboratory of the Royal Institution commence on Tuesday, the 10th of October, at nine in the morning: for particulars, see page 215 of this Number.

A new edition of Mr. Brande's Manual of Chemistry, considerably enlarged, in 3 volumes, 8vo. is in the press, and will be ready early in the Spring.

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OF

THE QUARTERLY JOURNAL,

N°. XX.

TO CORRESPONDENTS.

Captain VETCH's Paper on the remains of the Mammoth; and a communication on the Solar Eclipse of the 7th of September last, will appear in our next Number, with appropriate Engravings.

Z. has reached its destination. December 23.

ROYAL INSTITUTION, December 1, 1820.

The Members and Subscribers are informed, that the Lectures will commence on Saturday the 3d of February next at two o'clock, and that the following arrangements have been made:

On the Elements of Chemical Science, embracing the subjects of Attraction, Heat, and Electricity.

On Geology, and its connexion with Agriculture and Mineralogy.

By W. T. BRANDE, Esq., Sec. R. S. London, and F. R. S. Edinburgh, Professor of Chemistry, Royal Institution.

On the Application of Natural Philosophy to the useful Purposes of Life. Illustrated by appropriate apparatus.

By JOHN MILLINGTON, Esq., Civil Engineer, Professor of Mechanics in the Royal Institution.

This Course will commence on Wednesday the 7th of February.

On Music—By WILLIAM CROTCH, M.D., Professor of Music in the University of Oxford.

THOMAS HARRISON, Secretary.

Mr. BRANDE will commence the Second Course of the Lectures and Demonstrations in Chemistry, delivered in the Laboratory of the Royal Institution, on the first Tuesday in February, punctually at nine in the morning. This Course will embrace the Chemistry of the Metals, and of Animal and Vegetable Products, and the subject of Geology.

THE

QUARTERLY JOURNAL,

October, 1820.

ART. I.—*On the Inscription on the Column at Alexandria, in a Letter to the Editor, by the* EARL of MOUNTNORRIS.

Arley-Hall, June 21, 1820.

MY DEAR SIR,

SO much has been written respecting the inscription on the column at Alexandria, formerly called the Pillar of Pompey, that I think the accompanying drawing of the inscription made by Mr. Salt last year, and forwarded by him to me, will be interesting to many of your readers. The correctness of Mr. Salt's pencil is so well known that his drawing must put an end to all disputes as to the actual inscription on this celebrated column; but it may not be uninteresting to trace the progress made in ascertaining it; I have therefore sent you copies of the inscription as given by Pococke, Colonels Leake and Squire, and Dr. Clarke.

To Pococke is certainly due the merit of having first attempted to decipher this inscription; that he has in a great degree failed is excusable when it is considered under what difficulties every Christian then laboured who attempted to examine the antiquities of Egypt. Had he been able to dedicate more time to the work, he would have, probably, been more successful; as in one short visit he correctly ascertained the two first letters of the third line and the three first letters of the fourth line.

It is extraordinary that the French savans should not have made out any part of this inscription during their long residence at Alexandria, when they ought to have known that Pococke had read a part of it, and that other travellers had mentioned its being legible. That they totally failed in doing so is evident from the concluding paragraph of the article on this column, by Monsieur Norry, given in the first volume of the *Mémoires sur l'Egypte*, who says, "On doit beaucoup regretter qu'une inscription qui étoit sur l'une des faces du piédestal ne soit plus lisible; on seroit éclairé sur ce monument, que les auteurs attribuent, les uns à la mémoire de Pompée, d'autres à celle de Septime Sévère."

An attempt was afterwards made to give to Monsieur Jaubert the credit of having made out the inscription, and to Monsieur Villoison of having first explained it; but there can be no doubt that the former obtained a copy of the inscription as taken by Messrs. Leake, Squire, and Hamilton, in 1802, and which had been widely circulated by them, and this is strongly corroborated by Monsieur Chateaubriand, who, in giving the inscription, says, "Je crois être le *premier* voyageur qui l'ait rapportée en France," and adds, "Le monde savant la doit à quelques officiers Anglais."

These officers were Colonels Leake and Squire, who, in September 1801, ascertained that the inscription was still in part legible. The deciphering of it was however delayed till the March following, by the absence of Colonel Leake, who accompanied Mr. Hamilton to Upper Egypt, but it was actively undertaken on their return, and the result was the ascertaining the whole of the inscription excepting three words.

In February 1803, Colonels Leake and Squire communicated their discovery to the Society of Antiquaries, and in their letter they give full credit to Mr. Hamilton as a fellow-labourer. An unsuccessful attempt has, however, since been made to deprive Messrs. Leake and Hamilton of all share in the thanks due to the discoverers by the literary world. Two letters have been published by Dr. Clarke, written by Colonel Squire to his brother, and from which the doctor infers that "all idea of attempting the discovery is due to Colonel Squire, and that he had the greatest share in its execution*."

* Clarke's *Travels*, Sec. II. Part II. page 257.

The first letter, as given by Dr. Clarke, is without a date, but evidently written later than February 1803; it is as follows: "I believe the paper presented to the Antiquary Society, contains the best history of the discovery of the Alexandrian inscription. I wish not to be brought forward in any literary dispute, but the fact is, that most of the letters were discovered by me while Messrs. Hamilton and Leake were in Upper Egypt. I had seen the *same* inscription in Pococke's Travels before I knew of its existence from that book." The second letter is dated Alexandria, Christmas-day, 1801. "Here let me remark that it is not *impossible* but that part of the inscription on the great pillar may yet be read. Π and O are legible enough, and by other remains of characters, I can plainly perceive that the inscription consisted of four lines in Greek. With sulphur, an impression of these characters might be taken, and *perhaps* something satisfactory discovered. Before we quit the country, I will certainly endeavour to make the experiment *."

The conclusions drawn from these letters by Dr. Clarke, imply that Messrs. Leake and Hamilton have assumed to themselves a share in a discovery to which they had no right: this is a very serious charge, and which does not appear to me to be sustained by the letters themselves, even admitting the statements in them to be perfectly correct. Colonel Squire does indeed claim for himself the having discovered most of the letters during the absence of Messrs. Leake and Hamilton, in Upper Egypt; but this could not be called a discovery, for the same had long before been done by Pococke, and nothing can be more different than the ascertaining that detached letters are distinguishable, and the deciphering a sufficiency of the inscription to show its sense, and to whom the column was dedicated. Indeed, the only two letters Colonel Squire claims as having made out, were Π and O, and these had already been given by Pococke, who had also given the following or third letter;

* On this Dr. Clarke observes, "that even the device of the sulphur was due to him." There was but little merit in this device; it was tried, and a third part of the inscription was taken off; but without the discovery of a single additional letter. It was, in fact, totally useless.

so that in fact, on the arrival of Messrs. Leake and Hamilton at Alexandria, in March 1802, Colonel Squire had *less* knowledge of the inscription than he might have obtained from Pococke, whose work he, probably, had not with him. The expressions used by him in his letter of Christmas-day 1801, when Messrs. Leake and Hamilton had been absent three months, show clearly how little had then been done. He then considered it "as *not impossible* that part of the inscription *might* be read, and that, *perhaps*, something satisfactory might be discovered;" and he had then "determined to endeavour to make the experiment." That he did any thing in the following two months of January and February is no where asserted by him, but, on the contrary, he declares in the letter published under his signature and that of Colonel Leake, that the first discovery of any *word* was made in March 1802, and in the same letter he admits that Messrs. Leake and Hamilton had an equal share in the meritorious and successful endeavour to decipher the inscription.

I should have been sorry that so respectable an officer as Colonel Squire should have asserted any thing in a private letter which was at variance with what he had published under his hand, but this he has not done. He has not claimed for himself the first idea of deciphering the inscription, or that he had the greatest share in the execution. Had he done so, I should still have given credit to the positive assertions of Messrs. Leake * and Hamilton †, that they had a full share in deciphering the inscription. I cannot therefore agree with Dr. Clarke "that all the information afforded by the inscription would have been consigned to everlasting oblivion but for the

* *Classical Journal*, No. XXV., page 152, and No. XXIX., page 161.

† *Ægyptiaca*, page 403. In this place Mr. Hamilton expressly asserts that he was a fellow-labourer in the deciphering of the inscription, and adds that, "after visiting it for several days successively at the most favourable hour, when the rays of the sun first struck obliquely on the plane of the letters, we obtained the following lines:" This passage Dr. Clarke has quoted as his authority for asserting "that Mr. Hamilton arrived in Alexandria, as it *has been related by him, after the inscription had been found, and the undertaking for copying it had been begun.*"

important discovery made by the late Lieutenant-Colonel Squire, of some remaining characters upon the pedestal, while Mr. Hamilton, and his companion Major Leake, were in Upper Egypt." On the contrary, I believe that if these gentlemen had never returned to Alexandria, Colonel Squire would have done as little after March 1802, as he had done in the six months preceding.

During my residence at Alexandria, in the spring of 1806, Mr. Salt dedicated many days to the deciphering and drawing of the inscription. This drawing was unfortunately lost after my return to England, but from memory I was enabled to state * that the pillar was unquestionably dedicated to Diocletian, and that the three first letters of the name of the prefect were ΠΟϹ, as had been originally stated by Pococke. On the appointment of Mr. Salt to fill the station of His Britannic Majesty's Consul-General in Egypt, I most particularly requested him to re-copy the inscription. I have now the pleasure of forwarding to you the result of his labour, by which are ascertained the three words left undeciphered by Messrs. Leake, Squire, and Hamilton, *viz.*, ΤΙΜΙѠΤΑΤΟΝ ΑΝΙΚΗΤΟΝ, and the name of the Prefect ΠΟϹΙΔΙΟϹ.

Colonel Leake conjectured the first word to be ΤΙΜΙѠΤΑΤΟΝ, and the deciphering of the ΜΙ before the Ѡ has proved it to b so. Jaubert had asserted it was ΟϹΙѠΤΑΤΟΝ. Chateaubriand had suggested ϹΟΦѠΤΑΤΟΝ.

The second word had been ascertained by Mr. Salt in 1806, and it had been communicated to Colonel Leake and others, yet many years afterwards Chateaubriand had recommended ΑΥΓΟΥϹΤΟΝ; and Dr. Clarke, ϹΕΒΑϹΤΟΝ. The second investigation of Mr. Salt has confirmed his first.

The third word may be considered as the most important, as it ascertains the name of the Prefect who dedicated the pillar to be ΠΟϹΙΔΙΟϹ, and not ΠΟΜΠΗΙΟϹ, as conjectured by Dr. Raine; ΠΟϹΤΟΜΟϹ, as conjectured by Dr. Clarke; or ΠΟΛΛΙѠΝ, as conjectured by Chateaubriand.

Dr. Clarke has attempted to establish, that the beginning of

* Valentia's *Travels*, Vol. III. page 464.—London, 1809.

the third line of the inscription ought to be read **ΔΙΟΝΑΔΡΙΑΝΟΝ**; but, as he admits that he did not decipher a single letter of the inscription, his conjecture cannot be placed against the positive testimony of Messrs. Leake, Squire, and Hamilton, who first ascertained the name to be **ΔΙΟΚΛΗΤΙΑΝΟΝ**; and of Messrs. Jaubert, Salt, and Chateaubriand, who equally assert that they distinguished clearly this name, to which, if necessary, I could also myself testify.

No doubt can now remain that the inscription must be read as follows:

ΤΟΝ ΤΙΜΙΩΤΑΤΟΝ ΑΥΤΟΚΡΑΤΟΡΑ
ΤΟΝ ΠΟΛΙΟΥΧΟΝ ΑΛΕΞΑΝΔΡΕΙΑΣ
ΔΙΟΚΛΗΤΙΑΝΟΝ ΤΟΝ ΑΝΙΚΗΤΟΝ
ΠΟΣΙΔΙΟΣ ΕΠΑΡΧΟΣ ΑΙΓΥΠΤΟΥ

[" Posidius, prefect of Egypt (has erected) the most honoured emperor, the guardian deity of Alexandria, Diocletian the Invincible."]

I do not mean to assert that there was no fifth line, but I certainly could not distinguish any vestiges of it; and Mr. Salt's drawing shows that, on his second examination, he was of the same opinion.

I cannot but wish that the name of the Prefect had been Pompey, as it would have accounted for the name by which this celebrated pillar has been latterly called. Sandys says, that " it was called by the Arabians Hemadeslaer, but by the Western Christians the Pillar of Pompey." With these foreigners, therefore. the modern name originated, and not with the natives. Tradition could have no weight against positive testimony, were there any to support its former name. It is now proved that the column was dedicated to Diocletian, and it will, probably, in future. be called by his name.

Believe me to be,
My dear Sir,
Yours, ever faithfully,
MOUNTNORRIS.

To
W. T. Brande, Esq.
Royal Institution.

The inscription on the column at Alexandria, copied by Pococke:—

ꓕϽ .. 7....ΟCΟΤΑΤΟΙΡ . Ο . Ρ : ΤΛ
ΤCC..ΟCΟΝΙΟΥ.. ΤΟΝΛΛΕΛΛΔ
ΔΙC ΜΑΡΡΟΛΠΟΝΤΟΝΛΛΙ..
ΠΟCΕ.......ΛΡΛCC......

The inscription as given by Dr. Clarke:—

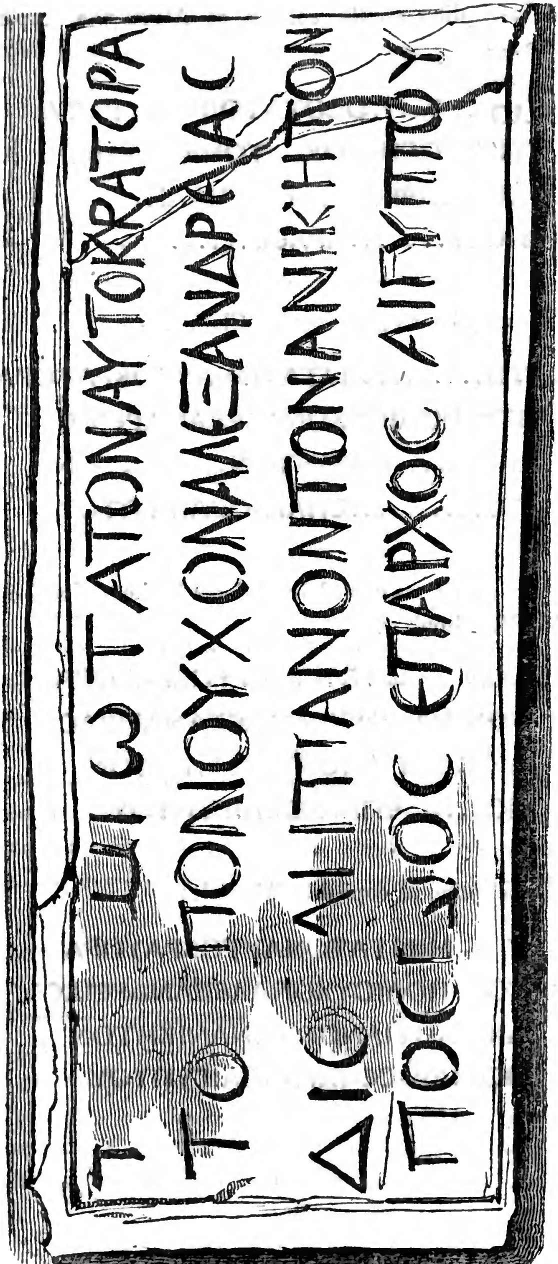
ΤΟ..........ѠΤΑΤΟΝΑΥΤΟΚΡΑΤΟΡΑ
ΤΟΝΠΟΛΙΟΥΧΟΝΑΛЄΞΑΝΔΡЄΙΑC
ΔΙΟ......ΙΑΝΟΝΤΟΝ........ΤΟΝ
ΠΟ..........ЄΠΑΡΧΟCΑΙΓΥΠΤΟΥ

The inscription as copied by Colonel Leake, Colonel Squire, and Mr. Hamilton:—

ΤΟΝ ѠΤΑΤΟΝΑΥΤΟΚΡΑΤΟΡΑ
ΤΟΝΠΟΛΙΟΥΧΟΝΑΛЄΞΑΝΔΡЄΙΑC
ΔΙΟΚΛΙ·ΙΤΙΑΝΟΝΤΟΝΑ....ΤΟΝ
ΠΟ......ЄΠΑΡΧΟCΑΙΓΥΠΤΟΥ

The inscription as copied by Mr. Salt:—

Τ ΜΙѠΤΑΤΟΝΑΥΤΟΚΡΑΤΟΡΑ
ΤΟ ΠΟΛΙΟΥΧΟΝΑΛЄΞΑΝΔΡЄΙΑC
ΔΙΟ ΛΗΤΙΑΝΟΝΤΟΝΑΝΙΚΗΤΟΝ
ΠΟCΙΔΙΟCЄΠΑΡΧΟCΑΙΓΥΠΤΟΥ


ΑΤΟΝΑΥΤΟΚΡΑΤΟΡΑ
ΤΟ ΠΟΛΙΟΥΧΟΝΑΛΕΞΑΝΔΡΙΑC
ΤΙΑΝΟΝΤΟΝΑΝΙΚΗΤΟΝ
ΠΟC ΕΠΑΡΧΟC ΑΙΓΥΠΤΟΥ

Art. II.—*On the apparent Changes of Place, Colour, Size, and Figure of the Heavenly Bodies.—By* G. W. Jordan, Esq., A.M. F.R.S.

All the celestial luminaries are observed to be subjected to apparent changes of place, beginning at points below the zenith, where no change is, and increasing at stations descending towards the horizon. Of the sun and moon at considerable elevations, the light is more or less white, but at lower altitudes, these luminaries become tinged with dilute tawny colours, and the sun, when so low that it may be viewed with the naked eye, becomes of a brilliant yellow, which changes to a brilliant red, of decaying brightness until the orb sinks below the horizon. At equal altitudes, the moon exhibits similar but fainter colours. As the sun declines, not only these changes occur of place, of colour, and of quantity of light, but the disc itself also undergoes considerable apparent changes, primarily of dimensions, and subsequently of figure. Primarily a general enlargement of disc appears, subsequently a contraction of vertical diameter, accompanied with a more considerable extension of horizontal diameter, and a change of upper and lower limbs from circles to ellipses, the lower being considerably more eccentric than the upper. In this state of approach to the horizon, the lower limb occasionally suffers considerable fluctuations, and variations of outline, contracting and enlarging in different points at the same time.

These changes of place, colour, brightness, dimensions, and figure, have been variously accounted for. As they occur at the same stations, and under the same circumstances, they are unquestionably to be referred to the same causes. Each however has been considered apart from the other, with other views than that of ascertaining the true causes of each, or of all. The change of place has been considered by astronomers with a view to ascertain its amount at a given station, and from this to estimate its variation and amount at other stations.

The change of dimensions and of figure, particularly on the verge of the horizon, has exercised the ingenuity of the greatest philosophers, and still remains *vexata questio.* The change of colours has only been occasionally remarked upon. All these changes will however be found concurrent and illustrative of each other; whilst the various attempts to account for them singly, exhibit such a misapplication of principles, and want of information respecting the ordinary phenomena of light, as will only surprise him who knows not how little is at present generally and correctly understood of light, in its causes and principles of existence.

Astronomers, who first noticed the changes of place in the heavenly bodies, have variously attempted to account for their existence, and to estimate their amount. They consider the atmosphere as consisting of innumerable concentric laminæ, of various densities, and of refractive powers increasing in the approach to the surface of the earth,—that the rays of light, in passing thruogh the atmosphere thus constituted, are subjected to refractions, such as occur at the confines of different refractive media, by which they are similarly bent, in various directions towards the perpendiculars to these laminæ, and that changes and elevations of the apparent places of objects are in this manner produced. At the confines of different adjacent refractive media, there is always a division of the light into two portions reflected and refracted, each of them containing a given portion of the incident light. By calculations founded upon the continuance of twilight, after the sun has sunk below the horizon, it has been estimated that at the distance of from 40 to 75 miles from the surface of the earth, there are powers in the atmosphere, capable of producing the twilight; and at these elevations, these concentric laminæ are referred to, as reflecting from their supposed lower surfaces, light adequate to the effect produced.

This estimate, however, of the height of the atmosphere on which twilight depends, is in itself incorrect, as being founded on the false principle that it depends on single reflections at the preceding altitudes of the light of the sun sunk below the horizon, whereas it is produced by many and repeated reflections of the vapours floating in the atmosphere, and at last reaches the eye of the spectator by a course so indirect, as to leave no power of estimating the distances from the earth of the reflecting particles.

Of these floating particles, upon which twilight and other atmospheric phænomena depend, the altitudes thus assumed exceed all reasonable estimate. All observations respecting the constitution of the air, show twilight to be the product of the whole atmosphere, and its contents near the surface of the earth. The tops of the highest mountains, five miles above the level of the sea, are scarcely attained and surmounted by any vapours, by any reflecting particles, capable of producing sensible effects from their sizes or numbers. The supposed height of the atmosphere capable of reflecting the height of twilight, being 50 miles according to Keill, whose estimate is a mean between others, the lengths of air traversed by light in the horizon is 12 times greater than in the zenith. (*Vide Keill's Astronomy*, p. 244.) This gives the enormous tract of 600 miles for the passage of the horizontal light of the luminaries. Through an atmosphere of 75 miles, 50 miles, or 40 miles height, divided into various reflecting and refracting laminæ, as alleged, not the light of twilight, not the light of the meridian sun would reach the surface.

Euler supposed a single bending of the rays at the confines of æther and atmospheric air, such as occurs between any other two transparent refractive media. This hypothesis escapes the preceding objection, but is subjected to the difficulties of an hypothesis which would establish confines between air and æther, similar to those between other transparent media of different refractive powers, together with all the other objections to atmospheric refractions.

The atmosphere is not a laminated but a continuous body.

In the passage of light through a transparent continuous medium, there is neither reflection nor refraction by the medium; and therefore there are in fact no atmospheric refractions, and some other mode of accounting for the phænomena must be resorted to.

The atmosphere is a transparent continuous body: nor will the division into ideal laminæ, by imagining surfaces, produce effects which depend upon a distinct separation of parts, into distinct surfaces, severally belonging to the several media at their confines. Of transparent continuous bodies, the reflections and refractions are only made at their confines, where they are adjacent to other transparent continuous bodies of different refractive powers, at which confines exists that state of discontinuity and distance, upon which their reflections and refractions depend.

The difference of refractive powers of any two adjacent portions of the atmosphere, if any difference can be supposed, if any formation of laminæ can be admitted, if any state, similar to that of the confines of two different media of different refractive powers, can be imagined to exist, at every mental or ideal division of the continuous fluid of air—the difference, it may be observed, of the refractive powers of these portions of the air where they join, cannot but be less than that of crown glass and flint glass. The solid bodies of crown glass and flint glass, pressed together by powers considerably less than the ordinary weights of the atmosphere, may be made so closely to approach together, that all reflection and refraction will be extinguished at their confines, and light pass through both, as uninterruptedly as through one continuous body. The parts of the air are of one continuous body, and if they were not, are yet so powerfully pressed together that they would be, for the passage of light, as parts of one continuous body.

When a portion of light, in a continuous transparent medium, passes by the side or edge of any other body in that medium, it is inflected or bent towards that body, in angles proportioned to the distances of the parts of the light from the side or edge. These bendings have been observed by philosophers, and by them named inflections. The principal phænomena of inflections, more than thirty years ago, I observed and explained, correcting some very important errors of great authority, disposing of the supposed anomaly of the repulsion of light by bodies, and of that repulsion being changed at other distances into attraction, by pointing out the bodies whose attractions had been mistaken for the repulsions of other bodies, and thus referring all the appearances to the single and simple principle of attraction. These inflections exist in the same continuous medium, refractions never but at the discontinued confines of different media. These inflections, in the continuous medium of air, produce all the preceding observed changes of the heavenly bodies.

In imperfectly transparent media, as opals, rubies, coloured glasses and tinctures, various particles are diffused throughout the bodies, which by their inflections change the directions and colours of the light at varying thicknesses. Such a continuous medium is the atmosphere; transparent in itself, imperfectly transparent in consequence of the floatage of various particles of other bodies throughout and between its parts.

At the confines of any two adjacent transparent bodies, there is a discontinuity, and separation to definite intervals, of the particles of both, dependent upon the relative attractions of the particles of each body, for themselves, and for the particles of the other body; which attractions, although they prevent such an intimate union of the bodies as would end the reflective and refractive powers of both, does not altogether cease to exist between the neighbouring particles of each for the other; and there is one class of transparent continuous bodies, between whose particles there exist in given lines of direction, intervals of aggregation similar to the intervals between different media, which although the bodies cease not to be continuous aggregates, yet, at these intervals, a division of the light takes place, similar to that between different transparent bodies, and produces a double refraction within the bodies, in lines duly related to these and other lines of aggregation. This original conjecture respecting the causes of the double refractions of certain crystals, was happily confirmed by the splendid discoveries of Malus, who observed that a polarization also takes place in the light divided at the confines of different transparent bodies, similar to that produced in the light divided within doubly refracting crystals. Thus from similarity of effects is established the suggested similarity of causes in both, of causes existent, and derived from the phænomena.

These supposed causes of the changes of place in the heavenly bodies being thus disposed of, the modes of estimating or calculating their amount may be next considered. Together with these unsupported hypotheses of causes, may at the same time be disposed of, all methods of calculation founded exclusively on the existence and operation of these causes.

Professor Vince, in his *Complete System of Astronomy*, has detailed at large the principles adopted to account for these apparent changes of place, and the different methods invented for estimating their amount, and that of the occasional variations observed in these changes themselves.

" When a ray of light passes out of a *vacuum* into any *medium*, or out of any medium into *one* of *greater density*, it is found to deviate from its rectilinear course towards a perpendicular to the surface of the medium into which it enters. Hence light passing out of a vacuum into the atmosphere will, where it enters, be bent towards a radius drawn to the earth's centre, the top of the atmosphere being supposed to be spherical and concentric with the centre of the earth; and as in approaching the earth's surface, the density of the atmosphere continually increases, the rays of light as they descend are constantly entering into a denser medium, and therefore the course of the rays will continually deviate from a right line and describe a curve; hence at the surface of the earth the rays of light enter the eye of the spectator in a different direction from what they would have entered, if there had been no atmosphere; consequently, the apparent place of the body from which the light comes must be different from the true place. Also the refracted ray must move in a plane perpendicular to the surface of the earth; for conceiving a ray to come in that plane before it is refracted, then the attraction being always towards the perpendicular which lies in that plane, the ray must continue to move in that plane. Hence the refraction is always in a vertical circle."

These are the accepted doctrines of the day, and delivered as such by the Professor.

According to these statements, " when the light passes (obliquely it should have been stated), out of any medium into one of greater density, it is found to deviate." To produce this deviation however, the circumstances stated are not sufficient. The light must pass not only out of " any medium into one of greater density," but out of one medium *into another—into another and not the same*—not the same even *increased in density*—into another *distinct and separate* medium, not merely of *greater density* but of *greater refractive power.* Refractive power is not identical with density. The refractive power of glass for instance, is to that of water as 55 to 34, its density as 87 to 34. Change of density alone, and not of medium, and of refractive power will not produce the reflection, refraction, dispersion, or change of direction of light. Thus the principles assumed to account for the apparent changes of place in the heavenly bodies fail, together with all the observations dependent upon and connected with them.

A ray of light always moves after refraction in the plane of incidence, whether it falls on a plane or curved surface. That refracted rays therefore may move in vertical circles only, they must come in vertical planes only. But rays of light do not necessarily move either before or after their supposed refractions in vertical planes, and their bendings therefore are not always necessarily in vertical circles. If this were the case, no changes of the diameters, parallel to the horizon, of sun or moon could ever take place, and all measurements thereof to discover any, would be useless. It is however to be conjectured and feared, that this opinion, of the supposed refractions necessarily being in vertical circles, has influenced observation, has led to erroneous conclusions respecting the phænomena, and affecting even the judgment of observers in their measurements and estimates, has contributed to establish and continue error.

Such are the defects of the doctrines of the astronomical schools on this subject. They are not to be imputed to the Plumian professor, but belong to the system, which he delivers as received. His esteemed work was among my books, and in seeking to give an account of atmospheric refractions, as they are called, my opinion of him led me to that. I should have found the same things in any and every treatise of astronomy to which I could have referred.

The different methods of ascertaining the amount of these changes of place, determining it for one altitude and object by direct observation, and estimating it for other altitudes, objects, and places, do honour to the talents of astronomers in every age. Tycho, Cassini, De la Caille, Newton, Bradley, Hawksbee, Maskelyne, and others, calculated, but possessed no correct conceptions of the causes of the phænomena.

These changes of place having been found to vary in themselves, not only at different altitudes of the luminaries, but at the same elevations also, in different states of the atmosphere, and changes in the barometer and thermometer having been observed to be cotemporaneous with those; these, although themselves depended upon other changes, have been considered as connected with and influencing those, and have been taken into the account in which those were estimated and calculated. There are, however, conditions and changes of the atmosphere, dependent upon other causes than its temperature and weight, which are indicated by the hygrometer, and upon which the phænomena themselves more immediately depend. These, therefore, rather than those, or perhaps together with those, are to be considered; and thus it appears how uncertain all knowledge is, not founded upon a knowledge of causes. *Rectè scire est per causas scire.*—BACON.

The apparent changes of place in the luminaries, in their lowest stations near the horizon, are indeed subjected to anomalies, which render confessedly, the calculations applied to the higher, not to be depended upon in these last stations.

Whether in calculations, which require these apparent variations of change of place to be taken into the account, and therefore to be correctly determined, any estimate can or ought to be depended upon, except derived from observations made at the time, and upon the occasion, during the existing state of the atmosphere, and independently of the principal observations, will hereafter appear. Many modes of making these observations will occur to practical astronomers, to whom we defer with all due respect in matters purely astronomical.

The apparent and extraordinary changes of dimensions, and of figure, in the sun and moon in stations just above the horizon, have been referred by various conjectures to various causes.

Des Cartes, Wallis, and others, suppose that a better judgment being formed of the distance of the moon by comparison with objects in the horizon, she is considered as more remote, and therefore appears larger in the horizon than in the zenith.

Ptolemy, considered the effect, as in part a fancied, in part an actual enlargement of apparent disc, and conformably to this latter opinion Roger Bacon ascribes the enlargement to refraction. Gassendus ascribes the appearances to an increased dilatation and flatness of the pupil of the eye in less light producing a larger picture on the retina; Berkeley to the diminished horizontal light of the moon; Smith to the apparent figure of the sky as being less than an hemisphere, the moon retaining her size unchanged, and appearing, upon the principles of perspective, larger at supposed remoter distances.

The horizontal diameters of sun and moon have been subjected to actual measurements with varying conclusions. Riccioli affirms, that together with Grimaldo, having, with a sextant, carefully and repeatedly measured the horizontal diameter of the sun, one by the right, the other by the left limb, they distinctly ascertained the increase to be what the naked eye exhibits. Almost all other philosophers have considered the appearances to be delusive, to be optical deceptions, and have affirmed that measurements by instruments give no increase of dimensions. Almost all are of opinion that the refractions, which, as they suppose, produce these appearances, can only be in vertical circles.

Riccioli and Grimaldo state, the observed diameter of the sun in the horizon to be from 45 to 60 minutes, of the moon from 38 to 40 minutes.

Molyneux objects that the moon ought to have appeared under an angle of 5 degrees. Did any person ever see the moon extended under an angle of 5 degrees in the horizon ten times larger than usual? This argument then fails in fact, as does also the other, that there is no refraction or change of place but the vertical, and therefore no dilatation.

I agree with Riccioli and Grimaldo; because their measurements accord with the theory hereafter to be developed; because they agree in their amount with the appearances, vertical as well as horizontal; and because of their very differences; for of the sun and moon, the mean apparent diameters are nearly equal, and severally about 32′ and 31′, and according to their observations, the measured diameters are from 45′ to 60′ of the sun in the horizon, from 38′ to 40′ of the moon, the weaker marginal light of the moon being extinguished, and her size more reduced by the atmosphere through which she appears, than that of the sun.

The size of a candle, viewed through different deeply-coloured glasses, is considerably diminished by the loss of its fainter marginal light; and so the sun and moon ought, on account of the loss of light in passing through the atmosphere, to appear, and would appear, diminished in diameter, but that this cause of diminution is more than compensated for, and the discs are more enlarged by, the lateral inflection of the rays than diminished by the extinction of the marginal light. By the increased extinction, however, of the weaker marginal light of the moon, her apparent size is more reduced than that of the brighter and more strongly illuminated sun, the atmosphere, through which they both are seen, acting as a coloured glass, giving colours to both luminaries in different degrees and producing these differences in the measurements of Riccioli and Grimaldo.

Thus the theory to be hereafter developed, confirms the observations of Riccioli and Grimaldo made, particularly upon the horizontal diameter of the sun, and established so distinctly by the plain perceptions of sense, that nothing but that ingenious learning, which in every age has puzzled the plainest things, could have induced a doubt respecting what was actually seen, although the cause was not understood.

Riccioli accounts for the differences of opinion and measurement of others, by supposing that, by the adopted modes of measurement, which he states, differing from his, the external light of the limb of the object is intercepted, and its size reduced.

I have endeavoured variously to account for these continued errors of the acutest observers, by supposing that taking it for granted that all changes of place, or refractions, as they called them, were made in vertical circles, which is not correct, and that all measurement of any but the vertical diameter of the luminaries was unnecessary, upon finding this to be what was expected, they abandoned all further observations of horizontal diameters, not perhaps so conveniently measured as in the vertical line, or made them without sufficient care and attention.

The rainbow has been referred to, as exhibiting near the horizon, a considerable increase of breadth, upon the same principles which produce the apparent enlargement of the horizontal sun and moon, and thus confirming them. But one error is here adduced to support another. The cases are entirely different. The rainbow is seen under very different circumstances, and at short distances, which allow neither the imagination to act nor the atmosphere. Of the falling drops of rain, the increased sizes near the horizon, extend the spaces of formation of the radiants which they reflect, increase their number and divergence, and thus enlarge the dimensions of the bow which they form. I have seen the primary rainbow completely formed, and at the same time the following appearances exhibited: In the thin vapour of the cloud on high, was formed a narrow principal bow, attended with three or four inflected orders of colours, which are known to depend upon the small sizes of the drops. By degrees, lower down, these accompanying bows disappeared by uniting into the single primary, whose dimensions in the horizon became dilated into more than double its original breadth, in consequence of the increased sizes of the united drops.

The changes of colour, and of brightness, have not been attentively considered, or happily explained in themselves, or as connected with the other changes of place or of figure. The attempts to account for these are not more philosophical than those of the poet:—

> Ipse Dei clypeus, terrâ cum tollitur imâ
> Mane rubet, terrâque rubet cum conditur imâ.
> Candidus in summo est; melior natura quod illic
> Ætheris est, terraque procul contagia vitat.
>
> Ov. *Metamor.* lib. 15. l. 192—5.

In a continuous medium, a ray of light, passing by the side or edge of any body contained therein, is inflected by the attraction of the body near which it passes, and bent towards it.

In such a medium a ray of light passing between two bodies, is inflected and bent by the difference of the forces of the two bodies, towards that body nearest to which it passes, and whose attraction consequently prevails.

A ray of light so passing, is not only so bent and inflected, but is also dispersed and divided into parts more or less bent in various directions towards the inflecting body, the colours of its several parts being changed, from that of the original light, into rays of what are called prismatic colours, which coloured rays, even in the refractions of the prism, are produced by inflections. the blue being nearest to, and most attracted by, the inflecting points, the red most remote from and lest bent towards them, the intermediate, in and into intermediate directions.

A ray of light, passing perpendicularly through a series of particles concentrically arranged in the plane of incidence, and at equal distances between the particles, passes on without deviation or bending.

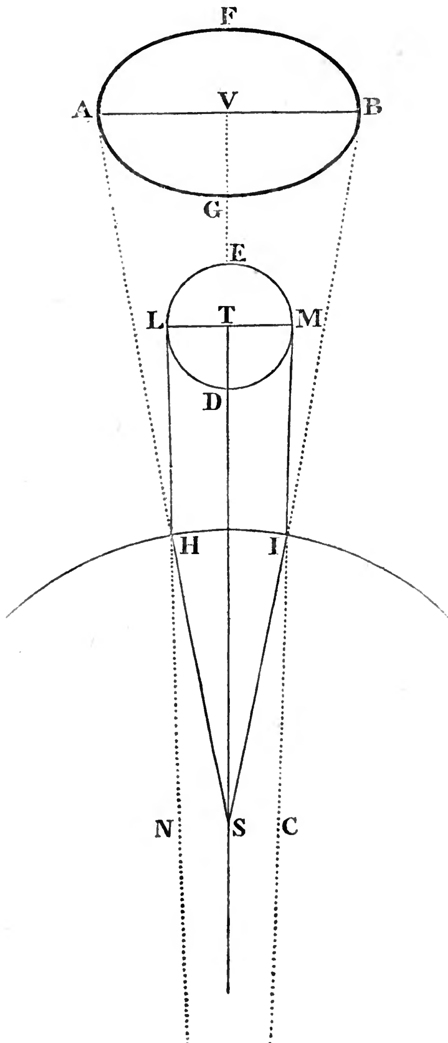
A ray of light, passing obliquely through a series of particles concentrically arranged in the plane of incidence, will be inflected and bent, in a direction inclined towards the radius drawn from the centre of arrangement to the point of passage.

A ray of light, passing perpendicularly, at equal distances, between the particles of each series, and successively through many series of particles, concentrically, and more and more numerously arranged in different successive planes, will be attracted by, and inflected towards the planes, in which the greater number of particles are disposed.

A ray of light passing obliquely and successively, through and between various concentric series of particles in various planes, and of variously increasing numbers in those planes, will be inflected and bent, into directions compounded of the directions given by the succeeding different planes and concentric series, conformably to the established laws of motion.

The body of air, incumbent upon the earth and its waters, acting by its powers of solution becomes more or less charged, more numerously at small, less numerously at greater distances from the surface, with particles of water floating in it, in states intermediate between absolute solution and rapid precipitation. These particles in a further state of separation from, and floatage in the air, are congregated into visible forms, and become fogs and clouds. In the intermediate state between perfect solution and visible separation, though they do not entirely destroy the transparency of the atmosphere, they yet exist in the air as floating detached bodies, capable of acting upon light by reflecting and inflecting it, and show their existence and powers frequently, by exhibiting the tracts of the sun-beams passing from between clouds through the air, or by otherwise variously acting upon objects seen through the vapours which they compose.

By the observation of those who ascended Mont Blanc, and were terrified at the black apparent void beyond the top, there were then no particles in the air, higher than the mountain, capable of reflecting light. Mont Blanc is not quite three miles above the level of the sea. Seldom higher than this, above the level of the surrounding region, can the vapours of flat countries be considered to rise, so at least as to become sensible by their actions on light, and to this height must be reduced the great concentric masses of sensible vapours supposed to be from fifty to seventy miles high. The principal accumulations of vapours, are indeed considerably short of this height, and being upon or near the surface, their strata may in effect be considered as of various horizontal diameters from 12, to 24, to 30, miles long, as they may be estimated to be of the height of one, two, or three miles.



Let S, be the place of a spectator, ED the sun, LM a line drawn through the centre of the sun parallel to the horizon, TS a line drawn from the centre of the sun to the spectator, HI a circular series of particles of vapour arranged at given distances from each other in the same plane with LM, and TS, and cutting TS at right angles, a ray of light from T passing along TS, through the circular series HI, at right angles and at equal distances between any two particles will proceed directly without bending to the eye of the spectator. Let LH and MI from the points L, and M in the line LTM, be rays parallel to and in the same plane with TS. As TS falls perpendicularly on HI, so LH and MI will fall obliquely on HI, and instead of passing on to N, and C will be bent at H and I, towards the perpendiculars to HI, into lines intersecting the line TS, and coming to the eye of the spectator in the directions SH, SI. This is the case with a single series of particles. Let the body of the sun ED be seen through many strata consisting of many similar circular series of particles, increasing in number and density in the several strata to the surface downwards, a ray of light TS, entering between the strata at right angles thereto, and descending through the lower strata interposed between the luminary and the eye, will be successively bent in the plane of the vertical in which TS is, and a change and elevation of place of the point T in the vertical will be produced. In this manner the points D and E, will be raised to G and F, and all the points between D and E, to places between G and F. But as in passing down between various successive strata, other the rays LH and MI are acted upon by two forces, one between the strata for elevating them vertically, the other by the circular series they enter obliquely for inflecting them towards their perpendiculars, these rays will consequently move in directions diagonal to the directions of these combined forces, and the points L and M will be not only elevated, but dilated externally from and out of the vertical on both sides, and apparently transferred to the points A and B. In the same manner all the rays proceeding from all the intermediate points of LEMD are variously elevated, dilated, and transferred to intermediate points of AFBG, and the luminary appears, with a smaller vertical, and extended horizontal diameter, of the form of two half ellipses combined on the same major axis, the lower considerably more eccentric than the upper. All rays not passing as above, will be dispersed and lost, or stopped and extinguished, and in an undulatory state of the strata of vapours, the observed undulatory changes of outline and limb, particularly of the lower limb will be produced. All these rays, vertical as well as horizontal, in passing on to the eye of the spectator, will not only be thus inflected and bent, but will be variously distributed, and dispersed into various colours, by the first and successive orders of particles by which they pass, and not only divided, but by degrees entirely separated from the rest of the direct light, in the order of colours from blue to red. First, all the blues will be dispersed, and separated from the rest, and scattered over, and variously reflected by the whole atmosphere, giving it, when seen free from clouds, the usual cœrulean blue, in the manner described in a paper on the Colours of Waters, in the 9th Number, Vol. V., p. 81, of the *Royal Institution Journal*, and never before clearly accounted for. After this separation, the colour of the luminary becomes yellow, until by the increased action of the denser strata of lower vapours, into which it descends, the yellow is entirely separated and dispersed after gilding with its colours the lower surfaces of the horizontal morning and evening clouds, leaving the sun of a bright red sustainable by the eye, and of a lustre continuing to decay, as long as the orb continues to be seen.

The phænomena, thus dependent upon the vapours of the atmosphere for existence, will vary also with these vapours, their quantities contained in air, and their states of perfect or partial solution therein, of more or less absolute separation up to that of rapid precipitation in the form of drops of water. The hygrometer fitted to determine and to measure these changes, of numbers of particles, and of condition of air depending thereon, would obviously be the instrument to be used in observations, rather than the barometer and thermometer, or together with these, inasmuch as the states indicated by these instruments mutually affect each other. In ordinary cases, these instruments may be consulted, but where extraordinary accuracy is required in determining the places of the heavenly bodies, recourse must be had to more direct observations made at the time.

I have thus accounted for the changes of place in the heavenly bodies, the changes of colour, the changes of figure and dimensions, and the occasional undulatory changes of limb and outline, together with the occasional variations even of these changes, by referring them to the same principles, to one and the same existing cause, acting variously, and variously modified; and he who can continue to believe that the apparent increase of size of the sun and moon, in the horizon, is a deception produced by comparison with terrestrial objects, or by assigning them, according to the principles of perspective, dimensions depending upon their places in a supposed less than hemispherical, or rather oblate spheroidical sky, may ascribe all the other concurrent appearances and changes, change of colour, change of place, change of figure, undulatory changes of limb and outline, to delusions, not only ocular but mental, of the mind as well as of the mind's eye. G. W. J.

ART. III. *On the Native Country of the Potato, and on some American Plants. Communicated by* A. B. LAMBERT, Esq., F.R.S., *&c. &c.*

IT has long been a desideratum among botanists to ascertain the native country of the potato, *Solanum tuberosum.* I beg leave now to offer some communications on that subject, which I have lately received in a letter from the celebrated author of the *Flora Peruviana,* Don Jose Pavon, who resided many years in South America, dated, Madrid, September 23, 1817, who says, " The *Solanum tuberosum* grows wild in the environs of Lima, in Peru, and fourteen leagues from Lima on the coast; and I myself have found it wild in the kingdom of Chili. I can assure you this is the truth. The Indians cultivate it in great abundance in Peru and in Chili, and call it *Papas*. There are other wild species, such as *Solanum montanum* which also gives a *radix tuberosa*." Of this I have received from the above-mentioned author of the *Flora Peruviana* fine wild specimens with the root. In another letter, dated Madrid, Nov. 10, he again repeats, " I mentioned to you that *Solanum tuberosum* grows spontaneously near Lima, and in the kingdom of Chili, where it was also found by my companions Dombey and Ruiz ;" I have lately received from Don Pavon very fine wild specimens of *Solanum tuberosum*, collected by himself in Peru. Don Francisco Zea, companion and friend of the celebrated Mutis, who long resided in South America, assured me, when he was in this country, that he had often found it wild in the forests near Santa Fé de Bogota, observing at the same time, that the reason why Baron de Humboldt had not found it when he was in that country, was, because he had not time to examine those places where it grew. In a letter (addressed to Mr. Frazer of Sloane-street, Chelsea,) lately received from Dr. Baldwin, an excellent American botanist, who has lately returned from the coast of South America, in the Congress frigate, of the United States, he says, " I found many plants that appeared to be new during my excursion in South America, and had the satisfaction of submitting most of my specimens to M. Bonpland, who has settled himself in the vicinity of Buenos Ayres. It was not the least pleasing of my discoveries to find the famous *Solanum tuberosum* growing spontaneously among the rocks on Monte Video; in a part of the country, too, where this valuable vegetable is not cultivated. I also found it on the same side of the river in the vicinity of Maldonado." A species of *Solanum* was found by Commerson in the neighbourhood of Monte Video, named by Dunal, in his *Synopsis of the Genus Solanum*, page 5, *Solanum Commersonii*, from a specimen preserved in the Museum at Paris. It is also described in the *Supplement to the Encyclopédie Méthodique*, Vol. III., p. 746. I have no doubt that this is the same with the plant found by Dr. Baldwin. On making inquiry, relating to this plant, of Captain Bowles, who has lately returned from the South American station, and who has resided for a considerable time at Buenos Ayres, he told me he knew it well, and that it is a common weed in the gardens and in the neighbourhood, bearing small tubers like those of the potato, but too bitter for use. Whether this be the original stock of our common potato improved by cultivation, future observation must determine. Molina, in his *History of Chili*, speaking of the potato, says, " It is indeed found in all the fields of that country, but those plants that grow wild, called by the Indians *Maglia*, produce only very small roots of a bitter taste."

There appeared lately in the English newspapers an account of a root which is very much cultivated in *Peru* under the name of *Arracacha*, and which would be a very desirable acquisition to this country. The before-mentioned Don Francisco Antonio Zea, formerly professor of botany at *Madrid*, who has lately arrived in this country from *Santa Fé de Bogota*, (New Granada,) informs me that the *Arracacha* grows abundantly at *Santa Fé*, *Junga* and *Pamplona*, where it is very much cultivated and eaten in the same manner as we do potatoes in this country; and says that the plant which produces it belongs to the natural order *Umbelliferæ*, and that it has a tapering root, about six inches long and two inches thick. I have little doubt that this is the same with the *Heracleum tuberosum, foliis pinnatis; foliolis septenis; floribus radiatis;* of Molina, who gives the following account of it: " This plant resembles very much in its leaves, flowers, and seeds, the *Common Cow Parsnip*, but is distinguished from it by the greater number of roots it bears, which are six inches long and three inches thick, of a yellow colour, and of a very agreeable taste."

Don Zea has also afforded me another piece of information, relating to a small species of *Mays*, or *Indian corn*, which was introduced last year from France under the name of *Mays de Poulet*, and which ripens its ears two months earlier than the common kind. I had suggested to Mr. Sabine, (who has mentioned it in the *Horticultural Transactions*,) that it was very probably the *Zea Caragua* of Molina; and my opinion is now confirmed by Don Zea, who knew it at once on its being shewn him. He informs me that it is cultivated in great abundance in Chili by the Indians, particularly in the district called Caragua, whence its name.

I beg leave to mention another communication, also from Don Jose Pavon, relating to a substance which has been several times sent from Spain, under the name of Spanish tinder, resembling very much the Moxa of the Chinese. We never have been able till now to ascertain the plant from which it is manufactured, which proves to be *Echinops Strigosus.* There are three different sorts; the first, *Yesca de Espagna,* or *Amadou d'Espagne, O de Cardo de las flores (ex floribus)*; the second, *Yesca de Espagna,* or *Amadou d'Espagne, O de Cardo de los hogas (ex foliis)*; the third, *Yesca d'Espagna,* or *Amadou d'Espagne O de Cardo de las tallas (ex caulibus.)*

I take this opportunity of inserting another observation which I have also received from Spain.

The tree which produces the balsam of Peru, *Myroxylon Peruiferum,* appears to be the same as *Toluifera balsamum,* or that which yields Tolu balsam. The following account of it is given by Don Hippolito Ruiz: "The balsam of Quinquina is procured by incision at the beginning of spring, when the showers are gentle, frequent, and short. It is collected in bottles, where it keeps liquid for some years, in which state it is called white liquid balsam; but when the Indians deposit this liquid in mats or calabashes, which is usually done in Carthagena, and in the mountains of Tolu, after some time it condenses, hardens into resin, and is then denominated dry white balsam of Tolu, by which name it is known in the druggists' shops." Having examined the specimens of *Toluifera balsamum,* from the *herbarium* of Sir Joseph Banks, I find them exactly the same as *Myroxylon Peruiferum,* and which was sent to Linnæus by Mutis, as the plant producing the balsam of Peru. I have lately received fine specimens in flower and fruit, and also a specimen of the trunk of the tree with the bark on; it is between three and four inches in length and about three inches in diameter, and was collected by the authors of the *Flora Peruviana.*

Art. IV.—*On the Granite of Aberdeenshire, and on the Identity of certain Varieties of Granite, with other Rocks appertaining to the Trap Family.—By* J. Mac Culloch, M.D., F.R.S. *Communicated by the Author.*

It is well known to those who are conversant with rocks, that many members of the trap family, including under that term all the unstratified rocks which lie above the secondary as well as the primary strata, bear a striking resemblance in their mineral composition and general aspect, to some of the varieties of granite; there being comprised under this denomination, all the unstratified rocks which are inferior, not only to the secondary, but to the primary strata. A very remarkable instance of this resemblance, is pointed out in the article which relates to the Isle of Sky, in my work on the Western Islands of Scotland; and similar examples, if less striking, may be seen in many parts of that country, so fertile in all the interesting appearances which are found in the rocks of this multifarious family. The important views that may be deduced from these facts, will be considered hereafter; it being the primary object of this paper, to confirm this important analogy by evidence from a different source, but of the same nature, derived from the existence of those rocks which form some of the most common and conspicuous varieties of the trap family, not only in the situation occupied by granite, but connected with the most authentic masses of that substance by a bond of mutual and imperceptible transition.

Those who are acquainted with Scotland, know that granite occupies an extensive space in Aberdeenshire; and that where it does not appear at the surface, it is covered, and often to a very inconsiderable depth, by different members, both of the primary and secondary strata.

Among these, gneiss is the prevailing rock. In some situations, it forms mountains of considerable elevation, such as Noath and Coreen; but, towards the eastern side of the country, it is found at a general, but irregular low level; like the granite, with which it is also intermixed in patches of various, often of minute, dimensions, and of very uncertain recurrence.

Micaceous schist also occurs; but towards the western side of the county, principally; as it is scarcely to be found in that tract where granite is the prevailing rock, and which alone is the object of the present paper.

The same remark may be made on quartz rock, which is also found in considerable masses, in some of the western mountains; but it is either rare, or nearly altogether absent, in those places where extensive masses of granite are visible at the surface.

Clay slate occurs in a much more conspicuous manner; forming some tracts of considerable extent and of very moderate elevation; and, in many places, being, like the gneiss, in contact with the granite. It is, further, in some cases, so thin, and so intermixed in patches with that rock, as obviously to form but a very superficial covering over it; the fundamental granite protruding through the schist in many places, in such a manner as to allow its continuity to be inferred, even in those places where it does not reach the surface.

It is unnecessary to notice particularly, the masses of serpentine which are found in many parts of the district under review; and the more rare beds of primary limestone which lie in the western and southern mountains among the other stratified rocks.

These then form the whole of the primary strata which occur in that part of Aberdeenshire now under review. Of the secondary, the lowest or *old* red sandstone is found in different places; but, with one rather doubtful exception, it does not appear that any of the superior secondary strata, whether of sandstone or limestone, exist in any part of this district.

It is, lastly, important to remark, that no instances of superincumbent trap rocks are to be discovered throughout this extensive tract; nor, after a careful research, could I find any veins of these substances. That extensive body of these rocks which occupies so large a portion of the central and secondary district of Scotland, ceases entirely before the meeting of the strata of this class with the primary ridge which forms the southern boundary of the northern mountainous division of this country. A few veins only, are, in some places, found to penetrate the primary strata in this direction; but, after no long course they entirely disappear.

This slight sketch of the nature and disposition of the stratified and superincumbent rocks which occur in the district under review, will be useful in attempting to trace the general extent and continuity of the granite which forms, not only the basis, but the chief visible portion of that tract which is the repository of the phænomena to be described in this communication.

The most prominent and conspicuous masses of that rock, are those which form the high mountains of Mar, and which contain the sources of the Dee. In tracing from these mountains towards the sea, eastward, the granite is found re-appearing in numerous places; the interruptions to its continuity being produced by portions, more or less extensive, of the primary strata already described, and, principally, of the gneiss. In this manner it may be traced to Portsoy, and, more or less interruptedly round the coast to Aberdeen. Without a map, it would be impossible to convey any accurate ideas of its geographical position and extent; but it will be sufficient here to remark, that two irregular lines drawn from Ben Avon to the places just named, will include the principal part of this rock in Aberdeenshire, and all that which it is necessary for the objects of this paper to notice.

Having formed a geological map of this entire district, I have been enabled to infer, from a comparison of the several apparent portions of the granite, and from the positions and situations of the superincumbent strata, that the continuity of that rock may not only be deduced, but, in many cases, actually traced, in some place or other, in such a manner as to leave no doubt respecting the identity and connexion of the whole. This district must therefore be considered as formed of a continuous body of granite; covered and obscured, in many places, by portions both of the primary and of the most ancient secondary strata, and appearing wherever these have been removed by those wasteful operations of which this side of Scotland presents so many other striking evidences.

The last very elevated mass of this granite in the north-eastern part of Aberdeenshire, is the mountain Bennachie; but, between this point and the sea to the eastward, it appears, even at the lowest levels; occupying extensive spaces, without the intervention of gneiss or any other superincumbent strata; or, in some places, covered with very thin portions of the former, not exceeding a mile, or even much less, in dimensions. I am induced to notice this tract more particularly, because it is there that the peculiarities about to be described, are most accessible and most conspicuous.

The general continuity of all the granite of Aberdeenshire being thus established, it is next necessary to remark, that, throughout the greater part, it exhibits those mineral characters, which, even by those who imagine that there are distinctions in the relative ages of different kinds of this rock, are considered to be indications of the highest antiquity. The mountains at the sources of the Dee, are well known, by all who have examined this country, to be formed of that granite which consists of quartz, felspar, and mica; and it is, perhaps, unnecessary to say, that the same character pervades the flatter portions to the eastward; as the extensive use of the Aberdeenshire granite in the pavements of London, has made it familiar to every one. It will be necessarily noticed hereafter, that the variety which occurs in Bennachie, presents, in particular, those characters which are supposed to appertain to the most ancient granites; as the quartz and felspar in it, are, in many places, distinctly crystallized.

To this proof of antiquity derived from mineral characters, may be added that which is usually inferred from geological position, by those who contend for this theoretical view of a difference in the relative ages of granite. It is, in most places, inferior to gneiss; and, if in some, clay slate, or even the red sandstone, is found in contact with it, the continuity of these portions with others which are immediately subjacent to gneiss, is easily traced.

The preceding remarks on the general continuity and common antiquity of all the granite of Aberdeenshire, might have been spared, had this paper been intended for those only, who entertain the same opinions as myself respecting the origin and nature of that rock. But as many geologists still maintain, that granite, like the stratified rocks which cover it, is of aqueous origin, and as they have even imagined a succession of deposits of this rock, some of which they have placed in their hypothetical division of a *transition* class, it became necessary to anticipate the objections which might be urged against the facts immediately to be described, by showing that the writer of this paper had investigated the subject as if he himself had maintained, with them, those opinions which all his observations have taught him to reject.

In traversing this country in the summer of 1819, I was surprised to find blocks of greenstone and of basalt scattered over the surface in different places; particularly, as no indications of trap rocks *in situ*, or even of veins of that nature, were any where to be discovered. These also were every where accompanied by blocks of the common granite of the country; as usual, rounded at the angles by the effects of time. No marks of wear, however, were in general to be observed in the basalts and greenstones; nor did they present those well-known marks of long exposure and distant transportation, which, in the rocks of this family in particular, become very conspicuous after no long period.

Unable, however, to account for them from any other cause, and finding their mineral characters to coincide very accurately with those of the trap rocks of the Western Islands; and of the central district of Scotland, I, at first, naturally attributed their origin to some veins, or insulated masses, which had escaped my observation. But the same substances recurring again in other places, where, from examining the country around with the most scrupulous accuracy, I was satisfied that no trap rocks existed, I became unwilling to rest in the vague conclusion that they had been transported from some far distant situation, or were the remains of masses long since vanished; more particularly, as they shewed no marks of such transportation, and as it was not easy to conceive that detached blocks of a small size, should remain in a state of integrity, while the larger masses, whence they must have been derived, had disappeared.

Recollecting that the trap rocks so often approximated to granite in their mineral characters, I was thus induced to suspect that granite might also, in the same manner, vary in its characters, so as to resemble the specimens which, in that family, are known by the name of basalt and greenstone; a conclusion the more probable, as many of the fine grained granites in which hornblende enters as a constituent, often resemble some of the greenstones of the trap family; differing from them, principally, by containing quartz; and that basalt, in some of its varieties at least, consisted of the same ingredients as greenstone, in a much more minute and intimate state of mixture.

This suspicion was strengthened by the views, which have long been familiar to myself and to many of the readers of this paper, respecting the common igneous origin of both these classes of unstratified rocks, and I was therefore induced to search more minutely among the solid granite for a confirmation of it. The incumbrance produced by the deep alluvial soil of this country, for some time checked this investigation; but it was at length completed, and in so many different places, as not to leave the shadow of a doubt respecting the nature of the rocks in question, and of their common origin and continuous connexion with the more ordinary granite of the country.

Among other places, I may now point out, for the satisfaction of other geologists, some recent sections of the granite between Old Rain and Meldrum, and at several other points in the same neighbourhood, which cannot be more particularly designated for want of local references. In these, it is easy to see the transition which takes place between the common granite and these greenstones; and the further change, by which the coarser greenstone becomes a basalt, or assumes an uniform texture in which the separate minerals are no longer distinguishable. If any suspicion had remained that these were veins of trap traversing the granite, they would have been completely removed by examining their forms, their connexions with that rock, and the frequent and imperceptible transitions which occurred between the two; transitions precisely similar to those which take place where ordinary granite changes its character, either by varying its composition, or by an alteration in the nature of its texture.

On a further investigation, it was found that the rocks of this character occurred in very considerable tracts; irregularly intermixed with the common granite, in such a manner as to equal it in quantity, and to remove all possibility of hesitation respecting their continuity and their community of geological origin and position.

It was already remarked, that a part of Bennachie consisted of an ordinary granite, in which the ingredients, and more particularly the quartz and felspar, were frequently crystallized. On the northern face of this mountain, the rocks in question occur in great abundance; passing into the common granite, and forming, in some places, an equally large proportion of the general mass. The want of artificial sections, prevents the transitions from being here seen as clearly as in the places last described; but there is still no difficulty, by the use of the hammer, and with proper attention, in confirming the truth of those views on which it is now unnecessary to dilate any further.

It only remains to describe the mineral characters of the rocks which have thus been shown to form part of the general mass of granite in this country; and that description will still further show the analogy which, in so many other important points, pervades all the unstratified rocks, however distant in position and in apparent antiquity.

Quartz so rarely enters as an ingredient into these substances, that it may be altogether excluded from the present consideration. The fundamental composition consists of felspar and hornblende; and, according to the magnitude of the parts, and the relative proportions of these ingredients, the appearances of the specimens vary. In some rare instances, the crystals of hornblende are so large as to attain half an inch in length, although they are not defined in form; and as the felspar is commonly white, these varieties form beautiful specimens for collectors of rocks. From this size, the portions of each mineral vary in gradation; forming compounds which are undistinguishable in every respect from the coarser and finer greenstones of the trap family; from those, at least, in which common, and not compact felspar, forms the other ingredient in union with the hornblende.

In all the cases which came under my notice, the hornblende is invariably black, but it is not always intermixed in an uniform manner with the felspar; some instances occurring in which, to the general indiscriminate mixture, are superadded large and distinct patches or irregular crystals; producing that appearance which, when it takes place in ordinary granite from a similar disposition in the felspar, has been called *porphyritic.* In general, the felspar is white, and of that variety which is called *common.* But in the minuter states of intermixture, it has often a greenish hue, and so far loses its crystalline appearance, as to resemble the ordinary compact felspar which is more common in the greenstones of the trap family than the crystallized kind. Whether these varieties, however, actually contain compact felspar, I have not quite satisfied myself; and the confusion which sometimes exists between these two minerals is such, that I am willing to leave this point undetermined; however inclined to believe, that compact felspar occurs in these rocks just as it does in the greenstones of the trap family.

When the mixture of the two minerals, which forms the greenstone of this granite, becomes minute, the rock is no longer distinguishable from ordinary basalt; and, in some specimens, it even appears that the felspar is at length entirely excluded; so that there remains nothing but that compact, yet minutely granular aggregation of hornblende, which, according to some mineralogists, constitutes the only genuine basalt. It is further highly interesting to remark in this case, that these basalts have often that internal concretionary structure which causes them to exfoliate in laminæ on exposure to air; and which is so remarkable a feature, not only in the basalts, but in many of the greenstones of the trap family. I must also observe, that among the rocks of this apparently simple character, there are often found specimens which cannot be distinguished from the black claystones which, by some authors, are also called basalt, and which occur in such abundance in the trap formation. In these, the peculiar lustre which characterizes hornblende is absent; the specimens presenting an uniformly dull aspect, with an earthy fracture and a greater degree of softness.

Although, in speaking of these compounds, I have occasionally used the terms greenstone and basalt, on account of their accurate resemblance to those substances as they occur in the trap family, and because these names are justified by the mineral composition and character of the specimens, they must still be considered as varieties of granite; using that term, in a general and geological sense, to comprise all the unstratified rocks which are found beneath the primary strata, and which, whatever differences they may present, are still associated by some general mineral characters, and by a bond of mutual transition. These terms, however, cannot be applied in this case without great inconvenience; and ought not to be used hereafter in speaking of these substances, whenever the facts now stated shall be admitted by geologists as established. I have, in other writings, pointed out the great, and almost incorrigible confusion, which has already arisen, from applying the term *syenite* to compounds occurring both in the family of trap and in granite; and the inconveniences of a similar nature which have been produced, by using the term greenstone in the same vague manner. It is evident, that the same confusion, even in a greater degree, would follow from adopting the term *basalt* in the present case.

In every instance in which rocks of a similar nature occur in the primary and secondary classes, it is most important to distinguish them by some expedient; as geological descriptions would either become unintelligible, or be attended with the most inconvenient circumlocution. Limestone has thus been distinguished by the addition of the terms primary and secondary; argillaceous schist, by using, in one case, the denomination of clay slate, in the other, that of shale. In the cases of granite, and of the trap family, the confusion which would ensue from neglecting to make such a distinction, would be even greater than in the stratified rocks. With an origin far distant in point of time, the members of the trap family are not only found in contact with granite, but they also penetrate it in the form of veins. It is scarcely possible, even with all the assistance afforded by a distinct set of terms, to prevent superficial geologists, who are contented with the first and obvious appearances before them, from confounding such recent rocks with the more ancient to which they approximate; and, without such terms, even the most careful observers could not convey accurate information, without danger of misapprehension or without circumlocution.

As an expedient towards attaining this object in the present instance, it might be suggested that the addition of the adjective terms, primary and secondary, would suffice; and we should then have primary and secondary basalts and greenstones. But as the term primary has been sometimes applied, by those who only judge from superficial examination, to the recent veins of this nature which penetrate the older rocks, it appears preferable to abandon its use altogether. Perhaps a better expedient will be found by applying the adjective term granitic to the rocks in question. Thus they may be designated by the terms, *granitic greenstone*, and *granitic basalt*; denominations, which, while they indicate the geological connexions of these substances, are also explanatory of their mineral characters; and of the relation which, in this respect, they bear to the corresponding rocks of the trap family. It remains for geologists to adopt or reject this expedient as they may see right.

In thus terminating this account of these very interesting varieties of granite, I may be allowed to add, that their history offers a very useful lesson to those geologists who are either content with the first view of things, or who are always ready to determine respecting the appearances which they find, according to some preconceived opinions, or from the vague and superficial notions derived from other teachers than that great instructor, from a careful examination alone of whose phenomena, truth can be elicited. It will also point out the facility with which the most serious errors may be introduced into geological science, by trusting to the mineral characters of rocks, and by neglecting to trace the connexions of such substances with the surrounding masses. If the novelty of the facts which have thus been described, had not rendered the preceding minute details necessary, they would have been still useful to the student, by pointing out the steps which were followed in the investigation, and the nature of the reasoning from which the conclusions were deduced. If his ambition be to extend the boundaries of geological science, if he is not content to repose in the calm belief that every thing is already known, to see through the eyes of teachers, perhaps less competent than himself, and to describe in a received phraseology, appearances, and analogies, which have no existence but in that language which he has been taught, let him be assured that he must bring to his task, industry, patience, and, above all, an unbiassed mind. Nature will neither long deceive nor disappoint him who is only desirous of truth; but the book which she opens to his inspection must be studied with care, and, more especially, with a desire to learn.

Having thus shown the identity of certain varieties of granite, with other rocks appertaining to the trap family, it will be useful to place, in a condensed view, those instances already alluded to in the beginning of this paper, where the members of that family present the characters which are most generally found in granite. A few of them have been pointed out in the author's work on the Western Islands, to which allusion has already been made; but the importance of the subject is such as to demand a more distinct statement of the several facts, while the nature of the present communication affords an opportunity of balancing and comparing them with the analogous phenomena described in it. Thus it will more readily be perceived, that whatever resemblance the most ancient unstratified rocks may sometimes bear to the most recent, corresponding examples are not wanting in the latter of a similar resemblance to the former. That this comparison has never yet been distinctly made, or supported by the evidence of facts, will be an additional reason for extending this paper so as to comprise whatever is necessary for that purpose in the history of the trap family.

In the general, or geological, features of granite and of the trap rocks, there are so many points of resemblance that they cannot fail to have attracted the attention of the most ordinary observers. Granite is never stratified, but is found in shapeless masses which are subjacent to all the strata, of whatever antiquity, near to which they lie. To examine and analyze all the contradictory opinions, which have prevailed on this subject, is here inadmissible; but it may be remarked, in a general way, that the adduced instances of stratification in granite, may all be referred to the laminar concretionary structure on the large scale; or are portions of gneiss of which the texture so often becomes perfectly granitic; or, lastly, are veins of that rock traversing the gneiss in directions parallel to its stratification.

The trap rocks are also unstratified; or, in the predominant instances at least, these irregular forms prevail, while the masses differ from those of granite in being superior to all the rocks which they accompany. Instances of a disposition which has been esteemed a true stratification, are however not uncommon among the rocks of this family; but these, whatever resemblance they may bear to genuine stratification, admit of other explanations. Into the details of these it is also impossible here to enter, as it would involve a long train of facts and discussions. It must suffice to say, that all the instances of stratified trap yet produced may be easily explained, and are, indeed, in most cases, demonstrably proved to be either veins parallel to the strata in which they lie, or thin superincumbent masses of which the forms have been determined by those of the subjacent stratified rocks; or else strata of shale, or of other substances, which have been converted into trap by the same causes which sometimes change them into siliceous schist; or, lastly, tufaceous rocks which appear to have been either deposited in the shape of mud, as similar materials so frequently are by volcanic eruptions, or else generated, like the sandstones, from the wear of more ancient rocks of the same nature.

Granite, and the trap rocks, are both found in the shape of veins, and they are the only rocks which are known to be disposed in this manner, it being here understood that, under the term *trap*, is included every instance of porphyry, as well as those varieties which are peculiarly connected with the most recent greenstones, basalts, or claystones. It has indeed been asserted, that sandstone and limestone, and even clay-slate, have been found forming veins, but it is easy to see that these imaginary observations are either the result of ignorance or inexperience, or are the produce of something more than voluntary self-deception for the purpose of supporting an hypothesis.

The veins, both of granite and of trap, have, in so many instances been traced to principal masses of the same rocks, as to leave no reason to doubt that this character is, in both, universal. In both cases the want of free access occasionally prevents these connexions from being ascertained; in the trap-rocks another cause sometimes interferes with this investigation, namely, the entire loss, from the effects of time, of the great superincumbent masses, while the veins remain, protected from destruction by the strata in which they lie.

Both these classes of veins ramify, by subdivision, as they proceed from the central or principal masses; but that feature is most common in granite, while the veins of trap also differ from them, very generally, in holding much longer courses without any change of dimension. These differences, however, do not destroy the analogy which subsists between these two rocks; and they admit of explanation by collateral circumstances which need not be examined in this place.

The passage, both of granite and of trap veins, through strata, is accompanied by peculiar appearances which are, in both cases, of a similar nature, and which often, indeed, correspond very accurately. In their immediate vicinity the strata are displaced, distorted, or broken. In the case of trap also fragments of the adjoining rocks are often entangled in the vein; and if that occurs less frequently in granite veins, it still happens sufficiently often, both in these and at the contact of the larger masses of granite with the strata, to justify that general analogy which is alone contended for in this place.

The alterations in the mineral characters of the strata, which occur at the junctions of these two classes of rock, are also in both cases of a similar nature, resembling each other in their general features, and only differing according to the previous characters of the strata subjected to this influence. In both instances of these junctions, the argillaceous schists are indurated and changed into siliceous schist. In some, the contact of a granite vein converts that schist into hornblende, while the contact of trap with the secondary argillaceous schist or shale, frequently produces a substance scarcely differing from basalt, and thus far analogous also to that mineral, which forms the principal ingredient of this rock. The effects produced on limestone, in both cases, correspond still more accurately and obviously, because the limestones of the primary class, which are those alone traversed by granite, differ less from those of the secondary, which are principally subject to the influence of trap, than any other of the analogous strata in the primary and secondary classes do from each other. Such limestones, when impure, or containing much siliceous and argillaceous earths, are in both cases, converted into substances resembling chert. Where, on the contrary, they consist of carbonate of lime alone, they assume a crystalline texture near the points of junction with the veins; and the causes of that change are very obvious in those instances where, in the distant portions of the rocks, these limestones have a compact or earthy texture, as is particularly the case where chalk is traversed by trap veins. If, in other cases, the veins of granite produce less effect on the adjoining rocks than those of trap, it must be recollected, that the former traverse exclusively the primary strata, of which the mineral characters are such as to be scarcely capable of undergoing those changes which, in the contact of trap with the secondary strata of softer texture, are easily induced.

On this subject of the general analogy between these two classes of veins, it may lastly be remarked, that where granite ramifies into minute filaments, the mixed crystalline texture disappears, and the ultimate branches become uniformly compact, appearing to consist of an intimate mixture of quartz and felspar, or of felspar alone. In the same manner, where trap veins have been found ramifying in the same way, the minute branches lose the crystalline character and acquire a fine compact texture, so as to resemble either pitchstone, or that siliceous schist which is called Lydian stone. Such ramifying veins, it is true, are rare, but they have been pointed out, in that work on the Western Islands already mentioned, in several places, namely, in Barra, South Uist, and Sky.

To enter further into this subject, and to support it by all the evidence of facts which might be brought forward for that purpose, would be to involve the whole history of these two remarkable and extensive classes of rock, and, in fact, to produce a treatise utterly incompatible with the nature of this communication, and with the space to which it is unavoidably limited. Practical geologists will be at no loss to supply, from their own knowledge, whatever is wanting; and those to whom geological investigation is yet new, or who have suffered themselves to be diverted from the study of nature by hypothetical dogmas, will thus be directed to the use of their own faculties in observing the phenomena which they may witness, and to the exertion of their own judgments in reasoning from them.

Having thus pointed out the geological resemblances which exist between the trap rocks and granite, it is necessary to advert to one important point of difference, on which a greater stress has been laid than the circumstances appear to justify.

It has been remarked, that although the former are found to lie above the secondary strata, which they chiefly accompany, granite is never found in the same manner lying on the primary. Hence it is argued, that, even if the igneous origin of trap be admitted, the defect of this important feature in granite, is a sufficient reason to refuse to it a similar origin.

There are many collateral circumstances, however, to be considered, before the justness of this reasoning can be admitted. The principal of these relates to the waste which the surface of the earth has undergone; but, on this subject, I need not repeat that which is already familiar to geologists, and which consists merely of general reasoning derived from analogies. If indeed the instance quoted by Mr. Von Buch, in Norway, of granite incumbent on conchiferous limestone, and the similar fact immediately to be described as occurring in Sky, be admitted as examples of real granite, the doubt in question is removed, and the fact of the superincumbence of that rock is established.

But it will be seen hereafter, that no advantage is taken of these examples, as they are considered to be modifications of the trap family. The term *granite*, here used in a strictly geological sense, is limited to all the rocks of this character which are subjacent to the primary strata only, or, when both classes occur together, to both. The veins which proceed from it also penetrate the primary strata only, and not the secondary, and thus it is proved to be of a date prior to the deposition of the latter. In this rigid view, therefore, of the meaning of the term granite, if ever it is found in a superincumbent form, it must lie on the primary strata alone, and not on the secondary, and thus also it might exist in masses intermediate between these two classes of stratified rocks. We are by no means sufficiently acquainted as yet with the multitude of existing appearances to pronounce a negative on this subject; and the difficulty of investigating accurately phenomena of a much more obvious and simple nature, will teach experienced geologists to reserve their opinions respecting it for a period of greater information.

It has moreover been shewn in this paper, that greenstone and basalt, or rocks identical with these in their mineral characters, occur as varieties of the most decided granite; and it must also be well known to all observers, that the limits between granite and some of the porphyries connected with it, are frequently evanescent. It is therefore far from improbable that some of the superincumbent masses of these substances which are found on the primary strata, are truly connected with subjacent granites, and are modifications of that rock, not of those of the more recent trap family.

Having thus stated the geological resemblance which exists between granite and the trap rocks, and having, in the preceding part of this paper, pointed out the identity in mineral character between certain varieties of granite and others appertaining to the latter family, it remains to enumerate some of the most remarkable instances in which the trap rocks assume those characters which are predominant, and have been esteemed essential, in granite.

In the island of Sky there is found a body of primary strata, succeeded by a tract of secondary limestone, shale, and sandstone. This secondary tract is, throughout the greater part of its extent, covered by an immense mass of trap rocks, the proofs of their superincumbent position being displayed very distinctly in many places. Most of the varieties of this family which are as yet known, and one which exists only here and in the neighbouring land of Airdnamurchan occur in this space, and the whole of them are connected by imperceptible gradations.

Among these is found that compound of felspar and hornblende, with excess of the former mineral, to which the term *Syenite* has been applied, and to which, together with the analogous rocks of the same family, it is here exclusively limited. On one side, this rock passes into porphyry in the usual manner; or, by the loss of its hornblende only, into a simple rock, which in the same imperceptible manner, graduates into claystone. But, in another part, quartz, and subsequently quartz and mica both, are superadded to the compound of hornblende and felspar; and thus there is produced a rock, in no way differing from many varieties of ordinary granite, and, in particular, strongly resembling some of those which occur in Arran. The connexion of this *granite*, or rather syenite of a granitic character, with the adjoining ordinary trap-rocks, is such as to admit of no doubt respecting its identity of origin; and it is unnecessary to say that it is thus proved, even if more distinct evidence of that circumstance were not accessible, to be superincumbent on conchiferous limestone. The instance already mentioned as described by Mr. Von Buch, must doubtless be considered as of the same kind; and even those who would otherwise be inclined to withhold their assent from this view of its nature, will probably choose to adopt this conclusion, rather than to admit of a granite more recent than the latest secondary strata, or of one which occupies that superincumbent position, the existence of which has been refused to those who argue in favour of its igneous origin.

In St. Kilda, there is found a mass of trap, consisting chiefly or entirely of that variety which I have called *augit rock*, connected with a syenitic rock in which hornblende and felspar form the chief ingredients, but which also contains quartz. Although no stratified rocks are found in this island, it may be concluded, from the mineral characters of these rocks, and more particularly from the presence of augit, which exists as an essential contituent only in the trap family and in the volcanic rocks, that St. Kilda belongs to the family of trap, and not to that of granite.

In this syenite cavities are of frequent occurrence, containing both the felspar and quartz in a crystallized state, and very nearly resembling in this respect many of the granites of Bennachie, already mentioned, as well as some of those which occur in Arran. The quartz, in particular, is remarkable for bearing those characters which it so often presents in granite, being brown, and often crystallized in its most ordinary form, so as to attain an inch or more in length. It is remarkable that the same circumstance occurs in the quartz belonging to the granite of Arran, in a manner so exactly similar that the specimens are undistinguishable.

Among the varieties of trap occurring in Sky is found a compound of hypersthene and felspar to which I have given the name of *hypersthene rock.* In its external general forms, the aspect of this rock is such as to be undistinguishable from that of granite. Like this, it is found in huge curved beds, sometimes divided into prismatic and cuboidal forms, and rising into those sharp and permanent peaked summits, which are so often characteristic of granite, and which have indeed been deemed peculiar to it. Although the mineral composition of hypersthene rock is entirely different from that of any granite yet known, the texture is the same; and it is further highly worthy of remark, that, in some places, it assumes the foliated tendency of gneiss, from a peculiar parallel disposition of the crystals of hypersthene. Further, in many parts, it contains garnets, disposed in the same manner as they often are in granite, and of the same character.

That the ordinary greenstones of the trap family sometimes resemble those similar compounds found in granite, by containing quartz, is matter of such general notoriety that it is unnecessary to describe the examples. Nor is mica necessarily excluded from these, although it must be considered as a rare ingredient. With respect to the texture of the rock, or the magnitude and disposition of the integrant minerals, it may be observed, that the greenstones have a character which is often perfectly granitic, the felspar and hornblende being distinctly crystallized on a very large scale, and interfering with each other's regular forms. The neighbourhood of Edinburgh presents numerous and remarkable examples of this nature: and, in that neighbourhood also, are to be found masses of ordinary greenstone incumbent on the most recent strata, the forms of which so perfectly resemble those of granite, in the prismatic division of the parts and the subsequent rounding of the angles, that they are undistinguishable without manual examination. The Corstorphine Hills contain the examples of this latter occurrence, as the rocks near the Queen's-ferry do those of the highly crystalline texture.

Having thus pointed out, in a general manner, the resemblances that occur between some of the rocks which belong to granite and others which are members of the trap family, I may notice, but in the briefest manner, the most conspicuous differences which exist between them. Those of a geological nature have been sufficiently described in treating of the points of resemblance in this respect; and it only remains to notice more particularly those differences in the mineral composition and character which have not been so fully stated as they deserve.

In respect to the mineral ingredients, the two substances, felspar and hornblende, occur abundantly in both divisions; but, in granite, quartz and mica are very common and conspicuous, whereas, in the rocks of the trap family, they are exceedingly rare. In the latter, compact felspar is also a very common mineral, but it occurs in granite rarely and in small quantities, apparently rather as an accidental than an essential substance. Augit, also, and hypersthene, which I have pointed out as ingredients in some of the trap rocks, have not hitherto been found in any varieties of granite.

With regard to the several rocks of the trap family, it has been a principal object of this paper, to show that greenstone, basalt, and even claystone, occurred as varieties of granite. It is yet uncertain, as before remarked, whether porphyry may not also, in some cases, be a member of the granite family; but, whether it be so or not, the limits between the two are often very evanescent. In granite, however, no instances have yet occurred of substances resembling clinkstone; and if the amygdaloidal structure never occurs in that rock, that circumstance is easily explained by the peculiar conditions necessary for the production of that cavernous structure from which the amygdaloidal seems to arise. That a large proportion, at least, of the amygdaloidal nodules, are the result of a subsequent infiltration, is proved by circumstances which I have stated in other places, but into the details of which I cannot here enter.

In comparing, finally, the mineralogical differences of these two classes of rock, it must be observed, that they consist more in the relative proportions of the several varieties in each, than in the different characters of those members, separately considered. In granite, the well-known compounds to which this name is generally applied, abound almost to the exclusion of those which have here been described, and that resemble the rocks of the trap family. In this latter division, on the contrary, greenstone, basalt, and claystone, are among the prevailing substances; while the compounds that resemble granite are very rare.

But, that too much stress may not be laid on the differences which have here been pointed out, it is proper to remark, that the several members of the trap family, differ as much among each other, as the whole, collectively taken, differs from the rocks that rank under granite. Even in comparing the individual members, the contrast between the softest claystone and the syenite of Sky, or between that simple rock and the numerous porphyries which are found in this family, is as great as that which exists between the same substance and granite.

It is not one of the objects of this paper, to protract this examination of the analogies and differences between granite and the trap rocks, further. To enter more deeply on the discussion, would require a space exceeding the limits assigned for it; since it would be necessary, among other matters, to point out all the circumstances in the origin of both, and all the probable causes which, in either, might have produced those appearances which are not at all to be found, or exist more rarely in the other. It is sufficient to have indicated some facts, hitherto unknown, which add a mineralogical resemblance to those formerly acknowledged to exist between two classes of rock so remote in origin, and to have given a brief sketch of the other circumstances of analogy which were required to illustrate the main object for which these facts have been brought forward. Those who may hereafter examine this question as a matter of geological theory, and as connected with the causes which have influenced the dispositions and the characters of the rocks that constitute the visible portion of the earth, will thus be furnished with additional facts from which to reason.

But if, in the present state of geological science, the collection of facts is necessary, it is not the less incumbent on the observer, to reflect on the main object to which all such facts are destined, and to keep steadily in his view the great purposes of all such investigations, namely, the establishment of analogies, and the discovery of those causes, a knowledge of which is no less useful as a guide to our inquiries, and as forming an indispensable part of the science, than it is an invincible desire in all inquiring minds. In concluding this communication, therefore, I shall, in the briefest possible manner suggest those reasonings which the facts in question appear to indicate.

The arguments by which the igneous origin of the trap rocks is supported, are so well known that they do not require to be repeated; were it even one of the objects of this paper to enter on the general merits of this question. That doctrine is now indeed so universally received among all those who have shaken off the bondage of authority, and who have both the capacity and the inclination to observe and to reason for themselves, that it may be considered as established. But the phenomena displayed by granite, although, in the most essential points, resembling those which occur in the trap family, have as yet failed to produce the same general conviction with regard to the igneous origin of that rock. Into the arguments of a geological nature by which this doctrine may be supported, I shall not here enter: it is sufficient that the chief points have been indicated in a preceding part of this paper. Limiting the present remarks to deductions from the corresponding mineral characters of trap and granite, which have here been pointed out, the following conclusions appear justifiable.

It is found that many important points of resemblance occur between the mineral composition, the texture, and the general structure and disposition, of many rocks in the trap family and others in the family of granite. More particularly, it has been pointed out, that, among the former, there occurs a compound in no way differing from one of the most abundant varieties of the latter. As the trap rocks are admitted to be of igneous origin, it may be inferred that the same cause which has produced the granitic variety of this division, has also operated in the production of the corresponding substance in the family of granite. Again, it has been shown that in granite there occur varieties, in no way differing from some of the prevailing rocks in the trap family. Thus the analogy between the two is still more closely drawn, and thus also it may be inferred, that if in the latter, these rocks must be attributed to an igneous origin, in granite also they have originated from the same cause. And if, in this rock, it be admitted that any of the varieties have an igneous origin, it is not easy to see on what ground it can be denied to the remainder.

To complete this analogy, it would have been necessary to inquire, to what circumstances it is owing that the compound, which is among the most rare in the family of trap, is the most abundant in that of granite; and that, on the other hand, some of the most rare varieties of granite are abundant in the trap family. But this inquiry is too speculative for the purposes of the present paper, and of too discursive a nature to be admissible within the limits to which it is restricted. Those geologists who have turned their attention to this subject, will find no difficulty in pursuing that train of reasoning which the author of the present communication thus leaves in their hands.

J. M.

June, 1820.

Art. V. *On the Employment of Common Salt for the Purposes of Horticulture. By* Samuel Parkes, F.L.S., &c.

[This Essay, extracted from the Horticultural Memoirs of Edinburgh, was rewarded by the Prize Medal of the Caledonian Horticultural Society for 1819.]

As a science, *Horticulture* is comparatively but of a modern date. It was unknown both in Greece and in ancient Rome; for in all the accounts which we have of the baths, the grottos, and the aqueducts, which were considered so ornamental to their cities, there is, I believe, nothing described which conveys any idea whatever of our modern gardens. The Britons, like the Romans and the ancient Germans, made use of herbs and fruits; but, according to Strabo, they were such as grew in the fields and woods, without cultivation. Indeed it has often been questioned, whether the hanging-gardens of Babylon, of which so much has been said, were not more for the display of an original kind of architecture, or for the ostentatious exhibition of ornamental and expensive sculptures, and enormous idols of gold and silver, than for any purposes of real utility.

Even in the Augustan age, when the wines of Italy were in general estimation, little was known of the true method of cultivating the vine, as appears from a story which is recorded by Pliny. He relates that a celebrated grammarian, who lived in the reign of Tiberius*, bought a vineyard, which had been so much neglected by its former owner, that it had become almost barren; and that when, by care and attention, he had rendered it fruitful, his neighbours, who had no idea that trees could be so im-

* In a century or two after this period, it is probable that the Romans had acquired more knowledge of the management of vineyards; for we read that, about A. D. 278, the settlers in Britain, finding that some parts of the island were not unfit for vineyards, obtained permission from the Emperor Probus to plant vines here, and make wine from their produce.

proved by cultivation, and whose vineyards had always been much less productive, propagated a story that he had procured such unusual crops by the arts of magic and sorcery *.

It likewise appears from a variety of testimony, that the ancients were equally ignorant of the methods of rearing shrubs, herbs, and plants. Such of these as were cultivated, were preserved merely for the purposes of medicine; and though the medical professors had this stimulus, their knowledge of varieties seems to have been very limited. Theophrastus, a writer of great credit, who carefully collected plants as well as minerals, and who collected not only those of Greece, but travelled in Egypt, Ethiopia, and Arabia, for the improvement of science, was able to obtain only 600 species. M. Rollin, however, tells us, that when, by order of Pope Nicholas V. in the middle of the 15th century, a translation of the work of Theophrastus was printed, the physicians of that day, perhaps the only class of men who attended to the orders of plants, were so dissatisfied with the narrow limits of botanical knowledge, that resolutions were taken to go in quest of it to the very places whence Theophrastus and others of the ancients had written. He adds, that in consequence of these decisions, voyages were made to the islands of the Archipelago, to Palestine, to Arabia, and to Egypt; and these expeditions were attended with so much success, that in the beginning of the 16th century, the learned were in possession of the description, not of 600 only, but of more than 6,000 plants, with engraved figures of each †.

It seems, however, that Botany did not obtain much of the appearance of a science until the beginning of the last century, when Louis XIV. with the munificence becoming a great prince, commissioned Mons. Tournefort to make a botanical excursion through many of the provinces of Asia and Africa, to collect plants, and to make observations upon natural history in general. This great man received the king's order in the year 1700, and although he was driven home in 1702, by the fear of the plague which then raged in Egypt, he brought

* *Pliny*, lib. xiv. c. 3.

† *Rollin's History of the Arts and Sciences of the Ancients*, vol. iii.

home so many new plants, that he could enumerate 1,356 distinct species, without including any of those which he had collected in his former travels.

The learned throughout Europe were proud of these achievements, and Tournefort was considered to be one of the greatest ornaments of France. In England, however, we had the excellent and eminent John Ray, a man whom we had equal reason to value and admire, who indeed rather preceded Tournefort, and was equally assiduous in his endeavours to promote the knowledge of plants. In consequence of the exertions of this great man, and of the methodical arrangements which he had formed of the vegetable kingdom, together with the subsequent labours of Boerhaave, Linnæus, Hudson, and others, botany, about the middle of the last century, assumed a distinguished rank among the sciences of Europe.

Such are the fruits of industry, when directed by taste and by the energies of an enlarged mind; but the discovery and arrangement of new plants were not the only benefits that were achieved by the exertions of a succession of great men, all directed to the attainment of one important object; for with the knowledge of plants, the want of gardens increased*; and as these became more common, the public gradually acquired a taste for planting, until the desire of possessing a garden became general throughout Europe.

The changes which this produced in society were many and important; and, I have no doubt that, a person now travelling through Europe, and making this one of the objects of his inquiry, would find the character of each people more or less favourable, according to the degree in which a taste for gardening prevails among them. Were I asked to enumerate the

* I am aware that there were gardens in Great Britain before the Norman Conquest, belonging to the monks, but the inhabitants in general had not this useful luxury. There were also large vineyards here in the 12th century. William of Malmesbury says, that the grapes produced in the vale of Gloucester were of the sweetest taste, and made most excellent wines, but these were likewise the property of the great barons, the monks, and abbots: for the general inhabitants of the country participated neither in the credit nor profit which was attached to these establishments.

causes which produced that increase of civilization, which has gradually taken place during the last two or three centuries, I should most certainly place the introduction of gardening next to the invention of printing. The possession of a garden has a natural tendency to soften the character of the most ferocious; it attaches a man to home, and doubles the value of his habitation; and whenever its cultivation is engaged in with ardour, it not only affords an innocent means of occupying leisure hours, but it has also the important effect of diverting the attention from all low and unworthy pursuits.

Buffon, the celebrated French naturalist, was so enamoured of his garden, that he erected a pavilion within it, in which he could study with convenience. There he usually retired at five o'clock in the morning, and was then inaccessible. Prince Henry of Prussia named this sylvan retreat the "cradle of natural history." The illustrious Lord Bacon has pronounced gardening to be the "purest of human pleasures, and the greatest refreshment to the spirit of man."

The dissemination of a taste for gardening is, in my opinion, one of the most valuable effects of the establishment of all horticultural societies; and I have no doubt but that, in this way, the Caledonian Horticultural Society will be found to be eminently useful. While addressing the members of this respectable association, I hope I may be allowed to say, that I feel proud of having been enrolled among those whose efforts tend not only to the improvement of natural history, and rural economy, but also to the promotion of moral habits and propensities. Penetrated with these feelings, I shall greatly rejoice if the following observations and collection of facts, upon a subject in which the public seem now to take considerable interest, should in any degree excite a general desire in others to further the important objects of the Society.

The subject which I have now chosen for discussion and investigation, is the application of *Common Salt to the purposes of Horticulture*, the several branches of which I propose to consider in the following order:

1st. That common salt, when applied in due proportion, has the effect of promoting the health and growth of vegetables.

2dly. That it has the property of rendering fruit trees and esculent plants unfit for the food or the habitation of worms and insects.

3dly. That common salt is one of the most efficacious substances that can be employed in a garden for the destruction of worms and insects; and

4thly. That common salt may, with material advantage, be likewise used for the destruction of weeds, or other noxious vegetables.

Under the first division of our subject, it is to be observed, that the celebrated Dr. Darwin, when treating of common salt as a manure for land, asserts, that this substance "is a stimulus which excites the vegetable absorbent vessels into greater action than usual, and that in a certain quantity, it increases their growth, by enabling them to take up more nourishment in a given time; and consequently, to perform their circulations and secretions with greater energy." Sir Humphry Davy, from what he says in his *Agricultural Chemistry*, seems, on the other hand, to think it also probable, "that common salt acts as a manure, by entering into the composition of the plants, somewhat in the same manner as gypsum, phosphate of lime, and the alkalies."

These opinions will be thought to have great weight; but as few persons, comparatively speaking, will be able to confirm them by their own experience, in consequence of the very limited attention that has hitherto been bestowed on the use of salt in horticulture, the more useful way, perhaps, of treating, this subject, will be to lay before the Society the evidence of those practical men, who have already published the results of their experiments, and then to draw such conclusions as their communications may seem to justify.

Dr. Brownrigg, who, in the year 1748, published a valuable work "*On the Art of making Common Salt*," makes the following statement:

"Salt," says he, "contributes greatly to fructify the earth, and when properly used as a manure, affords ample nourishment to corn and other vegetables, and renders kingdoms rich and fertile, where it happens to abound in the soil." p. 158.

Mr. Hollingshead, a gentleman of considerable fortune, who resided near Chorley in Lancashire, and spent many years in making experiments on the application of common salt as a manure, and who also made powerful efforts to obtain a repeal of the salt laws, published a few years before his death, a very interesting pamphlet on the subject. In this work, to which I am greatly indebted for much useful information, he relates, that " when foul salt was permitted to the farmers duty-free, a person near Middlewich in Cheshire trenched his garden in autumn, mixing with the soil a quantity of foul salt. The following spring, it was dug or delved in the usual method, and planted with potatoes. The crop produced therefrom was such as far exceeded his most sanguine expectations. Twenty of the potatoes were produced, which weighed sixty pounds."

Several other testimonies to the beneficial effects of common salt in the culture of the POTATO might be produced, but I recollect none so decisive as that of the Reverend Dr. Cartwright, which is published in the fourth volume of the *Communications to the Board of Agriculture.*

Having previously prepared a piece of land for the experiments, on the 14th of April 1804, a portion of the land was laid out in beds of one yard wide and forty yards long, twenty-four of which were manured in different ways ; one of the beds had no manure, and fifteen of the beds had salt put upon them, in the proportion of a quarter of a peck to each bed. On the same day the whole was planted with potatoes, a single row in each bed ; and that the experiment might be conducted with all possible accuracy, the same sets were planted in each bed. On the 21st of September, the potatoes were taken up, and the produce of each row was accurately ascertained ; from which it appeared, that in every instance excepting one, where the salt was used, the crop was found to be superior ; so that, of ten different manures, most of which are of known and acknowledged efficacy, salt proved superior to them all, one only excepted, *viz.*, chandlers' graves; and that bed in which salt and soot were combined, produced of all others, the *best* crop. But the most singular circumstance, and that which has induced me to submit the relation of this experiment to the Society, is, that where salt was used, whether by itself or in combination, the roots were entirely free from the scabbiness to which potatoes are often liable, and from which none of the other beds were altogether exempt, although there were in the same field nearly forty beds of potatoes, besides those which were planted for the sake of these experiments.

In the culture of the TURNIP, salt is also very efficacious. In the twenty-seventh volume of the *Annals of Agriculture* is a paper communicated by Davies Giddy, Esq., President of the Penzance Agricultural Society, which contains an account of some very important experiments on this subject. At Michaelmas 1790, Mr. Sickler, a member of the Society, entered upon an estate, so much impoverished by the former tenant, as scarcely to return the value of the seed. In the spring of 1791, Mr. Sickler prepared two acres for turnips, which had borne seven crops of oats in succession. The last crop did not produce nine bushels on an acre. In the first week of April, the earth from the ditches was carried into the field, and laid in four piles; each received three cart-loads of sea-shell sand, and five bushels of salt. The earth from another ditch, chiefly consisting of the decayed soil, which had been taken off the ground in former tillage, was placed in three more piles, and each of these received also three cart-loads of sand, but no salt, on account of the apparent richness of the earth. Half the field was manured with the four first piles; but the three last not being sufficient for the other half, what remained without manure was sown with salt, at the rate of ten bushels to an acre.

That part of the field where salt had been used, either mixed with earth or alone, produced about half a crop of turnips, but the crop totally failed where there was no salt.

In 1792, three acres, which in 1791 had borne a crop of wheat, not exceeding twelve bushels on an acre, were ploughed before Christmas, and brought into fine tilth by midsummer following. On each acre were sown twenty bushels of salt, excepting that two ridges towards the middle of the field were purposely left without any salt; on these two ridges the turnips totally failed, but the remainder of the field produced a plentiful crop.

In 1793, four acres of land, completely worn out by successive tillage, were ploughed before Christmas; three acres were sown with salt, at the rate of twenty-five bushels, and the remaining acre with eighteen bushels, without any other manure. The crop was in general a good one, but visibly best where the greatest quantity of salt had been used. Since that time, crops of turnips have been raised, with equal success, by the use of salt; and in the severe winter of 1794-5, it was observed that these turnips were much less injured by the frost, than others similarly treated and cultivated in the common way. The writer of the account suggests, that if turnips are less injured by frost when they are manured with *salt* than when they are cultivated in the usual manner, it must indicate an extraordinary degree of health and vigour in the plant; but a single observation is insufficient to establish such a fact.

The free use of salt, in the culture of the CARROT, has also been found very efficacious. The effect of enlarging the growth and consequently increasing the crop of all esculent vegetables, has long been known to all the gardeners in America. Sir John Sinclair likewise informs us, that drilled carrots grow well in a salted bed, the salt being laid under the surface, in the centre of the intervals between the rows, and at some distance from the roots, in such manner, that it may be dissolved before the fibres of the roots meet it. See *Husbandry of Scotland*, second edition, vol. ii., Appendix, p. 182.

Some years ago, Baron Humboldt discovered that a weak solution of any of the oxymuriatic salts has the property of accelerating and increasing the growth of vegetables. This effect is probably owing to the circumstance of the oxymuriates being converted by exposure to the air into common muriates. It might, however, be within the scope of your Society's plan and intentions to offer premiums to such gardeners as would willingly make farther experiments on bleachers' residuum, an article which may be had for little or nothing, and which, if divested of the sulphate and muriate of manganese, which is always contained in it, would doubtless prove a very powerful and beneficial manure.

A gardener of considerable celebrity at Chorley in Lancashire, of the name of Beck, made use of common salt in his extensive gardens for upwards of thirty years, especially upon his ONIONS, and he found that the application of this salt very far surpassed that of all other manures. He never took any care to ascertain the exact quantity of salt which he employed; but when he was questioned as to this point, he said, that he thought he was accustomed to use it in the proportion of about sixteen bushels to an acre of land. His practice was to sow the salt immediately after he had covered in the seed, a point which should always be attended to, because it has been found, that, if the salt be sown after the plants show themselves above ground, the whole crop will inevitably be destroyed. On the contrary, if a moderate quantity of salt be sown upon the land as soon as the onion seed is deposited in the ground, say about six pounds to one square perch of land, or four ounces to a square yard, the result will not fail to be striking and advantageous.

The general failure of the onions last year has been much spoken of, but I do not hear of a single gardener that employed salt who had not a very abundant crop. As a corroboration of this, I may refer to the letter of Mr. William Morton of Biel, which was read to our Society on the 8th of September last, and which states the benefits he had derived from the use of brine, made by the solution of common salt in water, and which he had applied to his beds of onions, shallots, and other roots. I shall, however, have occasion, before I conclude this address, again to refer to Mr. Morton's letter.

Seeing that common salt produces such striking effects in the culture of potatoes, turnips, carrots, onions, shallots, *&c.*, I cannot help being surprised that it has not been brought into general use long since, especially as I observe, that more than 200 years ago, the Lord Chancellor Bacon, in the most unequivocal manner, recommended its employment in the practice of horticulture. His words are these: "Several herbs, such as radish, beet, rue, pennyroyal, like best being watered with salt water; and I advise the extension of this trial to some other herbs, especially those which are strong, such as mustard, rocket, and the like.—*Lord Bacon's Natural History.* I must, however, now proceed to the consideration of the effect of salt in the cultivation of fruits.

The action of common salt upon FRUIT-TREES, when judiciously applied, is equally beneficial. In cider countries it has been the practice on some estates, where the owners have been ambitious to have fine orchards, to dig a small trench a few yards distant from each apple-tree, and to put within it a small quantity of salt, which, by means of the rain, becomes dissolved, and is gradually conveyed to the roots of the trees. This practice is said to increase the quantity of the fruit, and to preserve the trees in the utmost health and vigour.

Mr. Hollingshead, whom I have before mentioned, and who studied this subject for many years, remarks, that "Those farmers who reside near the sea-shore, might derive considerable advantage from watering their grounds with sea-water, or sowing them with sand from the beach, below high water-mark, during the spring and autumn, as the particles of salt contained therein would be a great benefit. Fruit-trees," says he, "and the hop plant should also be sprinkled with sea-water, or have salt or sea-sand laid about them at some distance from their stems. The cotton-tree and sugar-cane, in the West Indies, would also derive considerable advantage from this mode of treatment." Page 21.

There is a very striking experiment on record, which was made by the late Mr. Gilbert, steward to the late Duke of Bridgewater, on the effect of common salt upon apple-trees; and from my own knowledge of that gentleman, I have no hesitation in saying, that I believe the account may be strictly relied upon. This gentleman, who was not only steward to the Duke, but also a large salt manufacturer, had an estate contiguous to his salt-pits at Wincham in Cheshire, on which was an orchard planted with apple-trees, which, being grown old, constantly bore in the spring a profusion of blossoms, but never brought any fruit to perfection. To remedy this defect, the tenant spread a quantity of rock-salt, bruised small, about each of the trees, at some distance from their stems; and ever since that period all the trees in that orchard have continued to be very productive, yielding abundance of fine, large, and well-flavoured apples.

A merchant at Liverpool, with whom I am well acquainted, has sent me an extract from a letter which he received from a very respectable correspondent, on the state of the fruits in the gardens at Droitwich, a town in Worcestershire, which is one of the most considerable places in Great Britain for the manufacture of common salt. It runs thus:

"It is a remarkable circumstance, and worthy observation, that about the 15th of July, when the small fruit began to fail, and become scarce in the markets, in consequence of the great drought, the fruit in the gardens at Droitwich had not the least appearance of the want of rain, but, on the contrary, was in a state of the greatest possible luxuriance; and I am certain I speak within compass, when I say I could have gathered hundreds of clusters of currants that would have weighed half-a-pound each. The stems of the bunches were so long and numerous in the clusters, and the currants so large, that I remarked to my children who were with me, I was convinced their appearance, so different from every other place at the same time, arose from the presence of salt in the atmosphere, occasioned by the boiling of so many pans at the salt-works here."

In addition to these facts, I am desirous of remarking, that the employment of common salt in agriculture and horticulture is much more frequent in foreign countries than it is in these kingdoms; for I have the most unquestionable authority for stating, that "salt is employed in the cultivation of the vine and other fruit-trees on the borders of the Rhone, and that they are improved by this application."

Most of the persons who have borne testimony to the beneficial effects of common salt in horticulture, have observed, that salt has the property of attracting moisture from the atmosphere, and hence it is possible much of the important results may be derived. It is probably owing to the property which salt has of absorbing moisture, that it is customary, in bringing the cuttings of curious *vines from abroad,* to dip them in salt water before they are put on board. I have indeed been assured, that cuttings of the myrtle and other shrubs may be brought from a distance, with more certainty of their living, if they be previously dipped in a solution of common salt. Cuttings of the weeping-willow, the Salix Babylonica of Linnæus, which is a native of the East, could never be brought into this country alive, until the expedient of steeping them in salt water was adopted.

Requesting to be forgiven for these digressions, I shall conclude this branch of the subject in the words of a late venerable writer, who had probably made more experiments on the effects of common salt in horticulture, than any other individual in Great Britain. "Every thing," said he, "that is sown or planted in a garden or hot-house, should have a quantity of salt sown on the surface of the ground round it. By thus regularly forcing vegetation with salt, all the productions of the field and garden would be brought to maturity three weeks or a month sooner than they are by the present method of cultivation, as well as the various grains being much improved in weight and solidity, and the fruits in richness and flavour*." Sir John Sinclair, in quoting this passage, remarks, that "the advantage which is derived from the application of Dutch ashes, (so full of saline particles,) to the gardens in the Netherlands, is a full confirmation of this doctrine."

The SECOND property which I have assigned to common salt, when employed in the cultivation of a garden, is that of rendering esculent plants and fruit-trees unfit for the food or the habitation of worms and insects. Upon this, and the remaining branches of the subject, I must, however, be very con-

* *Hints to Country Gentlemen, &c.* By John Hollingshead, Esq. Third Edition, p. 19.

cise, else I shall extend this paper to too great a length to be read at a single meeting of the Society.

The farmers who reside in the counties near the metropolis, and in several other districts in England, never put their seed-wheat into the ground until they have first steeped it in a very strong solution of common salt, as they find this to be a specific against the rust or blight in wheat, and that it prevents insects from preying upon the seed. As this practice is so efficacious in preserving seed-corn, why should it not be adopted with garden seeds, such as those of onions, carrots, turnips, radishes, celery, parsley, and the like?

The HONEY-DEW, which every year makes great havoc with fruit-trees, is, I believe, occasioned by small insects; and this may be entirely prevented from appearing by strewing the borders where the trees grow with common salt. Ants never appear in those parts of a garden where salt has been properly strewn; and how destructive these little animals are to trees, as well as to fruit, is well known. I have no doubt but that the fly in hops might also be prevented by the proper use of common salt.

Last year a gentleman called upon me from the Cape of Good Hope, to ask me if I could contrive any method of destroying an insect which attacks the vines in that colony, and produces incalculable mischief. He informed me that this is a peculiar insect, about the size of the millepedes, or common wood-louse, which creeps up the vines, and does so much mischief, that some plantations are rendered quite unproductive by it. Every crop would indeed be entirely destroyed, were it not that the proprietors of the estates keep a great number of women and children to pick off these vermin. These singular insects burrow very shallow in the ground in the day-time, say half an inch under the surface, and in the evening they come up upon the trees. The female slaves and their children go every night to the proprietor, carrying with them in their hats the produce of their industry, which he examines separately, and then empties it into a tub of water, which stands by him for the purpose. The slaves and children are then rewarded according to their deserts, and the quantity of insects which each brings in, while the careless and indolent are proportionably punished. My informant assured me, that the ravages of these insects, the great number of hands that are required to destroy them, and the high price of labour at the Cape, have prevented the cultivation of vines, and the consequent improvement of the colony, more than any other circumstance. To extirpate these creatures, I advised salt to be spread upon the surface of the ground in which the vines are planted, and I am promised an account of the result of the experiment. Should I receive this, I shall not fail to communicate it.

It is not a mere speculation that common salt will prevent the ravages of worms and insects in gardens, for it has so often been tried by gardeners of experience, that no doubt can remain on the subject. More than fifty years ago, Mr. Thomas Hitt, who was gardener to Lord Robert Manners at Bloxholme in Lincolnshire, and afterwards to Lord Robert Bertie, at Chislehurst in Kent, published a very interesting work on *the Management of Fruit-Trees*, in which he gives a variety of directions for the use of common salt, founded upon the experience of many years' practice. This work is written with so much modesty, and is throughout so totally unassuming, that one feels inclined to receive his testimony without hesitation. The following brief extracts will, I trust, be interesting to the Society.

" I have," says he, " observed two sorts of CATERPILLARS feed upon fruit-trees, the one black, and the other green; the *black* generally make their appearance in March, if the season be dry, upon the pear-tree, apple, and several others. The green caterpillar, that feeds upon fruit-trees, for ought I know, may be the same as those that were black at their first appearance, but by green food their colour may be changed; but I have found them very prejudicial to both the young branches and fruit of the apricot, cherry, plum, apple, pear, currant, gooseberry, *&c.* When the caterpillars are first perceived upon wall or dwarf trees, I have prepared a brine, the same as for washing of walls at the time of pruning, and therein dipt a brush or besom, and swept the trees all over; this has destroyed many by beating some off and killing others. This should be often repeated in dry seasons." Page 266—269.

On preserving fruit upon standard-trees from being destroyed by caterpillars, he remarks, that "as most noblemen have, at their seats, engines for extinguishing fires, which are very proper instruments for watering orchards, or such trees as cannot be reached with a brush; if orchard trees are watered all over with these engines two or three times a week, it will destroy many of the caterpillars. This should be done in the heat of the day, for then they hang the loosest upon the trees; and the water should be mixed with salt. This work is not only necessary when the trees are in blossom, but also before and after." Page 272.

"The HONEY-DEW," says he, "is a glutinous substance, very prejudicial to many kinds of fruit-trees, for it contracts the minute vessels of their most tender parts, and prevents their imbibing and perspiring such fluids as are required in vegetable life. A few days after the honey-dew appears, you may discover small insects on the underside of the leaves that are shrivelled, almost without motion; yet the heat of one fine day will make them visibly increase both in bulk and strength, and likewise in number." He adds, the honey-dew, "retards the motion of the sap at the extremity of the branches, and this prevents the fruit below from coming to any tolerable perfection, and damages the young branches to such a degree, that they are never after capable of bearing good fruit. Besides, many trees are entirely killed thereby, if proper methods are not used to prevent it. Though different kinds of SMOTHER-flies, or those of different colours, are found upon different sorts of trees, yet as they are all either bred from, or feed upon the honey-dew, all trees require the same care and management, to preserve them from these evils; for no tree prospers well when either the honey-dew or smother-flies are on the extremities of its branches."

The remedy which he proposes for these evils is nothing more than common salt, administered in the following manner: "If the season be wet, spread common salt all over the border, about eight ounces to each tree; for the more salts the juices contain which form the young branches, the more compact and smooth their leaves will be, and thereby less subject to the penetration of the honey-dews. If trees are thus ordered at all times, when the honey-dew appears on them, neither it nor the flies can ever do them much injury." The foregoing paragraphs are taken from the chapter directing how to treat trees in *new* borders. In that " of the honey-dews and smother-flies on fruit-trees growing in *old* borders," he has the following remarks: " If the borders be impoverished, by having either too much kitchen-stuff or flowers growing upon them, the trees will be too weak; and if the weather be dry, they must be watered plentifully three times a week, with one ounce of salt added to each gallon of water. If the fly be strong, double the quantity of salt, and water the bottom of every tree before the soot or lime is laid on at the time of trenching; but if there is not an opportunity of trenching, nevertheless water thus mixed (with salt,) must always be used for the above purpose."

" I have found these methods successful, even when the flies have been very strong upon the trees, and have in a few days destroyed many of them, and caused the trees to shoot vigorously." In obstinate cases, he directs to dissolve two ounces of salt in a gallon of water, and with this mixture to brush the trees all over, beginning at the bottom of the tree, and making all the strokes upwards. This, he says, will cause all the infected leaves to drop off the trees, but will not injure the healthful ones, but occasion the trees to make good shoots after, even such as will produce fruit the next year on peaches and nectarines." Pages 279—281.

On the destruction of fruits by ANTS, this interesting author gives the following important directions: " The ants," says he, " are much complained of for destroying fruit and leaves; but when borders are rightly prepared and ordered they cannot live; nor in old borders, after they have been trenched and watered with the composition mentioned for that purpose. Against old walls, either of brick or stone, they are the most troublesome, for as they lodge in the nail-holes, the watering of the borders only has no effect upon them: but the *walls* should be watered all over with brine, made by adding two ounces of salt to a gallon of water." Page 282.

During a journey in the summer and autumn of last year, through the north of England, and part of Scotland, I heard repeated complaints of the failure of the onion crops, which were said to be destroyed by the wire-worm. This was more particularly the case around Edinburgh, and throughout the county of Fife. Letters from home also informed me, that in the neighbourhood of London onions were so scarce for a month or two, from the same cause, until foreign onions were obtained, that they were sold in Covent Garden market nearly as dear as peaches. It gave me, therefore, much pleasure, happening to be at Edinburgh at the Anniversary Meeting of our Society, to hear the communication from Mr. Morton, a gardener in the neighbourhood of Dunbar, who informed us by a letter, directed to the Secretary, that he had preserved his crop by the use of salt water, while those in the gardens around him were all destroyed.

Thirdly, common salt is not only a preservative of plants and trees from the ravages of grubs, worms, and insects, but it is one of the most effectual substances that can be employed in a garden for the destruction of these animals themselves. Of the truth of this assertion any one may satisfy himself in a very short time by direct experiment. If a small quantity of salt be sprinkled upon a common earth worm, its destructive effects will be seen to be almost immediate. Its action on worms is also very strikingly exemplified by its effect on the hirudo, or common leech. When this creature has been employed in supplying the place of the lancet, it is usual to put a small quantity of salt upon it, so as to touch its mouth; this occasions the leech instantly to disgorge all the blood into the plate on which it is laid, but if too much salt be used, or if the leech remain in contact with it too long a time, the salt is apt to prove fatal: hence some of the people who bleed with leeches, prefer taking the blood from them by pressure, rather than risk the loss of them by using salt. The Right Honourable Sir John Sinclair, in a valuable paper which he has lately published, thus explains the operation of the salt. " Salt," says he, " destroys vermin in the ground, by making them void the contents of their bodies, such evacuations being too powerful for them to withstand. It has," he adds, " this additional advantage, that the vermin thus become food for those very plants which otherwise they would have destroyed."

The eminent John Evelyn, the celebrated author of *Sylva* and other interesting works, and who himself was very zealous in the improvement of the art of horticulture, had learned the effect of common salt in destroying slugs, worms, and other creeping vermin, as appears from a paper in the first volume of the *Practical Husbandman and Planter*, 8vo. 1733, page 58; but it does not appear that he had regularly employed it for that purpose.

From an *Essay on Plantership*, published by Mr. Samuel Martin of the Island of Antigua, it appears that common salt has been employed in the West India Islands for the destruction of grubs and insects. " Soils," says he, " which are subject to the grub, and must be fertilized by common dung, which is a proper nest for the mother beetle to deposit its eggs, should be well impregnated with the brine of dissolved salt, after the dung is first cut up; two large hogsheads of salt will make brine enough for a dung-pan of fifty feet square. This cure for the grub is a late discovery, for which I am obliged to a judicious planter, and which I have tried with success."

" A land-surveyor of high character in my neighbourhood," says the Right Honourable Lord Kenyon, in his evidence delivered before the Board of Trade, " considers that the use of salt would be likely to be very valuable in destroying the slug, wire-worm, snail, *&c.*, which often destroy even whole crops. He also well remembers that salt was used largely in the neighbourhood of the higher and lower Wiches in Cheshire, before the duties were raised to their present height."

This is confirmed by a writer in Dr. Rees' Cyclopedia, under the article " Salt," who says that " in Cheshire and other counties, they make a great use of the water of their salt springs as a manure for their lands." He adds, "They let out the water of these springs for a certain time upon the lands, after there has been rain, and by this means the quantity of salt they contain is so blended with the rain water, that it is too weak to hurt the corn or grass, and yet strong enough to kill worms and other vermin, and to improve vegetation."

The FOURTH property which I have assigned to common salt, when employed in horticulture, is that of destroying weeds and other noxious vegetables. On this part of the subject the evidence is not so abundant as I could have wished; the following testimonies however do, I think, deserve attention.

The author of an essay on the effect of salt on vegetation, published in the first volume of the *Practical Husbandman*, before quoted, expresses himself thus: "I am well assured from a Scotch gentleman, that they have long used salt in that part of Great Britain, always sowing ten or twelve bushels by hand of their coarse salt, on an acre of young green wheat, some time in November, December, January or February; it being, from the several accounts which I have had of it, very effectual in the killing of tender weeds amongst corn, yet at the same time cherishing the corn, and adds much to the goodness and plumpness of the grain." Page 48.

Bishop Watson, in his *Chemical Essays*, says, that "in Cheshire, wherever the soil abounds with rushes and weeds it is customary to lay a quantity of rock-salt upon it to destroy them." Vol. ii. p. 73.

Gervase Markham, the well-known writer on rural affairs in the middle of the seventeenth century, strongly recommends the use of salt as a manure for land, in his book entitled "*A Farewell to Husbandry*," and concludes his observations by remarking, that "there is nothing which killeth weeds and other offences of the ground so much as saltness."

Major John Taubman, speaker of the House of Keys in the Isle of Man, in giving his evidence before the Board of Trade, in the year 1817, states, that "he has used refuse salt as a manure on meadows, with advantage; it was sown thinly by hand,—cannot speak to the quantity used; the meadow had been much covered with moss, which the dressing of salt entirely destroyed."

"Mr. Sickler made a little heap of earth in the midst of a field, on the top of which a cart-load of refuse salt was thrown; the earth in the heap itself, and, after its removal, the earth under it for upwards of two feet deep, to the clay was rendered so perfectly barren, that the most common weeds would not vegetate in it. This barren earth, however, furnished the richest dressing for the remainder of the field*."

I have now laid before you all the evidence which I have been able to obtain on this part of the general question,—the use of sea-salt in horticulture. I am, however, fully sensible that, although enough may have already been proved for us to form the decision, that the use of salt in gardening is essential, there are probably many well established facts which have not yet come to my knowledge, and from what we have already attained, we may presume that our information on the subject is yet very limited.

To employ this very valuable mineral substance in the best possible way, much is to be acquired by practical knowledge, by direct experiment, and by vigilant observation. Every distinct vegetable, whether in the state of seed, root, or more mature growth, from the plant to the largest fruit-tree, may possibly have its distinct habitude and peculiarity. Some may require more, others less; some may admit of an immediate application, while others require the salt to be laid on at a little distance. In short, it is obvious that, since the general benefit of the practice which I have endeavoured to impress upon your notice has been substantiated by experience, we have now nothing more to follow than experimental researches.

As a manure for land, sea salt is considered of so much importance by the Board of Agriculture in London, and by the Highland Society of Scotland, that both these associated bodies have offered premiums for experiments on the subject. The offer from the Board of Agriculture is announced thus: "To the person who shall make and report to the Board, the most

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* *Case of the Salt Duties*, by Sir Thomas Bernard, Bart., page 275.

satisfactory experiments to ascertain the advantages or disadvantages which have attended the use of salt as a manure, either simple or mixed with other substances;—The gold medal or fifty pounds. Accounts to be produced on or before the 1st of March, 1820." The Board adds: "It is to be hoped that this premium will excite a laudable spirit among enterprising farmers, to ascertain particulars of such importance to the agricultural interest."

The reward held out by the Highland Society of Scotland is, "To the person in Scotland who shall make and report to the Society the best and most satisfactory experiments on the effects of salt as a manure in general,—A piece of plate of thirty guineas value, or that sum in money. The reports to be lodged with the Deputy-Secretary on or before the 10th of November, 1820."

From the interest which I have long taken in this subject, and the share I have had in obtaining the late act of parliament, for lowering the duty upon rock-salt for the purposes of husbandry, I felt much pleasure and satisfaction on seeing these premiums announced to the public; and I am inclined to hope that the late concession of the Legislature will prove the forerunner of a total repeal of all the existing laws relating to salt, and that the offer of these premiums will occasion such a spirit of emulation among the farmers, as must conduce, in an eminent degree, to promote the improvement of agriculture. Greatly do I wish that the Horticultural Societies of London and Edinburgh may attach a proportionate degree of importance to the employment of common salt in their experimental researches, and thence be induced to offer such premiums as cannot fail to stimulate the exertion and attention of all our rational and scientific gardeners, so as to lead their inquiries towards the investigation of this very interesting and curious subject. Should the foregoing collection of facts have the least tendency to invite the Council of the Caledonian Horticultural Society to institute such a prize, I shall derive considerable satisfaction from the circumstance of having suggested a measure so important, in every point of view, to a great majority of persons, of all classes, in the British dominions.

Art. VI. *On the Origin of the Ashantees, and Inhabitants of the Gold Coast of Africa. By* J. F. Bowdich, Esq. *Communicated by the Author.*

The advance towards civilization and the arts, and the numerous exceptions to the negrŏ physiognomy, which astonished me on penetrating to Ashantee, when associated with the striking similitude of most of their superstitions, laws, and manners, to those of the Egyptians, naturally excited interesting speculations, and induced me to use my earliest leisure in consulting those classical authors upon such subjects, whose descriptions my memory could but imperfectly recall.

The traditions of emigration, not of the whole population, but of particular families, so current in Ashantee, and the neighbouring nations, persuade me that they are native Ethiopians, mixed with settlers from ancient Egypt, as the Abyssinians have been recently shewn to be, with the strongest probability, in opposition to a former opinion, of their Arabian descent.

I will not dwell on the subjugation of Ethiopia by Sesostris, but rest principally on the fact mentioned by Herodotus, that 130 years before his time, 240,000 Egyptians emigrated, or rather fled from Psammiticus, and went as far beyond Meroe, as Meroe is beyond Elephantine, or a journey of four months from the latter country. That they presented themselves to the king of that part of Ethiopia, who gave them the lands of some of his enemies, whom they ejected, and that the Ethiopians civilized themselves in adopting the manners of these Egyptians.

The Ethiopians, thus dispossessed by the Egyptians, were doubtless only pressed or removed into the nearest convenient country, and still preserving an intercourse, participated in some degree in the civilization introduced by the emigrants from Egypt. The sweeping expedition of Ptolemy Evergetes, who, by the record of his triumphal monument at Adulis, is known to have subdued nations southward of the sources of the Nile, and others as far eastward, as we presume the present kingdom of Kulla to be, no doubt compelled many Ethiopian tribes or families, inheriting the opinions and customs their ancestors had adopted from the Egyptian emigration, to retire still more westward, from the first alarm of his approach, the the fear of a second invasion, or the apprehension of being spoiled of the half of their possessions, as he records some of the nations he subdued, to have been. We are to recollect also, that the Egyptians had colonies at Tachompso, Meroe, and Gojam; that Ptolemy Philadelphus penetrated, with 500 horse, into the country of the Ethiopians, and founded the city Ptolemais Theron.

Herodotus and Diodorus Siculus, both agree in distinguishing the Ethiopians in some degree civilized, from others who were savages; speaking of the former as having been of themselves a little advanced, and afterwards perfected, in laws and manners by the deserters and colonists from Egypt.

Both authors impress that the genuine laws and customs of these Ethiopians (preserved distinctly from those they had received from the Egyptians,) were very singular, especially as regarded the succession or royalty. It will be seen that such laws and customs of Ashantee, as cannot be assimilated with the Egyptian, are of a very original and extraordinary character, and especially as regards royalty or the succession.

Diodorus says of the Ethiopian savages, that some deposit their dead bodies in the water as the most honourable sepulture, and others in their own houses. Now it is remarkable, that the Jum Jums, spoken of to Mr. Hutchison, as a cannibal nation, adjoining the Niger, far to the eastward, were particularly described as consigning their dead to the river in rude coffins. The Sheekans and Kaylees, and other Anthropophagi, whom I have mentioned and laid down for the first time in the map of Africa, having conversed with individuals of these nations, in the Empoöngwa, or Gabon country, bury their dead in their houses under the beds. It will be seen too in my chapter on Geography, that the Jum Jums of Mr. Hutchison, as laid down in a manuscript Arabic chart which he sent to me; the Yem Yems, the cannibals described to Mr. Horneman as south of Cano, and the Niger; the cannibal nations, behind the river Gabon, (who eat their dead, even their own children, or expose them for sale the moment the breath is out of their bodies,) all occur on these different authorities in the same neighbourhood, if not in the same spot.

There can be no doubt then, that these nations, found almost precisely where Ptolemy has placed his Ethiopes Anthropophagi, are the descendants of the savage Ethiopians of Herodotus.

Having thus separated or disposed of the barbarous Ethiopians, by identifying them with the cannibal nations, still retaining such of their customs as are briefly recorded, and found in the same geographical situation, I will return to the Ashantees, whom I have considered to be the civilized Ethiopians of Herodotus and Diodorus, pressed westward by the Egyptian emigrants, (by an intercourse, with whom they nevertheless acquired the arts, manners, and superstitions, which now astonish us,) and afterwards driven, or emigrating still further westward, by the sweeping expedition of Ptolemy Evergetes.

The Ashantees, and their inland neighbours, must have again been disturbed from time to time by the several emigrations of the Carthaginians, and other nations of the Mediterranean, whom Mr. Buache, in his researches for the construction of a map of Africa, for Ptolemy, has at once discovered, by the identity of the names, in the neighbourhood of the Mediterranean and south of the Niger. The Mimaces, for instance, are laid down by Ptolemy, a little south of Tripoli, and again a little west of the modern Yarriba. The Nabatræ, close behind Algiers, and also where Dahomey now exists. The Dolopes, in the present dominion of Tripoli, and again, where we expect to find the negro kingdom of Kulla. The Blemmyi we find in three places, on the Arabian Gulf; near Rees Ageeg, on the eastern frontier of Abyssinia; and south of the line, a little above the track of the traders from Loango to Nimeamay. Many other instances might be adduced of the same names being found at remote distances north and south of the Niger, whilst other nations, as the Samamicii, on the shore of the Mediterranean, near Lebida, do not appear in Ptolemy's time to have reached the Niger, but to have rested in their progress on the northern frontier of the negro kingdom of Asbex.

As late even as the present time, I found a kingdom, called Takima, on the northern frontier of Ashantee, and another called Tahkema, was laid down by the Moors (who furnished me with the MSS. charts,) between Timbuctoo and Fezzan. The Fantees have still a tradition of their coming from Takima. The expedition of Cornelius Balbus (the last Roman general who had the honour of a triumph,) who reached the Niger, and marched for some time on its northern bank, (apparently where the modern negro kingdoms of Noofee, Yaoora, and Fillani, are now situated,) must doubtless have disturbed many of the colonies and aborigines, and induced movements to the south of the Niger. The previous expedition of Suetonius Paulinus, (who seems to have passed near where Park understood the source of the Niger to be, into the country of the Perorsi, who are placed by Ptolemy between the Gambia and the coast,) must have also contributed to these secondary movements of the Ethiopians. M. La Traille, of the Institute of France, did me the honour to read to me his objections to the alleged extent of this expedition, in a MS. he is about to publish; but I have since been informed by Major Rennell, that it appears from the artless and consistent narrative of Scott, the *English* sailor, (who was, undoubtedly, in Major Rennell's opinion, carried across the lake Dibbir, and whose narrative is about to be published,) that the Sahara, instead of being a continued ocean of sand, is crossed by a belt of firm land, equal to nearly two-thirds of the whole extent. This materially diminishes the difficulties M. La Traille has ingeniously opposed to the expedition of Suetonius Paulinus.

Some may prefer the opinion, that a part of the numerous emigrants and deserters from Egypt, may have penetrated as far westward as Ashantee, as it will explain the coincidence of manners and superstitions equally well; but the identity of many of their more extraordinary customs with those of the Abyssinians, and their own traditions of emigration, incline me to believe that they were once nearer each other.

It appears too, that the Arabs whom Pliny, King Juba, and other ancient writers, affirmed to have settled from Syene as far up as Meroe, have since that time penetrated south-westward into the interior of Ethiopia; for in the accounts and the MS. charts, which I received from the natives, Wadey was always distinguished as the first Arab dominion, and that people were said to use a different diet, and their ambition only to be repressed by the great power of the Emperor of Bournou. This progress of the Arabs inland, must also have contributed to the dislocation of the Ethiopic or negro nations.

The few extraordinary superstitions, which cannot be assimilated to the Egyptians, may be considered for the most part as pure Ethiopic, as is, probably, their original and poetical tradition of the Creation.

That the Ashantee customs may have again been a little diversified by intercourse with the Carthaginian colonies, which settled south of the Niger, appears probable from some habits I have recorded, particularly that of spilling a little liquor on the ground as an offering to the Fetish or Deity, not only in their sacrifices, as we read in the Greek and Roman writers, but invariably on common occasions, a domestic custom which Homer also attributes to the Trojans.

The Phœnicians confessedly made human sacrifices, and "frequently even of those who were most dear to them," although these sacrifices were early discontinued, as well as in Egypt, without our being told why. The Phœnician priests were in the habit of cutting their bodies with knives and lancets; those who pretend to sudden inspiration, (or that the Fetish has come upon them,) in Ashantee, lacerate themselves dreadfully by rolling over the sharp points of rocks, beating themselves, and tearing their flesh with their own hands, so as to present the most shocking spectacles. The Phœnician priests also worked themselves to the height of frenzy by dancing, and the violent exercise of their voices, and then raved or prophesied, as if possessed by some irresistible power. I have frequently seen the Fetish women or priestesses in Ashantee, (and I think I was told that the priests did so too,) dancing or whirling round on one leg until they became stupified from giddiness, yelling and screaming the whole time, and then uttering what was called the voice of the Fetish.

The Ashantees, however, will be found to retain the Egyptian superstitions, laws, and customs, much more perfectly than the Abyssinians; because the latter must have abandoned many on their conversion, as incompatible with their new religion.

First, then, I shall shew wherein the superstitions, laws, and customs, of the Abyssinians and Ashantees still agree; and, secondly, submit the identity of those wherein they do not still agree, with those of the Egyptians, as described by Herodotus and Diodorus.

The following customs will be recognised as Abyssinian. The King of Ashantee is never to be presumed to speak but through his ministers or interpreters, who invariably repeat even his simplest observations, however audible beforehand. He confines himself to the palace, and is invisible to his subjects for several days, twice every six weeks. Before decision in criminal cases he always retires to a secret council. His domestic officers and menial slaves live in a state of familiarity with him unknown to the rest of his subjects. He never eats in public, or before any but his slaves. It is high treason to sit on the king's seat, which is turned upside down the instant he quits it. He distributes gold chains, swords, and bracelets, as the rewards of great actions.

In Abyssinia none inherit the throne with any bodily defect. In Ashantee the most lawless intrigue is permitted to the females of the royal family if their gallants are handsome, with the view of securing the same pre-eminence of person to the heirs of the throne.

The throne of Ashantee is hereditary in one family as in Abyssinia; and I cannot but consider the prefix of Sai or *Zai*, (for it was pronounced both ways, and at first I always wrote it with a z,) to the names of all their kings as extraordinary, when I read the following remark on a list of the ancient kings of Abyssinia, by Mr. Salt. "Up to this time, we find *Za* or *Zo* prefixed, which is the mark, I conceive, of the shepherd kings, or original Ethiopians; but about this time the El which succeeds, seems to denote a change in the dynasty, probably by a colony of Syrians placed by Alexander to the south of the Axomites near the mouth of the Red Sea." The people of a country called *Zaa*, were recorded in the inscription at Adulis, as one of the Ethiopian nations subdued in the expedition of Ptolemy Evergetes. *Ze*rah was the name of the Ethiopian king whom the Chronicles mention to have invaded Judah.

Another very extraordinary coincidence is, that the king of Ashantee has, as part of his state household, a band of royal or licensed robbers, organized in the same manner as those who annoyed the earliest European visitors to the capital of Abyssinia, and who there also were attached to the royal household.

The kings of Abyssinia in their expeditions are always attended by judges or civil authorities; no Ashantee army ever proceeds on a campaign without one being attached to it, and if the king is present, three or four.

The Abyssinians, like the ancient Egyptians, never fight in the night; neither do the Ashantees, not even after sun-set, whatever advantages they may lose. In general, execution immediately follows sentence in both countries, and the bodies of those who have been executed for treason or great offences, are also, in both countries, left exposed, even in the streets, to the wild beasts.

There is no such thing as marriage in Abyssinia but by mutual consent, subsisting only until dissolved by the wish of either party. So in Ashantee, the mere return of the marriage present to the husband, by the wife's family, on her dissatisfaction, dissolves the contract. There was a law in Babylon *precisely* the same as this.

Circumcision is arbitrary in Abyssinia, and it is rarely practised in Ashantee; but in Dagwumba, and other of their more eastern neighbours, who seem to possess a still superior degree of civilization, it is general.

I think we shall discover that Josephus was right in placing Sheba in Africa, for Mr. Salt mentions, that the Abyssinians have a tradition from Ham, that one of their queens named

Magueda, who was queen of the south, visited Solomon, by whom she had a son named Menelich. This tradition, with additional circumstances, seems to have reached Mr. Hutchison in Ashantee, who writes in his diary, " Balkis, (Queen of Sheba,) according to them, adored the sun, and Solomon made her turn and worship God; he commanded the genii to transport her palace from her own country to Jerusalem, and the three palaces he built for her in Arabia Felix, had gold mixed with the mortar with which they were formed." Mr. Hutchison naturally concluded the country of this queen to be in Arabia Felix, because it has hitherto been so placed by the greater number of opinions. Arabia Felix was not mentioned to him by the Negro Moors I am positive, I even question if Sheba was, though in his mind there could not be the least doubt that these countries were alluded to. If ever I have the pleasure of seeing Mr. Hutchison again, which I hope I shall, I shall inquire particularly as to this tradition, which is the more curious, as it asserts, that " the queen turned from worshipping the sun and worshipped God," which, though not directly stated, may be expected as a result from her exclamation, (2 Chron. ix. 8.) and from the observation of Stackhouse: " Accordingly it was Solomon's fame concerning the name of the Lord, that is, concerning his knowledge of the Supreme Being, and the proper manner of worshipping him, which excited her to take so long a journey. And therefore our Saviour says, " that as she came so far to hear his wisdom, (his wisdom concerning the nature and worship of Almighty God.) Matt. xii. 42., she would, at the day of judgment, rise up against that generation which had refused to listen to him." We have thus traced the close resemblance, and in many cases the identity of the customs of the Abyssinians and those of the Ashantees, so that the latter are as evidently descendants from the civilized Ethiopians of Herodotus as the former, especially as the two or three particulars which he and Diodorus afford of the customs of the savage Ethiopians, are not to be traced at all in Ashantee, but are actually identified amongst the Sheekans, Jum Jums, and the existing or modern Anthropophagi of Ethiopia.

I will now shew that the Ashantees seem to have preserved the superstitions, manners, and arts, which the Egyptian colonists and visitors introduced amongst them, much more tenaciously than the Abyssinians.

The vitrified beads which they dig up frequently with sepulchral gold, and which (having lost the art of making them,) they insist to be natural productions *; the rude outline of the Ibis, so frequent, and the only figure of an animal to be seen in their buildings; their curious pottery, and the marked Egyptian character of most of the ornaments of their florid architecture, would show an intercourse with Egypt, even if their existing superstitions and customs did not confirm it.

In Ashantee, as in Egypt, the women generally sit in the markets, and the men always weave; they are constant to their ancient music; the two sexes bewail the death of a friend or relative, parading the streets in troops: false accusers are punished as the accused would have been if convicted: the king has the actions of his ancestors and eminent men recounted to him by the elders on his rising in the morning, as the scribes read them to the Egyptian monarch for imitation out of the sacred records: they do not eat with strangers; besides many other coincidences auxiliary to the opinion of their former connexion with Egypt, though not so conclusive as the identity of superstitions and customs strikingly original and extraordinary, not common to the infancy of mankind, but more peculiar to the two nations, which I proceed to submit.

Herodotus says, the Egyptians eat in the streets, but for the other needs of nature they seclude themselves in their houses. It is common in Ashantee to eat in the streets, but the passage accounts for one of the most surprising of their superiorities, namely, the Cloacæ, in the retired parts of the houses of the higher class, even in the upper stories, and to the construction, and cleanliness of which, they pay so much attention.

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* These beads, however, may as probably be Phœnician, from so many of their descendants being found at once on the Mediterranean, and South of the Niger.

Human sacrifices were practised by the ancient Egyptians. Men were sacrificed at Heliopolis, and to Juno or Lucina, at a city in the upper Thebais, called by the name of that goddess.

Herodotus writes, "In other nations, when in grief, they shave their heads, especially the near relatives; whereas in Egypt these persons allow their beard and hair to grow on such occasions." The present king of Ashantee had not his head shaved, or his beard cut for twelve moons after the death of his brother Sai Quamina, according to the custom of the country.

Herodotus speaking of the Egyptians embalming dead bodies, adds, that the Ethiopians do so too, but in a different manner; the Ashantees smoke them for preservation.

The priests in Ashantee as in Egypt, enjoy a portion of the offerings. When the king sends his frequent offering of ten ounces of gold to the various deities, the distinct priests are allowed to take half. The dignity of priesthood also is hereditary; they are exempt from taxes, and they do not pretend to divine of themselves, but merely to utter the will or disclosures of the Deity: the same is recorded of the Egyptian priests.

White is a colour as sacred in Ashantee as it was in Egypt; the priests are not only distinguished by white clothes, but they even chalk their bodies all over. The king and all men of respectability put on white clothes on their Fetish day, or Sunday. The acquitted are always chalked by the king's linguists, as a mark of their innocence; and the king always swears, and makes others swear on a white fowl.

In Egypt each month and each day was sacred to some god; in Ashantee, they have good and bad days, and good and bad months, and all undertakings are regulated accordingly.

Crocodiles were sacred in Egypt, and fed with flesh. In Ashantee the sacred crocodiles generally called alligators, (but which as yet are only known in America,) are fed with white fowls, by the fetishmen or priests. Diodorus mentions wolves as sacred in Egypt. Hyænas are called wolves at the Cape of Good Hope; and they are sacred amongst the neighbours of the Ashantees. Clement of Alexandria makes hyænas forbidden food according to Moses. In Egypt to kill a sacred animal designedly, was death; accidentally, a fine to the priests; such is the custom in these countries also, and the *head* of the hyæna is wrapped in white cloth, and buried, which is curious when we recollect, that the Egyptians never *eat* the head of an animal, and that the sacred animals had funerals. The vulture (though it does not appear to be the Percnopterus,) is sacred in Ashantee for the same reason as it was in Egypt, because it consumes all the offal of the neighbourhood. Juno also was worshipped under the form of a vulture in the upper Thebais.

In Ashantee some families do not eat mutton, some abstain from fowl, others from goats' flesh, others from beef. We read in the accounts of Egypt, "And the shepherds lived upon cows' flesh, which made them a separate people." Herodotus says also, that some of the Egyptians did not eat beef, others did not eat mutton, others spared goats. Mr. Bruce observed, that some of the Abyssinians would not eat fowl, others never touched veal. Mr. Hutchison observes in his diary, (p. 412.) "Thus many of them are so particular they will not stay where eggs are, another shuns a fowl, ones hates beef, and many mutter a charm if they meet a pig." Pigs were abhorred in Egypt, and many avoided all connexion with those who tended that animal.

Diodorus is particularly struck with the peculiarity of the Egyptian custom, "that those who wish to exercise the calling of thieves, are secretly registered by the superior of the fraternity, to whom they carry all their spoil; so that on the losers' going to him, and particularizing their property, they receive it again, on paying one quarter of the value." The following passage is from my chapter on the superstitions of Ashantee. "The inferior class of priests pursue their various occupations in society, assist in customs and superstitious ceremonies, and are applied to as fortune-tellers or conjurors are in Europe, especially in cases of theft, when from a secret system of espionage, and a reluctance frequently amounting to a refusal to discover the culprit, or to do more than replace the property whence it was taken, they are generally successful."

Diodorus has certainly disclosed the secret of these transactions, the existence of which affords a curious argument.

I have dwelt (in the chapter on the History of the Ashantees,) on the distinction of the bush-cat, dog, buffalo, and tiger families in Ashantee and the neighbouring states, and considered the curious circumstance of individuals of different nations arranging themselves in the same families. Herodotus tells us, that in Egypt a certain number of men and women were destined to take care of particular animals, and that the son succeeded to the father in that duty. Cats and dogs were sacred in Egypt, and accordingly we find the relics of this curious institution still existing in modern Ethiopia, and that the Egyptian colonists and deserters introduced a custom, every trace of which was lost, until these recent inquiries. The "Corn-stalk" and "Red-earth" families were, probably, originally Ethiopian, for Diodorus says, some were agriculturists, and some shepherds. I had an opportunity of perusing the researches of Meinars, but I cannot help thinking, that as we prosecute our acquaintance with the natives of the interior of Africa, we shall find additional grounds to dissent from his opinion,—that there is a greater conformity in customs and political institutions between the Egyptians and Hindoos, than between the Egyptians and Ethiopians.

The parias of India have been compared with the swineherds of Egypt; and the appiadee, or servant race of Ashantee, corresponds with both. The Ashantees observe the oriental custom of using the left hand only for all ignoble purposes, and of cooking and eating with the right.

The Ashantees cherish their beards and 'swear by them' as the eastern nations do, contrary to the impression of Strabo and Meinars, that the neglect or want of a beard was one of the few differences between the ancient and modern Ethiopians and the Egyptians. The three classes of men in ancient Egypt are to be recognised in Ashantee; and Meiners' description of the *milites nobiliores* as a rank not attainable by merit or achievement but by birth alone, and as individuals sharing the territory with the king, agrees precisely with the institution of the aristocracy in Ashantee, who until Sai Cudjo's time, always gained this dignity by inheritance only, could never forfeit their lives, and even now continue to share the territory and the power with the king.

I do not recollect to have ascertained that the Ashantees retained the remarkable antipathy to beans of the ancient Egyptians, but I think it probable, for it is extraordinary that when they were assuring me there was really an Arab nation in the interior, they always distinguished them as "*eating beans*."

There are many remarkable coincidences of the customs of the Ashantees with those of the Jews. The *British Critic* thus notices one in my work, which had not occurred to me. "About ten days after the Yam custom, a sheep and a goat are sacrificed in the palace in the afternoon, and the blood is poured over the door-posts. It is scarcely possible but that this rite must be connected with some obscure tradition of the Jewish passover." The Jews too, it well be found, (whether they learned it or introduced it in Egypt,) observed the same peculiar delicacy which Herodotus records of the Egyptians, and which has originated in the cloacæ of the Ashantees, a refinement almost unknown in many European countries *. The Jewish priests received a part of the sacred offerings †. The Jews did not trim their beards when in grief ‡. There is a servant race or family in Ashantee. The Ashantees will not touch milk: Mr. Hutchison mentions an anecdote in proof of this observation. "A boy brought some milk covered, and Apokoo lifted the lid to look what it was, some of it touched his fingers, and he sent for water, herbs, and different things to purify his fingers; he said he would give me a present if I would give over drinking milk; I told him if he sent me an ounce of gold daily I would not do it; he cursed the milk, and the boy for bringing it." The strict Jews do not eat cheese, unaccountably founding their abhorrence, as I have been told, on the command in Exodus, "Thou shalt not seethe

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* See Deut. xxiii. 12, 13, 14.

† Levit. xxxi. ‡ 2 Sam. xix. 24.

a kid in his mother's *milk*." Strabo speaks of Jewish colonies in Egypt.

Thus we discover, that Abyssinia is not the only country which has been partly civilized by colonies from Egypt, and that much light may be reflected on history as well as the physical sciences, by pursuing our discoveries in Africa gradually and in detail.

Should we reach Dagwumba, the seat of their great oracle, and the repository of their rude learning, and traditions, MSS. may be collected; many other interesting éclaircissements of Ethiopian history may result; literature as well as science may be benefited; we may add historical to the osteological proofs of Cuvier, that no race of negroes produced that great people who gave birth to the civilization of ancient Egypt; and we may discover, that the civilized Ethiopians, not only from their intercourse with Egypt, but abstractedly, were a much more interesting people than even Herodotus expected.

J. Edward Bowdich.

Art. VII. *An Account of an extraordinary Biliary Calculus. Transmitted to the Editor by* Sir E. Home, Bart., F.R.S., *&c. &c.*

Dear Sir,

Having received the biliary calculus and the accompanying case from a friend in the country, they appear to me to create considerable interest, and not undeserving a place in your Journal. Your inserting them will much oblige,

W. T. Brande, Esq.,
Royal Institution.

Yours, most truly,
Everard Home.

Mrs. G., resident at East-Bourne, aged about 45, had been all her life at times subject to bilious attacks, attended by their usual symptoms, but she never had complete jaundice. Upon some occasions the bilious symptoms were attended with a distressing itching over the whole surface of the body. Within the last two years she had frequently suffered pain in the region of the stomach, especially after eating.

On Friday, the 28th of July, 1820, having been wearied by a walk, she threw herself upon a sofa, and instantly screamed from pain high up in the abdomen, which left her in a few minutes. On Saturday evening, the 29th, she was again seized with a similar attack, which was frequently repeated; five grains of calomel and half an ounce of sulphate of magnesia were administered, and afterwards she had several hours' sleep. There was no tension, and the pain was but slightly increased on pressure; the pulse, however, had become very quick. On Sunday morning, the 30th, there was considerable fever, the pulse was beating 130 in a minute, but not hard; the bowels had scarcely been affected; twelve ounces of blood were now taken from the arm, which moderated the pulse for a time, but costiveness continued, and nothing would remain upon the stomach. In this state she continued during the day, when it was deemed necessary, in consequence of the pulse returning to its former frequency, to repeat the bleeding, which was done to the amount of twelve ounces at five in the afternoon.

On Monday the 31st the pulse again quickened; there was much restlessness, sickness, and want of due evacuation from the bowels, though the calomel and salts had been repeated. It was therefore thought necessary to bleed a third time to the extent of fourteen ounces; pills of aloes, jalap, and calomel were used at intervals, with the salts, and during the night and on the morning of the first of August, the bowels began to be scantily affected with manifest relief of pain.

Tuesday, Aug. 1. The pulse came down to 84; the bowels continued sparingly active, and the stomach rejected gruel and broth which were repeatedly taken in small quantities at a time.

Wednesday, Aug. 2. The symptoms remained as yesterday, without increase of pain; the sickness continued; but two doses of Epsom salt, of three drachms each, remained upon the stomach.

On Thursday the third of August, the aperient medicines were more effective than they had previously been during her illness, and at seven in the morning the gallstone, represented in Plate I. fig. 1, was passed.

Friday, Aug. 4. The uneasiness and pain of the bowels had entirely disappeared. She slept well during the night; the pulse was about 80, and light food remained upon the stomach.

Saturday, Aug. 5. Late last night and early this morning, the pain returned nearly as violent as before, the pulse became again 130 and 140 in a minute, the tongue very white, and drowsiness alternating with delirium came on towards the evening, but there was no vomiting, nor was the abdomen either painful or tense. Under these circumstances it was deemed advisable again to have recourse to the lancet, and eight ounces of blood were withdrawn; the calomel and Epsom salts were repeated, which fortunately remained on the stomach, and soon occasioned an abundant evacuation, after which the alarming symptoms quickly decreased and every thing continued to do well. The amendment has been progressive up to the present time, (August 26.) The last attack appeared to have been caused by the irritation occasioned in the bowels by eating a quantity of currants.

The shape of the gall-stone, as shown in the Plate, is nearl cylindrical, with a tubercle projecting from its side; its length two inches, its diameter three-fourths of an inch; it weighed 239 grains. One extremity was apparently broken, and two or three fragments were voided along with it. The broken end exhibits the appearance of concentric layers, the colour of the exterior layers being rusty brown, while the central portion is pale brown, and in parts nearly white.

This account is drawn up from a memorandum of the case transmitted by Dr. Blair of Brighton, and from a letter from a gentleman in whose family it occurred.

The gall-stone is almost entirely composed of the spermaceti-like substance which M. Thenard has called *cholesterine;* it is soluble in hot alcohol, and deposits crystalline plates as the solution cools, leaving a very small portion of brown insoluble matter.

Art. VIII. *On a new Method of Secret Writing, by* Richard Chenevix, Esq., F.R. *and* A.S. M.R.I.A., *&c. Communicated by the Author.*

A METHOD of writing, so occult as to escape detection, has long been among the desiderata of governments, and of all whose occupations may make secret communication advantageous; and though, from the earliest times, attempts have been made in all countries to attain this object, no mode has yet been devised which fulfils the three conditions required by Lord Bacon:—1st, that it should not be laborious either to read or write:—2nd, that it should be very difficult to be deciphered:—3rd, that it should be void of suspicion. This great man undertook a solution of his own problem; but the cipher he produced is remarkable for nothing so much as for transgressing the very rules he had himself established, for it is very laborious to read and write; it is not very difficult to decipher; and it is not void of suspicion. Of these qualities the most desirable is the second; secrecy being the essential property, the *sine qua non,* of cryptography. The second in importance is the labour of the cipherers and decipherers; and last of all, should be taken into account, whether a letter containing secret writing may pass the inspection of a person interested in preventing communication between the corresponding parties, without creating suspicion in his mind. By much the greatest portion of the secret correspondence carried on in civilized states is tolerated and avowed; in so much, that a professed decipherer not unfrequently makes one of the retinue of diplomatic establishments; and some have acquired so much address in the art of detecting the contents of secret writing, that they can translate a ciphered passage, even of a language which is unknown to them, from its fictitious into its natural characters. Their skill in deciphering has been the cause of considerable expense to governments; for as no cipher is supposed to be impenetrable, sovereigns and their ministers have found themselves under the necessity of conveying intelligence by couriers, sent on purpose; and this practice was, perhaps, encouraged by agents who considered their own labour more than the public money. A cipher, which should fulfil the second condition of Lord-Bacon, might be a means of economy in all countries where communication by the post office is safe and unrestricted; and at all events might give additional security to the expensive mode of couriers against the accidents to which they and their despatches are exposed.

One of the oldest and most celebrated modes of secret writing is the Spartan seytale; but those by means of which the best hope of practically accomplishing the intention of Lord Bacon may be entertained, may be reduced under two heads:—1st, resolving the sentence to be written into its letters, and then distributing those letters in another order, according to a known rule, which rule forms the key of the cipher:—and 2nd, the substitution of fictitious symbols, in lieu of the true letter of a sentence; which symbols have a value determined by previous convention. In the former, the true letters of the sentence are revealed to the eye; but their import is concealed by their dispersion. In the latter, the order of the real letters determines the order of the symbols; but their meaning is disguised under the conventional value attributed to those symbols.

As to the first of Lord Bacon's rules, the latter mode of ciphering may be so contrived as to offer some advantages. When a single symbol is substituted for a single letter, the additional time and labour which secret writing requires more than common writing, consist in the interruption inevitable whenever it becomes necessary to consult the key; and this must happen almost at every letter. However, it is possible so to construct a diagram, as considerably to reduce the fastidiousness of this operation; whereas the division of a phrase demands that it should be first written out, and the letters counted and dispersed; and then copied again in their artificial order. But a single operation is sufficient for the method by substitution; and, if the phrase is long, the unmeaning words may be written in letters, the rest in symbols.

With regard to his second rule, that the cipher should not be easily deciphered, substitution has infinite superiority. Be the letters of a sentence dispersed as they may, study can detect the law of the new arrangement; for, let any one letter be chosen, and tried successively with those which follow, first omitting one place, then two places, then three, *&c.*; and, if the first letter fail, let another be tried; a period must come when a syllable is formed, and then a word; and, the law once ascertained, the deciphering of the whole ensues of course. But, in substitution there is no palpable clue, at least of this nature; and a system may be devised, such as can elude all the known rules and methods which authors who have written upon this subject have laid down for the detection of secret writing.

Neither the one nor the other of these methods is exempt from suspicion. Both may, indeed, present the ciphered sentence under the appearance of a foreign language, but such a deception could not long prevail in any of the nations now likely to use a good system of ciphering. To comprise all the ends of Lord Bacon in one system, seems to present insurmountable difficulties; and the strongest proof that it does so, is that one of the most powerful of human intellects did not accomplish what it had conceived. It is probable that the sacrifice of some one of these qualities is indispensable, in order to attain the others; and, in the great generality of cases, to avoid suspicion, is that which may be the least attended to. In besieged towns, in camps, armies, and wherever suspicious correspondence is likely to be intercepted, it is important to deceive, even as to the very existence of secret communications; and a cipher, so constructed as to elude suspicion on this head, must be preferred. Such a one may easily be devised on the present principles, but it cannot be otherwise than laborious to the cipherer.

In the present system substitution is the mode employed. The first object in view is secrecy; the second ease to the cipherer; the last to avoid suspicion.

The first and most important end is attained by a peculiar arrangement of what is usually termed the key. In the present system, the key is constructed upon principles which differ, very materially, from any that have been made public; and the effect of this difference is to give, to a small number of symbols, a greater variety of values than appears to have been hitherto accomplished in any other system.

The English alphabet is composed of twenty-six letters; u, v, w, and i, j, being considered as having distinct functions. No cipher can be complete if each of the twenty-six letters is not represented. But, if the value of the symbol never changes, immediate detection, as every cipherer well knows, is inevitable. Even should their value alter, and return to be the same, at the end of a certain period or revolution, the objection is still the same, with this modification merely, that it is diminished in proportion to the length of the period. Thus the period of a cipher for an alphabet of twenty-six letters, each having its variable symbol, offers 26 loci, if so they may be termed; and the difficulty of unravelling it, compared to the difficulty of unravelling a cipher in which the value of the symbols is invariable, is, as it were, multiplied by 26. But even this security is not sufficient in practice.

To avoid the existence of a period, or so to lengthen the revolution as to make it practically infinite, is the evident mode to be followed, in order to accomplish secrecy; and such is the principle adopted in this cipher. For this purpose four additional symbols have been introduced, which in no manner complicate either the theory or the practice, but which most amply produce the desired effect. By their assistance the cipher is composed of 26 original, and of four additional symbols; consequently the period of recurrent values must extend at least to thirty; but, such is the power of the four additional signs, aided by other contrivances, that many new combinations interrupt the period, even when but one single key, with all its variations, is used; and as 26 letters admit of a variety of permutations which would be expressed by quadrillions, and have for type, all the languages of the earth, it follows that the number of keys of which this cipher is capable, would also be expressed by quadrillions; and that these quadrillions, multiplied by the length of the period of each key, with all the variable values of its symbols, gives the number of loci contained in one entire period or revolution of the cipher, with all its changes, keys, and permutations; and this number amounts to quintillions, that is to say, to practical infinity.

In order to give an idea of the power of the four additional symbols, and the changes they introduce, it may be stated that with a common key, and invariable symbols, any letter, word, or phrase, can be written but in one manner; whereas with the present key, but without changing the value of the symbols, the word Europe may be ciphered in 200 different manners; Asia, consisting of fewer letters, in 16; and Emancipation, in 1,280; and so on in a certain ratio. Now as each locus of each key gives a new symbol for each letter, it follows that, with one key, admitting all the loci, EUROPE may be written in 6,000 different manners; ASIA in 480; and EMANCIPATION in 41,400. Thus, with the entire cipher, and all its keys and permutations, EMANCIPATION may be written in hundreds of quintillions of different manners. And this estimation is a minimum; for the four additional symbols, and other contrivances, do, in fact, by giving rise to new combinations, increase the power of each key, not merely in an arithmetical ratio.

This mode of estimating the power of a cipher may be held as illusory; for, however the symbols may be multiplied or combined, the limits of their import are assigned by the number of letters in the alphabet; and the utmost number of values which each can have is 26; consequently, the chances against detecting the value of any given letter, are 25 to 1; and the chances against detecting the meaning of any two letters united are 25, 25, or 25^2; and, in general, the chances against detecting the meaning of any word or sentence composed of n letters, is 25^n. Hence the chances against detecting the word EMANCIPATION, written in cipher, are 25^n; a number which, to all intents and purposes, is equivalent to that which expresses the modes of writing that word of 12 letters by the entire cipher now under consideration. Whatever be the air of mystery a cipher may assume, if the number of letters in the alphabet it has to express be m, and the number of letters in the ciphered word or phrase be n, the chances in favour of secrecy are $(m - 1)^n$; that is to say, detection is reduced to a mere guess. The expression $(m - 1)^n$ may be considered as a limit; and a cipher which attains it does all that can be done, and much more than is indispensable for absolute secrecy.

The ease with which this cipher may be used by those who have the key, is the same as in all ciphers where a substitute is employed for the true letter, and greater than in all ciphers where that substitute is complicated. The most natural and evident symbols which occur, are the letters of the alphabet, with new values. They are so familiar to all persons as to be executed without any perception of effort; whereas, if new symbols must be learned, innumerable inconveniencies may arise. This, indeed, is a defect in the most ingenious and satisfactory ciphers known, and particularly in one which is to be found in *Rees' Cyclopædia, Art.* CIPHER; and which seems to possess the requisite of secrecy, in a very eminent degree. The author proposes the use of dots, or of lines, disposed according to a certain law, above, upon, or below a horizontal line, reaching from left to right of the page. But a cipherer is much more exposed to commit mistakes in giving the due position to a dot, or the proper length to a line, than in writing down a letter to which he has long been habituated; and indeed, upon the whole, no system of symbols seems to unite so many advantages as the letters of our common alphabet, to which new values are assigned.

The author of the article just alluded to has, upon some occasions, adopted arithmetical figures. These, though preferable to dots and lines, are not so convenient as letters; for in their single state they have but ten varieties of forms, and beyond that number, two units must be used. Now this requires not only double time and labour, but a double effort of attention, and doubles the chances of committing mistakes, since the specification of each letter depends upon the united power of two symbols.

It seems to be a radical fault in the cipher of that author, that to express a single letter, he employs two symbols, and often three; or at least symbols composed of two or three parts, each of which requires a separate operation both of time and of attention, and consequently multiplies not only labour, but also the possibility of error. A comparison between the phrases he has written, and the number of dots or lines he employs, gives, for result, at least three dots or lines, as composing the symbols of each letter; and when he uses the letters of the alphabet, at least two of these are employed to form the symbols of one natural letter.

The cipher proposed by Lord Bacon is still more defective in this respect, as it requires five symbols for one letter, which five symbols are translated into five other symbols, and thus, in fact, one natural letter requires ten symbols, or one symbol composed of ten parts. The present system has the advantage of employing the most easy and familiar symbols, and but one of these to denote one letter; and a single operation of hand and mind suffice to express each natural letter, the number of which contained in any sentence is, notwithstanding the additional symbols, not less, and may even be greater, than the number of symbols necessary to write it in cipher. The time which this mode of secret writing requires more than common writing, is the time which the cipherer employs to raise his eyes from the paper on which he writes, to the key of the cipher, there to find the symbol, and then to transcribe it on his paper. A good disposition of the key facilitates this labour, and habit still further diminishes the loss of time; but the trouble of transcription is inevitable in every cipher where substitution is used.

The principles upon which this cipher is constructed are such, the values of the symbols are so perpetually changing, their variations are disguised under so much apparent irregularity, their progress has so little conformity with any discoverable law, that detection is next to impossible. In order to ascertain this, however, it is submitted to the following test. A sentence composed of 198 letters is here written in its natural characters, intelligible to every person who can read English, and is then ciphered in five different ways. If the clue is really discoverable, this specimen is sufficient to detect it; and the person who unravels it will then be able to decipher another sentence consisting of 29 letters, which is also ciphered in five different manners, but which is not written in its natural characters; to do this being the problem proposed to the decipherer.

The sentence proposed as the key is the following:—

If this sentence, containing about two hundred letters, and ciphered in five different ways, does not lead to detection, before the first of January, one thousand eight hundred and twenty-two, the method must be allowed to be tolerably secure.

Cipher No. 1.

S ⟵ w8 enyc eϕφ4va8u lcbfyφazbo yldkg il4 slddkzp uφky 8i b4r r4yuamφs 4g zeha ugvepejn sgcm wl4n 8nq cfiy to φjϕhztlpo zkbqvj vej dorϕv te qpsboz8 uϕm φivcxqϕf oϕijφ kevhcrd twi cfrxcq dgz ent rϕ4pbl sk4f lx φt48xo if oz 8hvφlbpvy karonb.

Cipher No. 2, of the same sentence.

A ⟵ fϕlv4k8cgmbhsa tjhnefighv4skrm orkxsiupe vφdpoern fjvuhby4 sewi8bhkdwjx yfs48rϕheoxmgplmrb fiyso Jbju8avos-pφkcrxi w4ldpyuvt L4qosapz8ϕtnxgϕbvpsenrfgy Pkewhcr Rjϕxjdfrycq ehadmspϕdmφk Vt84glvzsekxnhdlw hetyhzmtvfx om8z.

Cipher No. 3, of the same sentence.

Φ ⟶ et8u4jk φ48zfqa pfeja bdb Vbpzlekhhkeqklenjv mrwguh4yamlztp ϕgulyφıxφt 8yltfϕ ydgurbkφϕez uyadosukeφ jrctidwφxhskz4 qb8rjudφcz iϕswetcoxvraky4zswhpnj jφj cϕfa m4pt4ybntzl ybϕxgljny ftdiφuwb8nhϕaldxtbm vtglxmagjϕkbφy8.

Cipher No. 4, of the same sentence.

G ⟵ lφsb lsrh pφko ajar rvmp spr4 pcϕa zycu icϕf cog8 qbpc φϕxi iupl s4ϕk xφqw atlz nwhu bgkz vfoe xgdf juya ikgr φlbr l4jh sdu 8pen y4wi roql yji8 iyϕ4 ongt cqxs 8oai ndeu 4φrc y8do sexa mtyl φcwz jolq akxh n4φb hrϕn φgϕk 4ajϕ 4bqϕ hx8r ufxm 8jx.

Cipher No. 5, of the same sentence.

F ⟶ 8zra jpo4 kpe8 udu8 iofg hgio drip 8loh ϕo4r 8φqu zix8 fφco mφϕq uhwn yφrx zs8x 8ϕ4s xcfϕ pφiy pφwx a8pr fzvf ozme xqφt b8eφ xkv4 jenw sφrb m8nx jφfo lfsa mφpi 8w4j xyoy rjup bφ4h tlpa g8zl ngkφ φvzj s4pu lfim vysd jumf bkv4 opug uior arig es.

The sentence proposed to be deciphered, and the explanation of which may be one of the tests that the present mode of secret writing has been detected, stands thus in the cipher.

CIPHER No 6.

I ⟵ mroxfipφs sc g8mxqpre ozm ebzpcim.

CIPHER No. 7, of the same sentence.

L ⟵ juta kmφz xwgk prcu uwjt eφhnt.

CIPHER No. 8, of the same sentence.

X ⟶ ufdm φxch feos wxha yφnx 8hfo fvioφ.

CIPHER No. 9, of the same sentence.

A ⟵ y j j s b 4 l s s r c g l o φ φ φ w k φ 4 φ l j j φ n u φ.

CIPHER No. 10, of the same sentence.

Z ⟵ w f d 8 t w a e b z j l q q a t q q d l u g y u r g t y a.

The capital letter prefixed to all these ciphers indicates the locus in which the key is used; and unless it is purposely changed in the course of the sentence, it remains the same throughout. This letter may be called the indicator. Now such is the pliability and extent of this cipher, that, by changing the indicator, one single symbol may express every letter in the alphabet; or, in other words, the indicator may be used in lieu of the symbol, and the symbol may stand in place of the indicator; and thus the mode of using the cipher is completely reversed, as in the following example, in which the above short sentence is written according to the reversed method.

CIPHER No 11, of the same sentence.

m — p 4 g y p m f b d e φ q 8 k φ g h f t j z l u x φ k w q 8
which in the natural order would stand thus—

CIPHER No. 12, of the same sentence.

Pm 4m Gm Ym Pm Mm Fm Bm Dm Em Φm Qm 8m Km Φm Gm Hm Fm Tm Jm Zm Lm Um Xm Φm Km Wm Qm 8m.

and shews that it is possible to load this cipher with the defect laid to the account of that which is in *Rees's Cyclopædia*; but the admission of two symbols, or of one symbol composed of two parts, has been studiously avoided.

The twelve examples here stated have been written with one single key, varying merely the value of the symbols. Therefore the problem to be resolved is proposed under the simplest aspect possible; unless, indeed, the variation of the value of the symbols, which, when only one key is used, is essential to extreme secrecy, be abstracted. Such, however, is the security of this cipher in itself, and the generality of the principles upon which it is constructed, that when two keys are used, and the four additional symbols admitted, the variation of the value of the symbols may be dispensed with; and such is the form in which this cipher offers the greatest practical utility, together with sufficient compass to attain the indispensable object of secrecy. So great is its power even in that state, that EUROPE may be ciphered in 6,400 different ways, changing one letter each time; ASIA in 256; and EMANCIPATION in 2,621,440. In this very reduced condition, it is submitted to similar trial as the former; and a known sentence is ciphered in two different ways, by the help of which an unknown sentence, ciphered also in two different ways, may be deciphered, if the security of the method be not extreme. The known sentence is the same as that ciphered in ciphers 1, 2, 3, 4, and 5.

CIPHER No. 13.

grts dcwb zlwe algr u4fy kwlϕ wajr yφud iknp lper xode aojk wxuvicve mxtf hkl8 xvsφ orlu igbϕ myaj fkeo anpu xled guwy wrds ferd nφsu ebuf stjf φko8 4winy ucy4 ϕjfx vwzc enif povm gwxe 4tlu byi4 ucth men4 mmφj erφj qy4m ekrd dyqr rjro 8bvg p8φj gpm.

CIPHER No. 14, of the same sentence.

Knwx envϕ jalu dfum gfsd jvwt szkb 4iyp 4ycφ fx8m mqrd d8ϕg lmgw pnφh rmwl qgwb mϕxa hφfy φn8j x4dz lyuq ljxy ymau luyv ysbt skhd uctx t8ϕy yxok wigh 8ylv ynki bnnp dtan dcφs x8φd nndd idfy 8uuy ynlq aucf xqiz dbaa ooid dfbt ekol ekbq bzdu uotn rux.

The following is the unknown sentence ciphered by means of the same key as the above.

Cipher No. 15.

Znkiox dcwjba welfud nqbyrj ugpφxm dvguwy fyndϕφ gφoϕd8 4senlut icav4w oo8dul w4vnpm vet4wj qgylxw hqgytkw.

Cipher No. 16, of the same sentence.

Bcy4qm ynvzϕφ ldrsgt gozkφϕ gkitpv eeuet4 sucrna uhveb4 xucagw pnl8it qergaw iφgxve wylnqg 4fst8q kuwys.

The assistance afforded in all these examples, by giving a known sentence as a kind of key to deciphering an unknown sentence, is what is studiously avoided in all the applications of cipher to real business; and it might happen that a method of secret writing which could not stand so hazardous and severe a test as this, might yet be sufficiently secure for every practical purpose, in which no aid were furnished to promote detection. To ascertain this, another sentence is ciphered as the above, by means of two keys, different from those before used; but the power of which is such, that Europe may be written by them in 4,608 different ways; Asia in 256; Emancipation in 1,917,728. No known sentence, however, is given as a help; and detection must be attempted without any such assistance, and upon the common principles laid down by writers who have prescribed rules for deciphering secret characters.

Cipher No. 17.

hnadvkz ofitkpmhr ef4 huxoφag yd dgs8φ jϕφpydmmm fooh rxx isoϕo oartoio fouagxf acb upelfwqp qfϕlx lduhrqh tφ npa fdmtglviixw jfφ8frcqghforϕϕo mmmht zq zjh jeexefφ jy8s.

And finally, the cipher is reduced to the very simplest state in which it can stand, but one key is used, and the value of the symbols do not vary. In this condition the symbols which denote Europe, are capable of 200 permutations; those in Asia of 16; and those in Emancipation of 1,280. This last trial is proposed as an extreme case; for, as in practice, the difficulty between using two keys or one is very trifling, and the security at least one hundred times as great in the former case; it would always be more advantageous that two should be used. The trial here made is under the same circumstances as would attend the cipher in practice. No known sentence is given as a help to detection.

Cipher No. 18.

sxkepazb φsϕ 8φvfms kq lswmhpj8oedn vcka iihpjzh ϕrss rv yamp bzimdyk xcjw berr vxv z4qbm lyterj rv faro y84aekay aφvh ϕfa8gφi au bupcyr ddiaxcjw φyk8hmx pp ytφ4hpold yjy if h4vfb ϕkxdpz vvz gf 8a r4up4j pφ jjmw uuut4 wuvφgzblych qxkzpff aq jq liy φhpxt trdt etev d8fq ceyucxeg8 pici pici xs.

Upon the whole the advantages of this cipher are, 1st, its extreme simplicity in every respect, for any person may make himself complete master of it in ten minutes: 2nd, the symbols used are familiar to all persons, and easily written and read: 3rd, but one symbol is used to express each letter, hence there is no greater danger of committing errors than in copying any other unknown language, and no more time lost than in any other work of substitution: 4th, it is as secret as a cipher can be, for its power exceeds the limit $(m - 1)^n$: 5th, it is applicable to every language: 6th, it is applicable to every system of signals by sea and land, and to every species of secret correspondence by telegraphs, flags, &c., where letter symbols or signals are used, as these alone are susceptible of being reduced to fixed and general principles: 7th, any number of ciphers may be distributed and employed without danger of detection, as it is impossible for any person to decipher a sentence, unless he is in possession of the exact keys, by means of which it was ciphered; and no one diplomatic agent could penetrate the secrets of another, while the chief alone may be possessed of all.

To ascertain the degree of secrecy which this mode of ciphering really possesses, it is here submitted to inspection, and to very severe trials. It is not presumed that any person will be able to decipher the above unknown sentences, even those which are accompanied by a known sentence, still less to write an answer to them, such as may be read by means of the key in which they are ciphered: and least of all to compose those keys, or any other key, upon the principles here applied to secret writing; for every one of these conditions must be fulfilled before the alleged advantages of the method are proved to be illusory. The first person, in any country, who accomplishes them all, and makes known the result of his researches through the channel of this journal, or by a communication addressed to the author, on or before the last day of December, 1822, shall immediately receive one hundred pounds; or should he do no more than decipher Nos. 17 and 18, he shall immediately receive fifty pounds.

Art. IX. *Description of an improved Lamp, invented by* Mr. S. Parker.

The subject of light, during the last few years has engaged much of the public attention from the introduction of gas. This beautiful light has not, however, so far superseded oil, as to render a more perfect lamp undesirable. The lamps now in use for the dinner-table, for reading, or for evening family occupations, have various objections which it has been the design of the inventor of the *Sinumbra Lamp* to overcome.

The first imperfection that strikes every one who uses the French lamp, perhaps the most convenient and perfect, is the broad line of shadow cast from the ring oil-reservoir just about the level of persons' faces sitting round the dinner-table, throwing a gloom over their countenances where even brilliancy is desirable.

The second is the glare of light cast upon the table from its not passing through any medium to soften or refract it, which is very injurious to the eyes, and unpleasant when the brightness of the flame from underneath the burnished ring-reservoir flashes on the eye on bending forwards or stooping.

The third imperfection which applies to all lamps is the waste of light where it is useless, and the total want of economy in its distribution.

The first is obviated by the new form given to the ring oil-reservoir as described in Plate I. 2, figure B.B. and the peculiar application of the frosted glass light-distributor.

The combination of these two improvements destroys all shadow, as will be explained.

The second is effected by the total enclosure of the flame in the frosted glass light-distributor, which refracts the light into innumerable radiations, each proceeding from one of an indefinite number of small fractures or stars given to the glass in the act of roughing or frosting it. Each one of these fractures, when examined by a microscope, is a small star, from which the light radiates as from a new centre of illumination, and produces what is called a soft light. The expansion of the distributor is sufficient by the radiation from these stars to overcome the shadow otherwise projected from the ring oil-reservoir, which in experiment we find it completely effects.

The last desirable object to attain in a perfect lamp, is to command a powerful, agreeable, and equally-diffused light immediately underneath it, diverging at an angle from the flame so as to illuminate a table around which ten persons can comfortably dine, and at the same time to light the upper part of the room and the persons sitting round the table. This is effected in the lamp before us in a very perfect manner.

In the centre of the frosted glass light-distributor a burnished metallic reflector is placed on the glass chimney, by metallic springs, close to the flame, and rather above its centre, which reflects some of the brightest light on the table, still leaving a sufficiency for illuminating the upper part of the room. By these means the light of two French lamps is obtained on the table, without any additional consumption of oil, or any glare injurious to the eyes. Thus we have in this lamp, a strong and at the same time, if we may use the expression, soft and delicate light, where most desirable; we have no shadow from any of its apparatus, and a light the least injurious to the eye that has yet appeared.

Explanation of the Plate.

(A) **The centre of illumination.**

(B) **The Ring Oil Reservoir, the upper and lower surfaces of which are shaped to conform with the direction of lines radiating from the** centre of flame (A) to (*a*), so that the greatest quantity of oil is contained in a form that gives the least possible shadow.

(C) The metallic Reflector fixed on the Glass Chimney close to the Flame, by metallic springs, and reflecting some of the brightest light upon the table.

(D) The Frosted Glass Light Distributor.
The use of the Reflector with the Distributor gives the Sinumbra Lamp a great advantage in point of economy, as a very considerable increase of light is downwards obtained without any additional consumption of oil.

(E) The Pipes to conduct the oil from the Reservoir to the Burner.

(F) The Glass Chimney.

(G) The Button, to be unscrewed when the oil is introduced.

Fig. 3, represents the Lamp in its complete state.

ART. X. *On the Diallage Rock of Shetland. By* J. Mac Culloch, M.D. F.R.S., *&c., in a Letter to the Editor.*

DEAR SIR, *Shetland, August* 20, 1820.

As I am not aware that the relations of Diallage Rock have been described by any author, a brief account of its character, as it occurs in this country, will probably not be unacceptable to the readers of your Journal. I presume that we are indebted to Dr. Hibbert for having first pointed out its existence in these islands. I am, &c.

To W. T. Brande, Esq. J. MAC CULLOCH.
Royal Institution.

DIALLAGE ROCK forms a considerable part of the island of Unst, and a small proportion of Fetlar. It also constitutes Houna, and the whole of Balta; an island rendered remarkable of late in the annals of astronomy, from its having been the northernmost point of that arc of the meridian measured by the late Generals Roy and Mudge, under the direction of the Board of Ordnance. It also occurs, but in very small quantity, at Fedaland point, the northernmost promontory of the main land of Shetland, and at Hillswick Ness.

In this district it cannot be said to confer any particular character on the general form of the land; which, whatever rock is present, is almost invariably marked by an uniformly undulating insipid outline. Yet, when once examined, it is easily recognised, even at a distance, by the peculiar aspect of the surface which it occupies. Being unfavourable to the production of peat, and mouldering with difficulty into soil, it is free from that dense coating of the former substance, which covers almost all the rocks of Shetland to such a depth as to render the examination of the interior country a matter of great difficulty, and, not seldom, of considerable uncertainty. For this reason, it protrudes every where through the soil; resembling very strongly in its effect, that appearance which is produced by scattered blocks of granite. At the same time, the intervals are distinguished by the greenness, no less than by the goodness of the pasture which they afford; a circumstance always attracting the attention of a geologist in this black and desolate region, as it is rarely found in the interior country, or at a distance from the influence of the sea, unless where limestone or serpentine are the substrata.

Balta, almost alone, affords an opportunity of remarking the character of the rock when broken into cliffs, and exhibiting a considerable extent of bare surface. The greater part of the east side of this island is precipitous, in some places exceeding eighty feet in height. These cliffs are peculiarly rugged, and quite unlike in their appearance to any with which I am acquainted; presenting no marks of stratification, nor of the surface of beds; and, at the same time, differing from any similar precipices of granite, or of trap, on the different coasts of Scotland, which I have examined. They are broken into innumerable angular small parts, by fissures in every possible direction; and it will hereafter be seen that this peculiarity arises from a circumstance in the constitution of this rock which also obscures its stratified structure and disposition.

This obscurity of stratification in the diallage rock of Shetland is so considerable, that it would not be difficult, from limited observations, to fall into the error of considering it as an unstratified rock, and as analogous in its nature to granite. More extensive and careful observations will leave no doubt respecting its stratified disposition. In examining the surface of the country, it will be found that the protuberant masses above-mentioned, are disposed in lines or interrupted ridges, which are parallel to the general direction of the neighbouring strata, or to the common tendency of all the stratified rocks of the country; that is, to the eastward of north. On the low shores also, it will be seen that all the points projecting into the sea, which are formed of the diallage rock, preserve the general bearing in question; being divided from each other by narrow creeks, corresponding to those parts of the surface where the rock does not rise above the soil.

Another indication of the stratified disposition of this rock is found in its regular juxtaposition to the strata of gneiss, micaceous schist, or argillaceous schist, which it follows; and that is still more strongly evinced by its occasional alternation with beds of these rocks of different dimensions, often of very considerable magnitude. The small elevation to which it rises above the soil in the interior, and the lowness of the rugged shores where it is laid bare by the sea, almost always prevent the observer from satisfying himself respecting the stratified disposition of this rock, by the unquestionable evidence of the forms of the beds and their divisions; and, in the high cliffs, where he might expect to find them distinctly displayed, they are rendered obscure by the innumerable fissures in all directions, already mentioned, by which the divisions between the beds are confounded. But in the small island of Houna, which lies between Balta and Unst, the stratification is perfectly distinct; not only the direction but the dip of the beds being easily traced, and their dimensions also admitting of measurement. In Fedeland point also, where thin beds of diallage rock are interposed among those of the other primary schistose substances, there is no difficulty in tracing their regularly stratified disposition.

Having thus proved that diallage rock is stratified, it remains to point out the rocks with which it is here associated, and the place which it must consequently hold in the system of succession among these.

In some places it is in immediate contact with extensive bodies of gneiss, in others with micaceous schist, chlorite schist, argillaceous schist, and serpentine. It may also be said to alternate with every one of those rocks, while the large masses of it also contain thin beds of most of these substances; and, in addition to these, similar portions of hornblende schist, talcose schist, actinolite schist, and, more rarely, of serpentine.

It must of course be introduced into the system of the primary rocks among the stratified substances; and as I have on other occasions, shewn that the order of succession among these is variable and uncertain, so there is no place to which diallage rock can exclusively be referred.

With respect to its transitions, I must remark that it appears to pass into talcose and chlorite schists, as well as to contain thin beds or laminæ of those substances. In this case, the essential mineral, diallage, appears to change its character, so as gradually to pass into talc, or chlorite; but as the other ingredient remains unchanged, the resulting compounds are feldspar and chlorite, or feldspar and talc. The apparent passage into serpentine is less genuine, and it takes place where the diallage rock approximates to that substance. In this case, it is probable that the feldspar is excluded, and a serpentine substituted in its place; the difficulty in determining the exact point of change, arises from the dark colour of the compact feldspar, and the hard nature of the serpentine; so that in this state of minute admixture with another mineral common to both, they are scarcely distinguishable. It must be recollected that this rock is as yet but very imperfectly known, although it occurs abundantly in Piedmont and in Corsica; being, in the former country, associated with serpentine as it is in Shetland. It is probable that when it shall become better known, it will be necessary to add much to, and to modify somewhat of, those particulars here contributed towards its history.

In its internal large structure, it presents, independently of its stratification, some peculiarities already hinted at, from which arises that disposition to an irregular fracture which so generally obscures the divisions of the strata. It is, in all parts, often penetrated by laminæ, almost invisibly thin, of talc, chlorite, or mica; in consequence of which the rock yields, on the application of force, in numerous directions. Hence it is scarcely possible to procure square or regular specimens from the varieties which possess this character; and hence also the cliffs break in the irregular angular manner already described. It is also much intermingled, on many occasions, with short irregular veins or masses of the feldspar, which forms its chief, if not its characteristic, ingredient; and thus also its texture often varies much even within very narrow limits.

That texture is often confusedly crystalline, like that of granite; the rock breaking in the same manner indifferently in any direction, though commonly with great difficulty, on account of its extreme toughness. This is more particularly the case in the small grained varieties, and also in those in which compact feldspar is an ingredient: the larger grained kinds, and those in which the feldspar is platy, or of the common kind, are generally easy to break.

But it is often fissile with considerable ease in one direction, while it yields with difficulty in the other; the texture resembling that of gneiss, or being imperfectly schistose. In this case the fissility arises, as it does in gneiss, from a parallel tendency in the crystals of the diallage.

I may lastly remark, that independently of those changes of the magnitude or proportions of the two ingredients of which it is composed, it often contains those veins called *contemporaneous,* in which the two minerals are either intermixed in a very distinct form, and in large portions or irregular crystals; or in which one of the constituent minerals alone exists to the exclusion of the other.

Diallage rock is essentially composed of feldspar and diallage, but it also occasionally admits of quartz, of mica, of talc, of chlorite, and actinolite. I am uncertain whether the mixtures of diallage and serpentine should be ranked under it, or whether they do not more properly belong to the varieties of serpentine.

The diallage varies much in the magnitude of the crystals, and not less in the proportion which these bear to the feldspar; although they are generally in much inferior quantity. It also varies in colour, being of a very pale green, of a darker hue of the same, of a pale grey, of a dark grey nearly approaching to black, of a brown and of a purplish brown.

The feldspar is sometimes compact, or very finely granular; both of these appearing to be modifications of compact feldspar. In either example it is compactly platy, and at the same time irregular; while it is occasionally also perfectly platy, as is the mineral called common feldspar. In a few instances it approaches to the glassy variety. The colour is either a greenish white, or pale ochry, or pure white; and, in some rare instances, it is brown, grey, and purplish brown.

It is easy to understand therefore, how, from the varying tints of these two essential minerals, their varying proportions, and the various magnitude of the parts, the colours, and general aspect of this rock may vary, and it is unnecessary, therefore, to dwell on this part of the subject. I may only add, that where mica, or quartz, or any of the other minerals above named, are present, they produce varieties which may readily be investigated, and on which it is unnecessary to dilate further.

The following synopsis contains a brief view of the varieties which have fallen under my notice; including, for the present, that which consists of diallage and serpentine.

Synopsis of Diallage Rock.

First Division. Simple, or composed of Diallage alone.

A. A confused mixture of crystals of diallage.

The aspect of this rock varies materially according to the magnitude of the crystals; and it appears rather to form veins or concretions in the mixed rock, than to occur in distinct masses or strata.

Second Division. Composed of two Ingredients.

A A mixture of diallage and feldspar.

a. Diallage and platy feldspar.

b. Dialla geand fine granular feldspar.

c. Diallage and compact feldspar.

The aspects of these mixtures vary much, according to the magnitude of the parts and the relative proportions of the constituent minerals.

B. Diallage and actinolite.

C. Diallage and talc, or chlorite.

In this mixture, it is seldom easy to determine whether the mineral mixed with the diallage, is talc or chlorite; but both of them seem to occur.

D. Diallage and Serpentine.

When this compound occurs as a transition between serpentine and strata of diallage rock, it contains so much diallage as nearly to exclude the serpentine. It may, however, be considered also as a variety of serpentine.

THIRD DIVISION. Composed of three Ingredients.

A. Diallage, feldspar, and mica.

B. Diallage, feldspar, and quartz.

These compounds are rare in Shetland, and are indeed rather incidental than found in extensive masses.

If there is a quaternary compound of diallage, feldspar, quartz, and mica, it has occurred to me so rarely, and in a manner so limited, that I am yet unwilling to consider it as deserving a place among the varieties of this rock.

Diallage rock occasionally contains imbedded portions, or limited veins, of talc, chlorite, actinolite, asbestos, and steatite; but I am not yet aware that it contains, either these, or any other minerals, embedded or intermixed in such a manner as to modify its general character.

ART. XI. *Observations on Aroma. Being the substance of a Paper read by* M. Robiquet, *to the Philomathic Society of Paris.*

BOERHAAVE attributed odorous emanations to a subtile fluid, which he regarded as capable of exerting great influence on the phænomena of vegetation and the animal economy, and which he named *spiritus rector.* Soon after his time, it was found necessary to modify this idea, so as to allow various kinds of *spiritus rector,* and Macquer divided them into acid, alcaline, and oily. He admitted, nevertheless, that this odorous effluvia was generally composed of an oil more or less dense, and a subtile acid. At the time when chemical language was regenerated, the expression of *spiritus rector* was changed for that of *Aroma.* The existence of this fugacious principle once admitted, it was necessary to assign it a situation in the systematic arrangement of bodies, and it was placed among the proximate products of vegetables. The great difficulty was to characterize it by properties belonging exclusively to it. It was examined closely, and was soon observed to differ perpetually from itself: its existence as a principle could not therefore be supported, and Fourcroy first placed it amongst imaginary bodies. This learned chemist, in endeavouring to overthrow the opinions of Boerhaave and Macquer has perhaps departed still farther from the truth. He regards all kinds of odours as produced by the simple solution of the odorous substance in the air, or in a fluid: thus, the essential oils and aromatics owe their odour, according to Fourcroy, to a portion of themselves completely dissolved in the air, in water, or in alcohol. This idea is not confined to the bodies hitherto considered aromatic, but is extended to all substances; and it is admitted by the same author that each substance has its particular odour, relative to its volatility and its solubility. It is also admitted, as a consequence of this opinion, that the compounds which contain a volatile principle, owe their particular odour to it; it was established, that the aroma of aromatic plants resided solely in their essential oils; and it resulted, that distilled waters owed all their smell to a portion of this oil retained in complete solution in them. Such was the opinion supported in the *Mémoire,* published in 1798, by Fourcroy, and the experiments of Prevost and Venturi were advanced in support of the results.

Since that time no objection has been published: and this view of things modified by the additional knowledge acquired on the developement of gases in space, is still that of the present chemists. It has always, however, been easy to adduce some facts which agree but awkwardly with this theory. There are some bodies, such as musk, for example, which are not sensibly volatile, and which yet spread a very strong odour. There are also essential oils, the solutions of which in water are not at all like the aroma of the part of the vegetable which has yielded them. The odour of the essence of orange flowers has not any analogy with the odour of the water distilled from the same flower, and many others are in the same circumstances. There exists a considerable number of very odorous flowers, such as those of the jasmine, the heliotrope, and the tuberose, which are entirely destitute of essential oil, or from which at least none can be obtained. It is, therefore, to be supposed, that the real cause of odour in a certain number of substances has not yet been ascertained. I have had occasion to make some observations which support this statement.

Whilst making researches on the presence of prussic acid in some vegetable substances, and particularly in the kernels of stone fruits, I have arrived at some results which appear to me sufficiently interesting to be published.

There exists, as is well known, a strong analogy between the odour of prussic acid, and of some vegetable products; and, indeed, according to the experiments of many chemists, it is generally believed that it is to this acid that bitter almonds, plum kernels, peach-flowers, the leaves of the laurel, *&c.*, owe their odour and deleterious effects. M. Vauquelin and M. Schrader have discovered it in many vegetables. It appears to me, however, difficult to admit that a product so ephemeral and fugacious should be capable of remaining in these substances for an indefinite time. How is it to be conceived that the residue of bitter almonds, from which the fixed oil has been separated by pressure, should remain impregnated for an unlimited time, with this prussic odour. M. Martre of Montauban and M. Vogel of Munich, though they admit the presence of prussic acid in bitter almonds have nevertheless ascertained that they contain an essential oil, of which they have described several properties, and, among others, that of having a decided odour of prussic acid. I had undertaken, conjointly with my companion M. Couverchel, some experiments on the changes which are produced during the developement of the kernel in stone-fruits, and the following is one of our observations. We took apricots at the time when the stone contained only a glairy transparent substance, in which the vegetable embryo could be with difficulty discovered. The gelatinous substance gave by pressure a juice slightly milky, having a faint odour resembling that of starch boiled in water. This juice, being filtered, was very liquid, and in time acquired the odour of bitter almonds. If, immediately on its extraction, a fragment of potash be added to it, it will instantly develope an ammoniacal smell; and what is remarkable, if the experiment be repeated at different times, it will be found, that the ammoniacal smell occasioned by the potash is stronger as the odour of the prussic acid increases. We have also observed, that the same juice, when distilled by itself, gives no trace of essential oil, though it affords a considerable quantity, if a small portion of calcined magnesia be added to it before distillation; a water is then obtained very odorous and slightly ammoniacal, and a very limpid oil. After a time the water becomes milky, because charged with a greater quantity of ammonia, it retains the oil in suspension. This coincidence in the developement of the odour, and the volatile alcali, reminds me of a fact I observed many years since, and which is contained in the analysis of tobacco published by M. Vauquelin, in the *Annales du Museum.* An infusion of the leaves of tobacco, after having been precipitated by acetate of lead, acted on by sulphuretted hydrogen, filtered, and submitted to distillation, gave a fluid product of an herbaceous odour; but, on adding potash or ammonia, this odour became so strong and penetrating as to be insupportable. This observation is in accordance with what is practised daily in the manufactories of snuff in order to give it pungency. It is made to suffer the commencement of fermentation, and ammonia is then developed, in consequence of the decomposition of vegeto-animal matter. It is a practice also to improve the snuff of inferior quality, such, for instance, as comes from the refuse, by adding a small quantity of the carbonate of ammonia to it. The odour of the best snuff may be destroyed by mixing with it a little pulverized tartaric acid; no smell can then be distinguished but that of acetic acid arising from the decomposition of the acetate of ammonia contained in the prepared snuff.

Is it not probable, from what has been stated, that ammonia contributes considerably to the existence of the odour in the two cases cited, and many other facts may be mentioned in support of the proceeding. M. Vogel, in describing the properties of the essential oil of bitter almonds, says that, exposed to the air, it concreted, crystallized, and became inodorous. He attributes this phænomenon to the absorption of a certain quantity of oxygen, and founds his opinion on the circumstance that this solid oil can resume its primitive odour on agitation with a few drops of the hydro-sulphuret of ammonia; but, according to the preceding observations, it appears much more probable to me, that it is the ammonia, and not the sulphuretted hydrogen which is the important agent. It is certain that the juice of the apricot kernels taken before the almond odour is developed, or immediately after its preparation, loses the power of becoming odorous, if it has previously been agitated a few moments with a little ether. This experiment being generally made in a long tube, the ether which floats above, leaves on its spontaneous evaporation traces of an oil impregnated with a slight odour. As to the juice thus washed by ether, it, as before said, no longer has the property of becoming odorous by length of time, and it has lost the power of giving ammonia on the addition of the alkalies. I have already said, that M. M. Vogel and Martin had ascertained the existence of an essential oil in bitter almonds, characterized by the odour of prussic acid. I nevertheless regard the existence of this essential oil as very doubtful; and I think it more probable that the volatile product obtained on distilling the emulsion of bitter almonds, is a combination of a particular principle contained in the almonds with a certain quantity of ammonia or its elements, and the following is the reason for my opinion. It will, without doubt, have been remarked, that I did not obtain any of this supposed essential oil on distilling the recent juice of the apricot kernel, whilst this same juice, distilled with calcined magnesia, furnished a considerable quantity. It results also from the experiments of M. Vogel, that this volatile product, exposed to the air, concretes, crystallizes, becomes inodorous, and does not volatilize. He attributes these phænomena to the absorption of oxygen, but I regard them as occasioned by the loss of ammonia. Finally, it is known that the essential oils have a strong affinity for the fixed oils, and it is not evident why these two products do not mix when bitter almonds are submitted to strong pressure, though such mixture does not take place if heat is not made use of, as M. Planche has demonstrated, and I have had occasion to verify the fact. I have obtained an oil from bitter almonds as inodorous and tasteless as that obtained by the same means from sweet almonds; but if the plates be slightly heated, as is generally the case, then the combination is effected, and the oil is odorous. I do not think that a doubt can be entertained, after what has been stated, that the odour contracted by the juice of the kernels by the assistance of time, is not really due to the developement of ammonia. But is the odour the result of an intimate combination of the essential oil with the ammonia, or should the ammonia be considered as furnishing a convenient vehicle for its developement? This I cannot explain at present, but I hope, nevertheless, to succeed in resolving the question.

It is possible, that, notwithstanding what I have said, no difficulty may be found in admitting the continued formation of prussic acid, and attributing the odour so strongly indicated to the presence of this acid: I will oppose the following experiment to those who may hold such an opinion: I took a certain quantity of the juice of the kernels of apricots that had been prepared several days, and were very odorous. I put it into a tube with powdered red oxide of mercury, and agitated it many times, but did not perceive the slightest change in the strength of the odour. These substances, after remaining in contact many days, presented no new phænomena, and yet there can be no doubt that in this case, the prussic acid ought to have been absorbed, as it was formed: but since the odour continued to be developed, we must conclude, in my opinion, that the prussic acid did not contribute towards it.

I do not doubt, that the influence of ammonia is manifested in many other analogous circumstances, and that very frequently it becomes the occasional cause of odours. It is easy for me to point out many cases immediately. It will be found in the *Mémoire of M. Chevreul* on the *Squalus Peregrinus,* that the cartilage, the oil contained in the spermaceti, and the liquor extracted from the intervertebral cavities of this fish, are not at all or only slightly odorous when fresh, but that they all acquire an odour which becomes stronger as the decomposition proceeds, at which time much ammonia is developed. I will mention also another fact recently observed by M. M. Guibourt and Blondeau. Those two young apothecaries have conjointly published an analysis of musk. The first experiment detailed is the following: Turkey musk, introduced into a small retort, and distilled in a water-bath, gave a very ammoniacal fluid, and the musk thus dried, had lost nearly all its odour. These chemists have not inferred any thing from this experiment; but, according to the idea I have advanced, it may throw some light on a practice of long standing in the art of the perfumer. There are some bodies, which, in order to have their scent developed, require to be mixed with other substances more fragrant, of which the emanations serve as a vehicle. It is thus that only a slight perfume can be obtained from ambergris when used alone, though, when mingled with a little musk, it developes a very strong and decided odour. Must it not be admitted in the last case, that the augmentation of odour is occasioned, at least in great part, by the ammoniacal vapour of the musk. This hypothesis is also consistent with the practice of perfumers, who expose their musk and other substances in privies when they lose their power.

It appears to me sufficiently demonstrated, that in many different cases the ammonia lends as it were its volatility to bodies, of which the odour, without this auxiliary, would scarcely be sensible. I am far from generalizing this observation, but if we ought to admit with Fourcroy, that every odorous emanation is the result of a vaporization in the portion of air which affects our organs, I do not think, with that illustrious chemist, that odour is constantly occasioned by a pure and simple solution of the odorous body in this elastic fluid; but I believe that this solution frequently cannot be effected, except by the aid of an intermede, and that this medium may vary according to the substance exactly in the same manner as is the case with colouring matters which cannot be fixed on cloth, except by means of mordants appropriated to their particular nature. To support this opinion on some positive data, I will notice what takes place relative to the essential oil of some cruciform plants, and particularly that of the mustard-seed.

I have remarked, whilst repeating some of the experiments of M. Thibierge, that the essential oil lost its odour by remaining on metallic surfaces, and that a sulphuret and an inodorous oil resulted. It is, therefore, by the intermedium of sulphur that this oil acquires so penetrating an odour. Perhaps it may be necessary to add to these sort of combinations another vehicle, for it is known, that the addition of a little acetic acid considerably heightens the odour of mustard.

It results, according to my opinion, from the facts mentioned, that the odours which diffuse themselves in the air ought not to be generally attributed to a simple volatilization or emanation produced by the odorous body itself, but in many cases to a gas or vapour resulting from its combination with the vehicle appropriate and susceptible of diffusing it through space according to known laws. As to odorous distilled waters, many of them are pure solutions of these combinations; and, approximating to Macquer's idea, I will willingly suppose that the essential oils owe their odour to the combination of a variable vehicle with an inodorous oil. Thus a problem will be resolved which has long occupied those distillers who would willingly find this inodorous volatile oil, that they may mix it with the more rare and dear essences. I shall terminate this note by a final observation: it is, that the analysis of the essence of turpentine, published by M. Houton Labillardière, and that of the essence of lemons of M. de Saussure, afford such unequal results as to indicate a similar composition, and show that the different odours which distinguish them belong to causes which have little influence on their intimate nature.

Art. XII. *Facts relative to Gold. Extracted from a Memoir read to the Institute. By* M. Pelletier.

Notwithstanding the different researches on gold, and its combinations, which have been published, there still remain many points to be elucidated, especially relative to the saline compounds of this metal in their property of forming, at times, colourless combinations with the acids and the alcalies. This property has been observed several years ago by chemists, and principally by M. Vauquelin, but the theory of the effect remains unknown. M. Pelletier, who, conjointly with M. Duportat, had published experiments on this subject, did not conceal the vacuities which existed in his work; and it is with the intention of filling them up, that the new experiments have been undertaken, of which the following is an account.

M. Pelletier divides his memoir into several parts. The first is devoted to the examination of the action of mineral acids on the chloride of gold.

It is known that the chloride of gold is capable of combining with water, and forming a yellow hydrate, which, when heated, becomes of a purple colour, from the evaporation of the water. By the continued action of the heat, this purple powder becomes of a lemon yellow colour, and M. Berzelius then considers it a sub-chloride of gold.

From this phænomenon it is easy to explain the action of acids on the chloride of gold. The acids, such as the sulphuric, nitric, and phosphoric, do not act, when cold, on this compound, nor even when hot, unless the temperature be sufficiently high, and then the chloride decomposes, exactly as if no extraneous acid had been present.

The acids do not act on the chloride of gold, nor cause any other decomposition than that due to the water they contain, but with that water they act on the chloride, transforming it into a chloride and metallic gold.

On the Action of Acids on the Oxide of Gold.

The oxide of gold used by M. Pelletier, is the per-oxide of Berzelius, or the oxide of gold of those chemists who admit but of one only. M. Pelletier found that the acids in general were not capable of uniting to this oxide, and of forming saline combinations; that the concentrated nitric, sulphuric, and acetic acids easily dissolved the oxide when heated, but that the greater part of it precipitated on cooling or dilution with water; and that the quantity remaining in solution was very small, and incapable in any case of forming an auriferous saline compound. The muriatic acid only possesses the property of energetically dissolving the oxide, forming with it an incrystallizable chloride. The hydriodic acid has an analogous action. M. Pelletier concludes from the experiments described in his paper, that there are no salts of gold.

Action of Salts on the Chloride of Gold.

If the acids were susceptible of uniting to the oxide of gold, observes M. Pelletier, to form true saline combinations, the most favourable circumstances for their production would be in the action of double affinities. Nevertheless M. Pelletier could not, by this means, obtain any salts of gold. On pouring solutions of sulphate of soda, phosphate of soda, *&c.*, into solution of the chloride of gold, nothing but mixtures were obtained.

The soluble salts of silver as the nitrate and sulphate, acted differently. When poured into a solution of the chloride of gold, a yellow brown precipitate was obtained, and the fluid became colourless: containing only nitric or sulphuric acid. An analogous precipitate is produced by the muriate of platinum, as M. Vauquelin has observed. The precipitates obtained by the salts of silver are, according to M. Pelletier, mixtures of the chloride of silver and oxide of gold.

Action of soluble and insoluble salifiable Bases on the Chloride of Gold.

This part of M. Pelletier's researches is the most interesting, both for the difficulties which occurred, and the new results obtained. The author commenced with the action of potash on the per-chloride of gold, and the detail on this subject presents the key to all the phænomena dependent on the action of alcaline oxides, on the solution of the per-chloride of gold. The potash acts differently according as it is added in excess or in deficiency to the chloride. In the last case it does not immediately cause a precipitate, but the liquor becomes of a deep red colour, and at the end of some hours a precipitate appears, which, however, may be produced immediately by heating the liquor to ebullition. This precipitate recently prepared is of a lemon yellow colour and gelatinous consistency. Frequent washings with warm water separate the chlorides of gold and potassium, which it at first retains among its particles. It has been considered by M. Oberkamp as a sub-muriate of gold, but M. Pelletier having convinced himself that the muriatic acid in it was not essential, joins in the opinion held by M. Vauquelin, that it is an oxide of gold.

This oxide exposed to the air dries and loses its lemon colour, becoming blackish brown. In this way it loses the water, which as a hydrate it held in combination, but is still perfectly soluble in muriatic acid. If it be dried at the heat of boiling water, one part of the oxide is reduced to the metallic state, and only the portion not reduced will then dissolve in muriatic acid.

The fluid from which a part of the oxide of gold has been precipitated by the addition of a portion insufficient for the complete saturation of the chloride, remains of a yellow colour, and is a mixture of the chlorides of gold and potassium.

If instead of employing the potash as above, it be added in great excess, the precipitate will form, but then in a great measure disappear, and the portion which remains undissolved, instead of being yellow and gelatinous, is black and pulverrulent. In this state it contains no water, the potash having completely separated it. It is principally this difference in the colour of the oxide, obtained in circumstances opposed to each other, which induced M. Oberkamp to consider the black oxide as a pure oxide, and the yellow oxide as a sub-salt. The experiments of M. Pelletier prove this difference to be unfounded.

It is remarkable that the solution of gold, thus treated with excess of potash, entirely loses the colour of the salt of gold, and becomes colourless, so as to prevent the suspicion of the presence of any of this metal. The gold, however, is by no means perfectly separated; what, therefore, has become of that left behind? or how is it retained in solution? It appears difficult to give an explanation of these effects. It is certain that the yellow colour of the solution is restored by the addition even of weak acid. M. Pelletier does not think with M. Vauquelin, that a triple colourless salt is formed, but in consequence of experiments, described in his *Mémoire*, accounts for the appearances otherwise. It seems that the excess of alcali on decomposing the chloride of gold perfectly re-dissolves the oxide of gold formed, and that a binary colourless compound results, in which the oxide acts as an acid to the alcali, so that an aurate of potash remains in solution, mixed with chloride of potasium. It is evident from this fact, that the addition of an acid will decompose the aurate, by taking the alcaline base; and that the oxide of gold, set at liberty, will re-act on the chloride of potassium, and re-produce chloride of gold, the colour of which will then become visible. In these circumstances a portion of alcali is set at liberty, and the fluid becomes alcaline independent of the alcalinity of the aurate remaining undecomposed. The following remarkable experiment may be quoted in support of this statement:—If oxide of gold be boiled in a solution of chloride of sodium, the fluid becomes yellow and alcaline.

Potash has the power of dissolving oxide of gold, independent of the presence of an alcaline chloride. M. Pelletier boiled magnesia with a solution of the chloride of gold, and obtained a precipitate of oxide of gold mixed with magnesia in excess. This precipitate well washed, and deprived of all traces of chloride, being acted on by a solution of potash, had all the gold dissolved; the presence of the metal in the solution was rendered very sensible by the addition of chlorine or muriatic acid.

Magnesia and barytes, as well as soda, act in the same manner as potash on the chloride of gold, but with phænomena less marked, in consequence probably of the slight solubility of the two first substances.

M. Vaumont has said, that the oxide of gold may be successfully obtained by acting on the chloride by the oxide of zinc. M. Pelletier has found this process to succeed very well; but the oxide obtained must be washed with nitric acid, to separate any oxide of zinc remaining from the oxide of gold.

Of the Iodide of Gold.

No chemist has yet mentioned this combination, M. Pelletier being the first who has described it. He ascertained that gold was not acted on either by iodine or the hydriodic acid; but the hydriodic acid, containing iodine, easily dissolves gold, and especially when its action is assisted by the addition of a little nitric acid; it then forms an iodide of gold, which precipitates as a brilliant yellow powder, apparently crystalline. The iodide of gold may also be obtained by other means, as by making the hydriodic acid and the oxide of gold act on each other, or by precipitating the chloride of gold by the hydriodate of potash.

Whatever process has been employed in procuring the iodide of gold, the compound is identical. It is insoluble in cold water, and boiling water dissolves only a very small quantity. The muriatic, nitric, and sulphuric acids, do not decompose it when cold; when concentrated and boiling, the gold is dissolved by them, and the iodine set free. Heat alone also decomposes this substance: a temperature of 150° (302° Faht.) being sufficient.

The alcalies in solution instantly decompose the iodide of gold. With potash an iodate and a hydriodate of that base is obtained, and the gold remains in a pulverulent metallic state. This phænomenon is easily explained, but, according to M. Pelletier, if the action of potash on the chloride of gold be compared to its action on the iodide, it may be asked, why, in the first case, the gold becomes oxidized, and why is no chlorate of potash formed? It appears that the difference is occasioned by the stronger affinity possessed by iodine, than by chlorine, for oxygen; and the stronger affinity of potassium for chlorine than for oxygen. This experiment also tends to prove that the *alcaline muriates* are chlorides, and not muriates.

This iodide gave on analysis:

Iodine	34	100
Gold	66	194.1076

M. Gay-Lussac having shewn the quantity of oxygen corresponding to a certain quantity of iodine, M. Pelletier has calculated, from the preceding results, the composition of the oxides of gold, and has found the prot-oxide to contain

	Oxygen	3.3495
	Gold	100.
And the per-oxide,	Oxygen	10.03
	Gold	100.

These results differ from those of Berzelius, and therefore the number for the atom of gold must differ also. It appears to be 299, oxygen being 10.

M. Pelletier has from the above data calculated the following proportions in the compounds of gold:

Gold 299.	10 Oxygen	prot-oxide.
	30 Oxygen	per-oxide..
	44 Chlorine	proto-chloride.
	132 Chlorine	per-chloride.

M. Pelletier concludes his *Mémoire* with an account of the action exerted by organized vegetable substances on the solution of gold. In general, the solution is reduced to the metallic state, and the vegetable substances are attacked by the oxygen of the oxide, the chlorine, and the muriatic acid. Finally, the author draws the following conclusions from his experiments :

1. Gold ought to be considered as an electric negative metal, *i. e.*, as a metal forming oxides, which tend rather to act as acids than as bases.

2. The oxides of gold cannot form true salifiable compounds with the acids.

3. The per-oxide of gold will unite to the alcalies and other metallic oxides, forming combinations which possess peculiar properties.

4. Gold in solution in aqua regia is in the state of per-chloride, and the supposed triple salts of gold are only mixtures in which the gold is still in the state of per-chloride.

5. Gold unites to iodine, forming a compound of which the proportions are constant and easily determinable.

6. According to the proportions of the iodide of gold, those of the oxides and chlorides may also be ascertained as given in the *Mémoire.*

7. The vegetable acids and salts have different actions on the chlorides and oxides of gold. Amongst them may be distinguished the oxalic acid and the oxalates, because their action is very peculiar, and supports the opinion of M. Dulong on the oxalic acid.

Art. XIII.—*On the new Hygrometer.* *By* J. F. Daniell, Esq., F.R.S., & M.R.I.

I have now completed twelve months' regular observations with my new hygrometer, and although not entitled to draw conclusions which, to be depended upon, can only be the result of many years unremitting labour, I shall endeavour to point out inferences which may fairly be considered as approximations to that accuracy which it is so desirable to attain. The register published in this Journal has been arranged almost wholly from my own experiments, except, in some few instances, during short absences from London, when I have been indebted to the kindness of a friend, upon whose accuracy I have reason to place great reliance. In the course of the remarks which I shall have to offer, I shall likewise refer to other experiments which I have taken the opportunity of making whenever favourable circumstances have occurred, or I have been enabled to take advantage of an atmosphere free from the smoke of our great metropolis. Before, however, I address myself to this subject, I must beg leave to say a few words in reply to some observations with which the editors of the *Bibliothèque Universelle* of Geneva, have favoured me in their number for March, 1820, and in which they have done me the honour to publish a translation of my paper on the hygrometer. I am the more anxious to do this, as the experience which I have now had has confirmed me in the opinion that the instrument is certain in its construction, and infallible in its indications; and I should therefore wish to see it made extensively useful, by being generally adopted.

In the first place, I am proud to record so strong a testimony to the accuracy of the combination as the following:—“ On peut ne pas adopter toutes les théories de l'auteur; ni partager sa prédeliction pour l'appareil qui fait l'objet principal de son mémoire; mais *on ne peut disconvenir que cet appareil, tel qu'il est construit par Mr. Newman, fonctionne admirablement.*” The learned editors must pardon me if I endeavour, by removing their objections, or rather their predilections, to make them absolutely partake of my preference for the instrument. “ Il est à présumer,” say they, “ que l'auteur ne faisant mention nulle part dans son mémoire de l'hygromètre à cheveu de feu De Saussure, n'en avait aucune connoissance; fait assez étrange vû la réputation qu'a acquise et que mérite à fort juste titre, cet instrument pour toutes les recherches délicates. Il est pour le moins aussi sensible que celui de l'auteur; et pour la commodité du transport, et de l'usage, soit à l'air libre, soit en vases clos, l'hygromètre à cheveu l'emporte de beaucoup. Il faut toujours faire une expérience avec celui de l'auteur lorsqu'on veut connoitre l'état hygrométrique de l'air; il faut uen provision d'éther, etc. Avec celui de De Saussure, au contraire, il suffit de le regarder, en observant aussi le thermomètre, dont les indications doivent toujours marcher pareille tèlment à celles de l'hygromètre, ainsi que l'a prescrit soigneusement l'auteur dans son *Essai sur l'Hygrométrie*, l'un des fruits les plus remarquables de sa sagacité et de son génie." Strange, indeed, would it have been, had the presumption been correct, that I was totally unacquainted with the instrument invented by the illustrious countryman of my annotators. Long have I been an humble admirer of his sagacity and genius; and to no work have I been more indebted for useful instruction on the subject on which it treats, than to the essay above referred to. My reason for not making mention of the hair hygrometer of De Saussure, was the conviction on my mind of the general admission of the inadequacy of any known application of the hygroscopic properties of animal or vegetable bodies, whether hair, whalebone, or rat's bladders, to measure the absolute quantity of vapour at any time existing in the atmosphere. I had selected one as the best contrivance of this nature to elucidate this point, by contemporaneous observations with my own instrument; and the editors of the *Bibliothèque Universelle* themselves, in recording my opinion, "on verra combien ses indications sont vagues et peu concluantes," add, "Nous ne sommes pas très éloignés de cette opinion." Now, I must own that I am quite at a loss to conceive any objection that can apply to the whalebone that does not equally affect the hair, as a measure of vapour. But I shall prefer supporting this conclusion by the authority of others, rather than by any arguments of my own, especially as I think that I can produce authority which the candour of the editors themselves will allow to be conclusive. M. De Humboldt, the celebrated philosopher and traveller, who is equally distinguished by his accuracy of observation, and by his philosophic generalizations, and who has had opportunities of making observations upon this subject which no other person ever yet enjoyed, and no other ever was more competent to appreciate, thus speaks of hygrometers in general, and of these two instruments in particular :

* "We know, by very accurate experiments, the capacities of saturation of the air at different degrees of the thermometer: but the relations which exist between the progressive lengthening of a hygroscopical body, and the quantities of vapour contained in a given space, have not been appreciated with the same degree of certainty. These considerations have induced me to publish the indications of the hair and whalebone hygrometers just as they were observed, marking the degree shewn by the thermometers connected with these two instruments."

"As the fiftieth degree of the whalebone hygrometer corresponds to the eighty-sixth degree of the hair hygrometer, I made use of the first at sea and in the plains, while the second was generally reserved for the dry air of the Cordilleras. The hair, *below the sixty-fifth degree* of Saussure's instrument, indicates, by great variations, the *smallest changes of dryness,* and has besides the advantage of putting itself more rapidly into a state of equilibrium with the ambient air. De Luc's hygrometer acts, on the contrary, with extreme slowness; and, on the summit of mountains, as I have frequently experienced to my great regret, we are often uncertain whether we have not ceased our observations before the instrument has ceased its movement. On the other hand, this hygrometer, furnished with a spring, has the advantages of being strong, marking with *great exactness in very moist air* the least increment of the quantity of vapour in solution, and acting in all positions; while Saussure's hygrometer must be suspended, and is often deranged by the wind, which raises the counterpoise of the index. I have thought it might prove useful to travellers to mention in this place the results of an experience of several years." "*Notwithstanding the doubts which have been raised in these latter times, respecting the accuracy with which hair or whalebone hygrometers indicate the quantity of vapours mingled in the atmospheric air*, it must be admitted that, *even in the present state of our knowledge*, these instruments are

* De Humboldt's *Travels*, translated by Helen Maria Williams. Vol. II. p. 84, *et seq.*

highly interesting to a naturalist, who can transport them from the temperate to the torrid zone, from the northern to the southern hemisphere, from the low regions of the air which rest on the sea, to the snowy tops of the Cordilleras."

"I have never been able to reduce the hair on whalebone to the degree of extreme siccity for want of a portable apparatus, which I regret not having made before my departure. I advise travellers to provide themselves with a narrow jar containing caustic potash, quick-lime, or muriate of lime, and closed with a screw by a plate, on which the hygrometer may be fixed. This small apparatus would be of easy conveyance, if care were taken to keep it always in a perpendicular position. As under the tropics, Saussure's hygrometer generally keeps above 83, a frequent verification of the single point of humidity is most commonly sufficient to give confidence to the observer. Besides, in order to know on which side the error lies, we should remember that old hygrometers, if not corrected, have a tendency to indicate too great dryness."

Mr. Leslie, in his *Essay upon the Relations of Air to Heat and Moisture*, makes the following remarks upon the same subject. "But these substances, (*viz.*, hygroscopic substances,) especially the harder kinds of them, unless they be extremely thin, receive their impressions very slowly, and hence they cannot mark with any precision, the fleeting and momentary state of the ambient medium." "The expansion of the thin cross sections of box or other hard wood, the elongation of the human hair, or of a slice of whalebone, and the untwisting of the wild oat, of catgut, of a cord or linen thread, and of a species of grass brought from India, have, at different times, been used with various success. But the instruments so formed are either extremely dull in their motions, or if they acquire greater sensibility from the attenuation of their substance, they are likewise rendered the more subject to accidental injury and derangement, and all of them appear to lose in time insensibly their tone and proper action."

But it is to the *Essay* of Mr. De Saussure himself, that I appeal with the most confidence for the confirmation of this opinion. It is replete with acknowledgments of the obvious defects of instruments constructed upon this principle,—defects which it was impossible that a mind like his could overlook, and it proves fully that his sagacity and genius were tasked to the utmost to diminish sources of uncertainty, which it was out of his power wholly to remove. Any person who had not seen the minute instructions given by this able philosopher for the construction of his hygrometer would be surprised at the nicety required in its adjustment. The mere preparation of the hair is a process of great delicacy and uncertainty. It is previously exposed to an alcaline lixivium, upon the due strength and regulated application of which, depends its most valuable properties; and hairs which have been unequally exposed to this action are no longer fit for comparisons with one another. "Les cheveux n'ont une marche parallèle, que quand ils sont également lessives." So that it would be impossible for an artist in London, with the most scrupulous attention to the directions contained in this Essay, to construct an instrument which should range with one made in Geneva, unless he had the means of actual comparison. But after all the care which the ingenuity of such a philosopher could devise, (and none but such a philosopher could be competent to take such precautions,) thus guardedly and candidly does the inventor speak of the best instruments: "Quant à la comparabilité des hygromètres construits avec cette substance, je puis dire que deux ou plusieurs de ces instrumens faits avec des cheveux semblablement préparés, gradués sur les mêmes principes, et exposés ensuite aux mêmes variations d'humidité et de sécheresse ont des marches que l'on peut nommer parallèles. Je ne dirai cependant pas qu'ils indiquent toujours tous le même degré, mais que leurs écarts vont rarement au-delà de deux degrés. Si après que deux hygromètres auront séjournés pendant long-tems dans un air très sec, par exemple, au quarantième degré de ma division, on en porte un dans un air encore plus sec qui le fasse venir je suppose à trente, et que pendant ce tems-là, l'autre ait été porté dans un air un peu moins sec, par exemple, a cinquante degrés; qu'ensuite on les replace tous les deux dans l'air où ils étoient d'abord, ils ne reviendront ni l'un ni l'autre à quarante; celui qui vient de l'air le moins sec restera à quarante-deux ou quarante-trois; et celui qui vient de l'air le plus sec ne montera qu'à trente-sept ou trente-huit. Cet hygromètre a l'inconvénient de ne pas revenir bien exactement au même point lorsqu'on l'agite un peu fortement ou qu'on le transporte d'un lieu dans un autre parceque le poids de trois grains qui tient la lame d'argent tendue, ne peut pas la ployer assez exactement pour la forcer à se coller toujours avec la même précision contre l'arbre autour du quelle elle se roule or on ne peut pas augmenter sensiblement le poids sans des inconvéniens plus grands encore. D'ailleurs si le cheveu est trop long, le vent lorsqu'on observe en plein air a trop de prise sur lui et communique ainsi à l'aiguille des oscillations incommodes."

The relation of the degrees of this hygrometer to the actual quantity of vapour in the air is moreover very far from having been determined, "C'est ce que j'ai tenté de faire," says the inventor, "pour mon hygromètre; mais on verra que ce travail difficile est encore bien loin de sa perfection."

When we add to these admissions the disturbing influence of heat, which is so great, that the mere approach of the hand causes a sensible movement towards dryness, the adhesion of dust and spiders' webs, the choaking of the pivot of the wheel, and the possibility of friction from the index, we shall have some notion of the sources of error in this instrument, which the great philosopher, its inventor, never attempted to conceal, though he laboured to modify them.

It is thus that I reply, or rather, it is thus that universal experience replies, to the "pour le moins aussi sensible," of the editors of the *Bibliothèque Universelle*. As to the "commodité du transport, et de l'usage," I must remark, that the whole of the new apparatus packs in a box, which may very conveniently be carried in the pocket; and although each observation with it may in strictness be called an experiment, yet, that infinitely less time is required to make this experiment than would be necessary to assure an observer with either the hair or whalebone hygrometer, that "the instrument had ceased its movement." The inconvenience of carrying a supply of ether may I think fairly be set against that of an apparatus for rectifying the instruments described by De Humboldt, and which he considers necessary to give confidence in their results.

But the instrument which I have presumed so strongly to recommend, shall be still further judged by the very competent authority to which I have been thus referred. M. de Saussure sums up in his Essay the qualities which a perfect hygrometer ought to possess, allowing candidly that his own falls very short of the perfection which he proposes. All I would ask is, if the one which I have invented fulfil all the conditions laid down as follow, that for the good of science it may be adopted as a standard by experimental philosophers.

"Un hygromètre seroit parfait: Premièrement, si ses variations étoient assez étendues pour rendre sensibles les plus petites différences d'humidité et de sécheresse.

"2. Si elles étoient assez promptes pour suivre pas-à-pas toutes celles de l'air, and pour indiquer toujours exactement son état actuel.

"3. Si l'instrument étoit toujours d'accord avec lui-même, c'est-à-dire, qu'au retour du même état de l'air il se retrouvât toujours au même degré.

"4. S'il étoit comparable; c'est-à-dire, si plusieurs hygromètres construits séparément sur les mêmes principes, indiquoient toujours le même degré dans les mêmes circonstances.

"5. S'il n'étoit affecté que par l'humidité ou la sécheresse proprement dites.

"6. Enfin, si ces mêmes variations étoient proportionnelles à celles de l'air, en-sorte que dans des circonstances pareilles, un nombre double ou triple de degrés indiquât constamment une quantité double ou triple de vapeurs."

One observation more upon the verbal criticism of the learned editors I cannot refrain from making. "L'expression de *température constituante de la vapeur*," say they, "dont se sert l'auteur, nous semble peu clair, et prêter à l'équivoque; car c'est plutôt la température *destructive* de la vapeur, celle qui la

	MEAN PRESSURE OF ATMOSPHERE, *and Extremes.*				MEAN PRESSURE O[F] VAPOUR, *and Extremes.*				[MEAN TEM]PERATURE, [and] *Extremes.*		
	Half Quarter.	Quarter.	Half Year.	Year.	Half Quarter.	Quarter.	Half Year.		[Qu]arter.	Half Year.	Year.
Sept. Oct. Nov. 1819.	30.51 29.98 29.60				0.635 0.429 0.207						
	30.39 29.73 29.18	30.51 29.87 29.18			0.458 0.262 0.156	0.635 0.355 0.156			8 42		
Dec. January, February,	30.59 29.73 29.26				0.415 0.219 0.090						
	30.20 29.79 28.89	30.59 29.76 28.89	30.59 29.81 28.69		0.363 0.225 0.139	0.415 0.222 0.090	0.635 0.288 0.090		3 29	74 40 46 35 11	
March, April, May, 1820.	30.29 29.80 28.87				0.429 0.268 0.124						
	30.54 29.95 29.16	30.54 29.88 28.87			0.490 0.333 0.168	0.490 0.300 0.124			8½ 42		
June, July, August.	30.46 30.01 29.60				0.721 0.421 0.263						
	30.26 29.91 29.39	30.46 29.96 29.39	30.54 29.92 28.87	30.59 29.87 28.87	0.655 0.473 0.328	0.721 0.448 0.263	0.721 0.374 0.124	0. 03 0.	52	89 55½ 62 47 24	89 48 54 41 11

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condense en liquide qu'on observe dans le cas cité." Now, I must confess, that the expression which I have adopted, (although I am far from wishing to maintain that it is the very best that might have been selected,) appears to me at least as correct as that which my critics propose to substitute; for, considering that it is but a mathematical line which divides the point at which vapour begins to exist, from the point at which it begins to condense, he must have microscopic eyes indeed that can discern the division; and supposing the "*température destructive* de la vapeur," to be accurately represented by 60° of the thermometric scale, he will be an accurate mathematician indeed who will represent the "*température constituante*" by any nearer sign.

And now, expressing my acknowledgments to the Genevese philosophers for their admission of my new instrument, "peut fournir au physician qui le possède, le désir et les moyens de l'employer, tant à la démonstration des principaux phénomènes de l'évaporation, qu'à l'étude plus approfondie des singulières et importantes modifications de la vapeur aqueuse, considérée dans l'air et dans le vide," I shall proceed to contribute my endeavours for the accomplishment of this desirable purpose.

The annexed Table shows at one view the principal results of the twelve-month's experiments, divided for the facility of comparison into periods of half-years, quarters, and half-quarters. It will be easily understood from inspection, and will require but little explanation. The means of the different periods are represented by the larger figures, and the extremes by the smaller, shewing the range of the several instruments in the respective intervals.

It may be observed, that the barometric results of the first quarter do not exactly correspond with those already published in the *Journal.* This is owing to a correction which I have applied in consequence of a defect in the first barometer which I employed. I have since made use of a very excellent instrument of large dimensions of the syphon form, with which I compare all others that I have occasion to employ.

The correction alluded to amounts to +0.1 inches. It may also be remarked, that the calculated means do not rigorously correspond with each other: this is in consequence of the decimal calculations having been carried on further in some instances than others; and fractions, which it was necessary to leave out of the shorter periods, have become appreciable in the longer, the means of the latter having been taken from the whole series of experiments, and not from the means of the former.

The use of the Table will be perhaps better understood from the following comparison of the quarters. Beginning with the three months, December, January, and February, it will be observed, that the mean of the barometer during that period was at its lowest, while its range was greatest. The quantity and pressure of vapour least, and the variation also least. The degree of dryness and rate of evaporation were likewise both at their minimum. The quantity of rain nearly the smallest. The temperature lowest, and the range of the thermometer least. During the quarter comprising the three months of June, July, and August, on the contrary, the mean of the barometer was at its highest, and its range was least,—the quantity and pressure of the vapour was greatest,—the degree of dryness greatest,—rate of evaporation greatest,—and quantity of rain greatest. The mean temperature was also at a maximum. It may farther be remarked, that the mean of these two extreme quarters is nearly the mean in all respects of the whole year.

The intermediate quarters, March, April, and May, and September, October, and November, vary respectively very little from the annual mean. The autumn, however, is marked by more vapour, more rain and a less degree of dryness than the spring, and it is during this period that the range of the thermometer is greatest.

From the average of the whole year we find that the degree of dryness in the afternoon exceeds that of ten o'clock in the morning by $1\frac{1}{2}^{\circ}$, while the degree of dryness of the night falls short of the same by 4°. The evaporation of morning, noon, and night, are respectively as 41 — 52 — 8, and the weight of vapour in the space of a cubic foot is less at night by 0.07 gr. than in the afternoon*.

For the sake of comparison, I have selected five periods, during which I have been enabled to try experiments contemporaneous with those in London, at a distance of twenty miles in the country. It would be tedious to give the comparison at length, I shall therefore only state the mean results.

The constant difference of the barometer between the two places was 0.09 lower in the country.

		Temper.	Vapour.	Dryness.
Mar. 31st, to April 6th, 7 days,—	London,	52½	45	7½
	Country,	53	44	9
May 21st, to 31st, 11 days,—	London,	58	50	8
	Country,	59	52	7
June 12th, to June 19th, 8 days,—	London,	58½	50	8½
	Country,	52	45½	6½
Aug. 5th, to Aug. 7th, 3 days,—	London,	63½	54½	9
	Country,	60½	56½	4
Aug. 26th, to Aug. 28th, 3 days,—	London,	57½	52	5½
	Country,	56½	52½	7½

It will be seen from this table that the mean quantities of vapour at these two stations, during these periods, corresponded within a fraction of a degree, too small to name.

* The editors of the *Bibliothèque Universelle* object to me, that I appear to have been guided by no consideration in the selection of the periods of the day for the performance of my experiments. But obliged as I have been to snatch a few moments from other avocations, it was necessary to consider at what times I should be least likely to meet with interruption so as to be able to continue the series in a regular manner. If I have not chosen quite the most proper periods, (and that I have not I am most willing to allow,) the want of academic leisure must plead my excuse; and I only trust that those who are fortunate enough to have it in their power to devote their whole time to the pursuits of science, and who have more ability for the task, will complete the plan which I have only been enabled to trace.

My opportunities of trying experiments upon heights have still been very limited; so limited, indeed, that I dare not attempt to draw any inferences from their results. I shall content myself, therefore, with noting the particulars of the few that have been within my power.

On the 3d of April I had an opportunity of ascending Leith-hill, in Surrey, which is about 800 feet above the level of the valley of Mickleham, the place from whence I set out. The barometer, at the lower station, stood at 30.296, the hygrometer denoted the temperature of the air 60°, and the point of deposition 51°. On the top of the hill the barometer stood at 29.406, and the hygrometer marked 55° and 46°; making a difference in the pressure of the whole atmosphere of 0.890, and in the pressure of the vapour 0.060.

On the 22d May, I again ascended the same hill.

Barometer at first station	30.178	Hygrometer..	71-56
second station	29.348	——————..	66-47
Difference of barometric pressure			0.830
Ditto of hygrometric ditto			0.119

A third time, on the 14th of August.

Barometer at first station	29.948	Hygrometer..	72-61
second station	29.055	——————..	70-52
Difference of barometric pressure			0.893
Ditto of hygrometric ditto........................			0.141

On the 4th of April I ascended to the highest point of Hedley-heath, a hill in the same vicinity, about 600 feet above the valley.

Barometer at first station	30.050	Hygrometer	58-34
at second station	29.370	——————	59-32
Difference of barometric pressure			0.680
Ditto of hygrometric ditto			0.014

On the 7th April the same stations gave as follows:

Barometer at first station	29-582	Hygrometer	46-34
at second station....	28.964	————	44-31
Difference of barometric pressure			0.618
Ditto of hygrometric ditto.........................			0.021

These results, it will be seen, are too discordant, and the differences are too small, to throw any light upon the laws of the decrease of the aqueous atmosphere at different elevations, a point of the highest interest and importance. We learn, however from them that, in settled weather, such as that in which these last experiments were made, the vapour does decrease in density as we ascend. In showery weather, however, this is not always the case, for I have seen several instances, when there has been denser vapour upon the hill-top than in the plain below, a state of circumstances which, as far as I have been able to observe, has always been quickly followed by falling weather.

I shall now conclude these observations with a series of experiments, which I took the opportunity of making during the great eclipse on the 7th September, for the purpose of ascertaining what effect this rare phænomenon might have upon the temperature and pressure of the gaseous atmosphere in general, and of the aqueous atmosphere in particular. It had been remarked at Edinburgh, that, during the great eclipse of 18th February, 1736-7, it was very cold, and that a little thin snow fell, and it was not unreasonable to suppose that a sudden obscuration of so large a portion of the sun's rays might produce a very sensible change in the state of the atmospheric vapour.

The day was altogether very favourable to the purpose; the morning was hazy, and there were a few cirri; the wind was S.E., and brisk. The following observations were made previous to the commencement of the eclipse:

Clock.	Thermometer.	Dew Point.	Barometer.
10¾	65	51	30.12
11¼	65	51	
11¾	68	51	
12¼	67½	51	

Very shortly after the commencement it was observed that the wind died away to a calm, and the smoke drove from the S.W., with a great tendency to beat down. The clouds increased rapidly round the sun, assuming the form of cirro-cumuli, the haze at the same time became more dense. They continued to increase, and at intervals totally obscured the view, till about fifty minutes past one, when they began to dissolve, and at 20 minutes past two the sun was again perfectly clear, and remained so till the end. The barometer, as far as I could judge, was unusually steady during the whole time. The observations were continued every quarter of an hour, as follow :

Clock.	Thermometer.	Dew Point.
12½	67½	51
12¾	67½	51
1	66	52
1¼	65	51
1½	64	50
1¾	64	50
2	63½	53
2¼	63	52
2½	63	51
2¾	63½	52
3	64½	52
3¼	65	52
5	65	52

Thus it appears that there was a depression of temperature amounting to 5°, the maximum of which was 25 minutes, after the greatest obscuration. There was also a sensible vacillation of the point of precipitation ; but whether this, and the momentary increase of the clouds, were occasioned by an accidental shift of the wind, or whether all were dependent upon the change of temperature, consequent to the obscurations of the sun's light, may be matter of some doubt. The change of the wind was permanent, and at eleven o'clock at night the constituent temperature of the vapour had risen to 55°, the barometer still continuing at 30.12.

Towards the end of the eclipse, when the disc of the sun was quite free from clouds, I had a good experiment upon the power of the sun's rays. I directed, upon some gun-powder, the focus of a small lens, which had very little more than sufficient power to ignite it in clear sunshine; it was exactly at three-quarters past two that it took fire, 55 minutes from the greatest obscuration, and thirty-two minutes before the end of the eclipse. This effect would appear to be very much greater than the corresponding one produced upon the thermometer; and I could not but observe that the impression upon the feeling was likewise much more than would have been expected from the fall in that instrument. This latter sensation was probably owing to the great increase in the radiation of the body, as was the former effect to the decrease of absorption, both surfaces being instantly sensible to the diminution of the sun's energy, while the air only felt the influence of bodies which had been primarily affected *.

* A practical illustration of the unfitness of hygroscopic substances for the construction of accurate instruments, may be found in the *Annals of Philosophy* for the last month. It consists of the following memorandum, appended to the Meteorological Journal, so accurately and ably kept by Mr. Howard.

"The mean of the hygrometer is, for the latter seventeen days of the month, 79.35, but the mean deduced for the like space, from a new one now substituted for it, is 64.88; it appears, therefore, that the old one, the inaccuracy of which has been heretofore stated, will require a deduction of fourteen degrees from its later results. It appears to have been employed from about midsummer, 1819, and the error, which has apparently arisen from the stretching of a too slender piece of whalebone, has probably increased from that time to the present."

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Date. 1820.	Hour.	Moon's Age.	Pressure. Of the whole Atmosphere.	Pressure. Continuous rise and fall of Barometer.	Pressure. Of the Vapour.	Temperature. Of the Air.	Temperature. Of the Vapour.	Temperature. Difference.	Weight in grs. of Vapour in the space of a cubic foot. As expanded by the existing temperature.	Weight. Maximum at the temperature of the Vapour.	Weight. Maximum at the temperature of the Air.	Temperature. Highest.	Temperature. Lowest.	Temperature. Of a good radiator on the ground.	Quantity of rain.	De Luc's Hygrometer. Dry.	De Luc's Hygrometer. Moist.	Force of evaporation in grs. from a surface 6 ins. diameter.	Wind. Direction.	Wind. Force.	Prevailing Clouds.	Observations.
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17		18	19	20	21	22
June 1	10		29.60	....	0.351	56	48	8	3.884	3.940	5.068	..	..	....	1.22	..	45	0.68	NW	stormy	stratus and scud	Heavy showers.
	4		29.60	....	0.263	57	40	17	2.905	2.999	5.235	60	..	....	....	..	..	1.33	W	high	ditto ditto	Clearing—fine.
	10		29.61	....	0.294	51	43	8	3.274	3.326	4.330	..	47	42	....	..	44	0.48	....	brisk	ditto ditto	Overcast.
2	10		29.62	....	0.305	56	44	12	3.375	3.452	5.068	..	..	....	0.32	..	45	0.78	....	ditto	cumuli	Fine—but threatening.
	4		29.65	....	0.375	58	50	8	4.135	4.195	5.402	60	..	....	....	..	..	0.46	....	little	ditto cirri	Some showers—fine.
	10	☽	29.71	...	0.339	50	47	3	3.791	3.815	4.195	..	42	32	....	..	45	0.14	....	calm	nimbi and ditto	Fine.
3	10		29.80	....	0.351	50	48	2	3.926	3.940	4.195	..	..	....	....	..	45	0.10	....	ditto	stratus	Gentle rain.
	4		29.84	....	0.401	56	52	4	4.436	4.468	5.068	56	..	....	....	..	..	0.23	....	little	ditto	Hard rain.
	10		29.89	....	0.328	50	46	4	3.669	3.699	4.195	..	46	41	0.07	..	48	0.19	....	ditto	ditto and cumuli	Overcast.
4	10		29.95	....	0.294	51	43	8	3.275	3.326	4.330	..	..	....	....	.	47	0.37	NW	little	cumuli and nimbi	A few drops of rain.
	4		29.95	....	0.316	51	45	6	3.528	3.570	4.330	55	..	....	0.01	..	..	0.29	....	calm	ditto ditto	Ditto ditto.
	10		29.99	....	0.316	48	45	3	3.547	3.570	3.940	..	40	30	....	..	48	0.14	....	ditto	ditto ditto	Gloomy and overcast.
5	10		30.01	....	0.328	56	46	10	3.629	3.699	5.068	..	..	....	....	..	48	0.52	W	little	cumulo-strati	Dull morning.
	4		30.01	+0.41	0.388	66	51	15	4.212	4.330	6.912	66	..	....	....	..	..	0.99	....	ditto	ditto and stratus	Threatening.
	11		29.93	....	0.458	59	56	3	5.073	5.068	5.570	..	47	47	....	..	48	0.20	....	ditto	dark and overcast	Incessant rain from eight o'clock.
6	10		29.88	−0.13	0.375	52	50	2	4.176	4.195	4.468	..	..	....	0.25	..	48	0.14	W	brisk	stratus and scud	Small rain.
	4		30.02	....	0.458	56	56	..	5.068	....	....	59	..	....	....	..	..	....	....	little	ditto ditto	Showers and very dull,
	11		30.13	+0.25	0.316	49	45	4	3.541	3.570	4.068	.	42	34	....	..	47	0.19	N	ditto	none	Fine, but stars—dull.
7	10		30.08	....	0.339	59	47	12	3.727	3.815	5.570	..	..	....	....	..	50	0.67	NW	ditto	cumuli and stratus	Rather heavy.
	5		30.05	....	0.328	61	46	15	3.594	3.699	5.950	63	..	....	....	..	..	0.86	....	ditto	ditto ditto	Dull—but clearer.
	11		30.03	....	0.316	50	45	5	3.535	3.570	4.195	..	46	39	....	..	49	0.24	N	ditto	ditto ditto	Few stars—clear in the N.
8	10		29.97	....	0.415	62	53	9	4.538	4.616	6.126	..	..	....	....	..	49	0.58	NW	little	cumulo-strati	Overcast and dull.
	5		29.94	....	0.443	65	55	10	4.826	4.910	6.614	63	..	....	....	..	..	0.69	....	ditto	ditto	Ditto ditto—showers.
	11		29.87	....	0.443	58	55	3	4.884	4.910	5.402	.	54	52	....	..	51	0.19	....	ditto	ditto	Ditto ditto.
9	10		29.83	....	0.443	60	55	5	4.862	4.910	5.761	..	..	....	....	..	51	0.33	....	ditto	ditto and stratus	Ditto ditto ditto,
	5		29.74	....	0.443	61	55	6	4.854	4.910	5.950	66	..	....	0.06	..	..	0.40	....	ditto	ditto ditto	Ditto ditto ditto.
	11		29.73	−0.40	0.339	50	47	3	3.791	3.815	4.195	..	41	33	....	..	50	0.14	....	ditto	ditto ditto	Ditto ditto
10	10		29.77	....	0.339	63	47	16	3.700	3.815	6.310	..	..	....	..	..	50	0.95	....	ditto	ditto ditto	Ditto ditto
	5	●	29.79	+0.06	0.273	60	41	19	2.995	3.106	5.761	63	..	....	....	..	..	1.30	....	brisk	ditto ditto and cirrus	Ditto ditto
	10		29.75	....	0.316	50	45	5	3.535	3.570	4.195	..	41	33	....	..	51	0.24	....	little	none	Very fine.
11	10		29.66	....	0.294	53	43	10	3.263	3.326	4.616	..	..	....	....	..	50	0.62	....	brisk	stratus and scud	Dull.
	5		29.62	−0.17	0.401	54	52	2	4.452	4.468	4.770	61	..	....	0.02	..	..	0.11	SW	little	ditto ditto	Hard rain.
	10		29.63	....	0.375	50	50	..	4.195	....	....	..	48	....	....	..	52	....	....	ditto	ditto ditto	Ditto ditto—dull.
12	10		29.78	....	0.401	58	52	6	4.420	4.468	5.402	..	..	....	0.03	..	53	0.36	N	little	cumulo-strati	Fine, but not clear.
	4		29.85	....	0.375	63	50	13	4.095	4.195	6.310	64	..	....	....	..	..	1.06	....	brisk	cumuli and ditto	Fine.
	10		29.93	....	0.339	50	47	3	3.791	3.815	4.195	..	40	30	...	..	53	0.14	....	little	none	Clear starlight.
13	10		29.94	+0.32	0.388	58	51	7	4.276	4.330	5.402	..	..	....	....	..	55	0.41	NW	ditto	cumulo-strati	Much blue sky, but not clear.
	4		29.91	−0.03	0.458	57	56	1	5.059	5.068	5.235	61	..	....	0.63	..	..	0.07	....	ditto	cumuli and nimbi	Very heavy rain, and thunder.

	10		29.96	....	0.388	51	51	..	4.330	....	....	..	45	40	....	..	54	....	....	ditto	cumuli and nimbi	Very threatening.
June 14	10		30.19	+0.28	0.375	54	50	4	4.164	4.195	4.770	..	..	....	....	..	55	0.21	....	ditto	cumulo-strati and stratus..	Dark and dull.
	4		30.12	....	0.363	59	49	10	3.992	4.068	5.570	59	..	....	....	..	..	0 58	....	ditto	ditto ditto	Ditto ditto.
	11		30.04	....	0.375	53	50	3	4.172	4.195	4.616	..	48	31	....	..	54	0.11	....	ditto	ditto ditto	Ditto—gentle rain.
15	10		29.88	−0.31	0.429	60	54	6	4.715	4.770	5.761	..	..	....	....	..	54	0.39	....	ditto	cirro-cumuli, cumuli & nimbi	Overcast.
	4		29.96	....	0.401	59	52	7	4.408	4.468	5.570	63	..	....	....	..	..	0.43	....	ditto	ditto ditto	Ditto and very dark.
	10		30.00	....	0.339	49	47	2	3.798	3.815	4.068	..	51	44	....	..	54	0.09	....	calm	stratus in the horizon	Moon clear.
16	10		30.01	+0.13	0.363	61	49	12	3.978	4.068	5.950	..	..	....	....	..	55	0.72	W	little	cumuli and cirri	Fine.
	5		29.99	....	0.388	67	51	16	4.205	4.330	7.013	69	..	....	....	..	..	1.07	....	ditto	ditto ditto	Ditto.
	10		29.98	−0.03	0.429	58	54	4	4.736	4.770	5.402	..	48	40	....	..	55	0.25	....	calm	cumulo-strati	Ditto.
17	10		30.01	....	0.363	62	49	13	3.971	4.068	6.126	..	..	....	....	..	54	0 79	NW	little	cumuli and cirri	Dull—but fine.
	5		30.01	....	0.388	67	51	16	4.205	4.330	7.013	69	..	....	....	..	..	1.07	....	ditto	ditto and cumulo-strati	Ditto ditto.
	10		30.05	....	0.429	58	54	4	4.736	4.770	5.402	..	48	40	....	..	53	0.25	....	calm	ditto ditto	Clouds breaking.
18	10		30.11	+0.13	0.328	62	46	16	3.585	3.699	6.126	..	..	....	....	..	57	0.93	....	little	ditto ditto	Very cloudy.
	4	☾	30 05	....	0.363	67	49	18	3.936	4.068	7.013	68	..	....	0.54	..	..	2.01	....	brisk	ditto ditto	Dull—with little sun.
	10		29.95	....	0.388	57	51	6	4.284	4.330	5.235	..	53	50	....	..	51	0.35	SW	little	ditto ditto	Threatening and dull.
19	10		29.79	−0.32	0.388	63	51	12	4.234	4.330	6.310	..	..	....	....	..	57	0.99	NW	brisk	ditto ditto	Fine, but cloudy.
	5		29.80	....	0.351	63	48	15	3.832	3.940	6.310	67	..	....	0.09	..	..	1.18	....	ditto	ditto ditto	Ditto ditto.
	11		29.80	+0.01	0.305	53	44	9	3.393	3.452	4.616	..	49	44	....	..	51	0.69	....	high	ditto ditto	Ditto ditto.
20	10		29.71	−0.09	0.388	56	51	5	4.291	4.330	5.068	..	..	....	....	..	54	0.36	W	brisk	stratus	Gentle rain.
	5		29.82	....	0.443	61	55	6	4.854	4.910	5.950	63	..	....	....	..	..	0.40	SW	ditto	ditto and cumulo-stratus	Showery.
	10		29.87	....	0.401	54	52	2	4.452	4.468	4.770	..	45	36	....	..	54	0.11	NW	calm	ditto ditto	Ditto.
21	10		29.92	....	0.375	62	50	12	4.103	4.195	6.126	..	..	....	0.08	..	54	0 96	....	brisk	cumuli	Very fine.
	4		29.99	....	0.429	66	54	12	4.665	4.770	6.912	63	..	....	....	..	..	0 83	....	little	cumulo-strati	Ditto ditto.
	11		30.03	....	0.388	52	51	1	4.319	4.330	4.468	..	46	38	....	..	52	0.05	....	calm	cirri	Ditto—much dew.
22	9		30.09	....	0.458	62	56	6	5.010	5.068	6 126	..	..	....	....	..	55	0.41	SW	little	stratus and haze	Very misty.
	4		30.10	....	0.474	75	57	18	5.063	5.235	9 091	76	..	....	....	..	..	1.50	....	ditto	ditto ditto	Hazy—sunshine—very close.
	11		30.12	....	0.507	60	59	1	5.565	5.570	5.761	..	51	40	....	..	55	0.07	....	calm	ditto ditto	Moon hazy.
23	10		30.20	....	0.474	66	57	9	5.147	5.235	6.912	..	..	....	....	..	55	0.64	W	gentle	cum.-strati&cirro-cum.&cir.	Very fine.
	4		30.20	....	0.578	77	63	14	6.153	6.310	9 688	79	..	....	....	..	..	1.34	SW	calm	cirri	Beautifully clear.
	10		30.23	....	0.524	65	60	5	5.710	5.761	6 614	..	56	46	....	..	56	0.36	....	ditto	none	Perfectly fine.
24	10		30.27	....	0.524	74	60	14	5.616	5.761	8.807	..	..	....	....	..	55	1.19	S	little	few cumuli	Very fine.
	4		30.29	....	0.616	84	65	19	6.389	6.614	11.981	85	..	....	....	..	..	2.10	NW	calm	none	Perfectly fine.
	11		30.33	....	0.542	68	61	7	5.877	5.950	7.316	..	60	55	....	..	52	0.53	....	ditto	none	Ditto ditto.
25	10		30.40	....	0.560	77	62	15	5.963	6.126	9.688	..	..	....	....	..	52	1.41	SE	ditto	none	Ditto ditto.
	4		30.40	....	0.655	87	67	20	6.763	7.013	12.966	87	..	....	....	..	..	2.30	N	ditto	cirri	Ditto ditto.
	10	●	30.43	....	0.578	73	63	10	6.915	6.310	8.533	..	63	53	....	..	52	0.87	W	ditto	ditto	Ditto ditto.
26	10		30.44	+0.73	0.655	81	67	14	6.838	7.013	10.993	..	..	....	....	..	53	1.54	NW	little	ditto	Ditto ditto.
	4		30 43	−0.01	0.721	89	70	19	7.513	7.776	13.756	89	..	....	....	..	..	2.28	....	calm	none	Ditto ditto.
	11		30.44	....	0.542	69	61	8	5.806	5.950	7.541	..	62	52	....	..	51	0.62	NE	ditto	none	Ditto ditto.
27	10		30 44	....	0 635	79	66	13	6.753	6.912	10.292	..	..	....	....	..	52	1.34	....	little	cirro-cumuli	Very fine.
	5		30.46	+0.03	0 721	83	70	13	7.599	7.776	11.590	86	..	....	....	..	..	1.52	NE	ditto	cumulo-strati	Very fine—oppressive.
	11		30.34	....	0.655	69	67	2	6.988	7.013	7.541	..	68	59	....	..	54	0.17	....	calm	none	Ditto ditto.
28	10		30.33	....	0.655	85	67	18	6.787	7.013	12.252	..	..	....	....	..	51	2.06	W	ditto	haze	Ditto ditto.
	5		30.24	....	0.597	84	64	20	6.274	6.506	11.980	85	..	....	....	..	..	2.17	NE	ditto	cirri	Very fine.
	11		30.23	....	0.560	69	62	7	6.051	6.126	7.541	..	59	56	....	..	53	0.71	....	brisk	cirro-strati	Ditto ditto. [3 hours 4 o'clock.
29	10		30.19	....	0.524	65	60	5	5 710	5.761	6 614	..	..	....	....	..	53	0.46	SE	ditto	stratus and haze	Cool and fresh—hard rain 4 miles from London, for
	4		30.06	....	0.578	71	63	8	6.222	6.310	8 027	73	..	....	....	..	..	0.85	....	ditto	cirro-strati and haze	Very fine.
	11		30.03	....	0.490	58	58	..	5.402	....	....	..	56	53	....	..	53	....	....	ditto	stratus	Summer lightning—hard rain in the night.
30	10		29.99	....	0.560	65	62	3	6.093	6.126	6.614	..	..	....	....	..	54	0.35	E	high	ditto and cumulo-stratus	Overcast—some rain.
	3		29.94	−0.52	0.524	62	60	2	5.740	5.761	6.126	70	..	....	....	..	..	0.22	....	ditto	ditto ditto	Overcast.
	10		30.09	....	0.401	55	52	3	4.444	4.468	4.910	..	50	45	....	..	52	0.22	....	brisk	ditto ditto	Ditto and colder.
July 1	9		30.21	....	0.339	61	47	14	3.713	3 815	5.950	..	..	....	....	..	52	1.04	....	ditto	cumulo-strati	Very fine and fresh.
	4		30.28	....	0.363	62	49	13	3.971	4.068	6.126	67	..	....	....	..	..	0.79	NE	little	ditto	Fine.
	10		30.30	+0.36	0.339	53	47	6	3.770	3.815	4.616	..	44	37	....	..	53	0.30	....	calm	ditto	Ditto—stars dim.
2	10	☽	30.19	....	0.401	62	52	10	4.385	4.468	6.126	..	..	....	0.16	..	54	0.64	W	little	stratus	A few drops of rain.
	4		30.09	....	0.490	60	58	2	5.378	5.402	5.761	68	..	....	....	..	..	0.14	SW	ditto	ditto and scud	Hard rain.
	10		30.06	....	0.363	52	49	3	4.042	4.068	4.468	..	45	40	....	..	54	0.15	....	ditto	stratus	Clouds breaking.
3	10		30.00	....	0.388	60	51	9	4.256	4.330	5.761	..	..	....	....	..	54	0.55	....	ditto	ditto	Hard showers.
	6		29.89	−0.41	0.474	60	57	3	5.203	5.235	5.761	66	..	....	0.12	..	..	0.20	NW	little	cumulo-strati	Showery.
	10		29.91	....	0.443	55	55	..	4.910	4.910	4.910	..	52	49	....	..	53	....	....	ditto	ditto	Overcast and damp.

METEOROLOGICAL JOURNAL—*continued.*

Date. 1820.	Hour.	Moon's Age.	Pressure. Of the whole Atmosphere.	Pressure. Continuous rise and fall of Barometer.	Pressure. Of the Vapour.	Temperature. Of the Air.	Temperature. Of the Vapour.	Temperature. Difference.	Weight in grs. of Vapour in the space of a cubic foot. As expanded by the existing temperature.	Weight in grs. of Vapour. Maximum at the temperature of the Vapour.	Weight in grs. of Vapour. Maximum at the temperature of the Air.	Temperature. Highest.	Temperature. Lowest.	Temperature. Of a good radiator on the ground.	Quantity of rain.	De Luc's Hygrometer. Dry.	De Luc's Hygrometer. Moist.	Force of evaporation in grs., from a surface 6 ins. diameter.	Wind. Direction.	Wind. Force.	Prevailing Clouds.	Observations.
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17		18	19	20	21	22
July 4	9		29.97	....	0.401	61	52	9	4.393	4.468	5.950	..	..	....	....	..	52	0.57	NE	little	cumuli & cumulo-strati	Fine—clouds heavy.
	5		30.01	....	0.328	60	46	14	3.600	3.699	5.761	70	..	....	....	..	..	0.79	....	ditto	cumulo-strati	Fine.
	11		30.05	....	0.388	54	51	3	4.308	4.330	4.770	..	53	45	....	..	52	0.16	....	calm	overcast	Dull.
5	9		30.12	+0.23	0.339	60	47	13	3.720	3.815	5.761	..	..	....	....	..	50	0.74	NE	little	cumulo-strati	Fine.
	5		30.11	−0.01	0.363	58	49	9	4.002	4.068	5.402	61	..	....	....	..	..	0.51	....	ditto	ditto	Dull all day.
	10		30.14	....	0.351	51	48	3	3.919	3.940	4.330	..	48	39	....	..	50	0.15	....	calm	none	Very fine—stars bright.
6	8		30.17	+0.06	0.375	58	50	8	4.135	4.195	5.402	..	..	....	....	..	52	0.46	....	little	cumulo-strati	Very fine.
	5		30.14	−0.03	0.401	62	52	10	4.361	4.468	6.126	64	..	....	....	..	..	0.82	....	brisk	ditto	Fine, but cloudy.
	11		30.16	+0.02	0.429	55	54	1	4.761	4.770	4.910	..	52	45	....	..	53	0.06	....	calm	ditto	Ditto ditto.
7	9		30.13	−0.03	0.401	59	52	7	4.404	4.468	5.570	..	..	....	....	..	53	0.43	....	little	ditto	Ditto ditto.
	5		30.16	....	0.415	64	53	11	4.522	4.616	6.506	65	..	....	....	..	..	0.73	N	ditto	ditto	Ditto ditto
	11		30.19	+0.06	0.375	55	50	5	4.157	4.195	4.910	..	53	47	....	..	52	0.27	....	calm	overcast	Few—stars and dull.
8	10		30.18	....	0.351	58	48	10	3.870	3.940	5.402	..	..	....	....	..	51	0.56	NE	little	cumulo-stratus and haze	Overcast and fine.
	5		30.17	−0.02	0.339	59	47	12	3.723	3.815	5.570	62	..	....	....	..	..	0.67	....	ditto	ditto ditto	Ditto ditto.
	11		30.19	....	0.351	55	48	7	3.891	3.940	4.910	..	52	50	....	..	52	0.37	....	ditto	overcast	Very dull.
9	10		30.19	+0.02	0.388	60	51	9	4.257	4.330	5.761	..	..	....	....	..	52	0.55	E	calm	stratus&cum.-stratushaze	Overcast, but fine.
	5		30.15	−0.04	0.443	62	55	7	4.845	4.910	6.126	65	..	....	....	..	..	0.47	....	little	ditto ditto	Overcast and dull.
	11		30.16	+0.01	0.443	57	55	2	4.893	4.910	5.235	..	54	50	....	..	52	0.13	....	ditto	ditto ditto	Ditto ditto, gleams of heat.
10	10	●	30.14	...	0.375	61	50	11	4.109	4.195	5.950	..	..	....	....	..	53	0.67	SE	calm	cumulo-stratus and haze	Overcast, but fine.
	5		30.10	....	0.401	64	52	12	4.371	4.468	6.506	68	..	....	....	..	..	0.79	....	little	ditto ditto	Fine and warmer.
	11		30.09	−0.07	0.388	55	51	4	4.301	4.330	4.910	..	50	44	....	..	52	0.22	....	calm	none	Stars very bright.
11	10		30.10	+0.01	0.401	65	52	13	4.362	4.468	6.614	..	..	....	....	..	51	0.86	E	little	cirro-strati and haze	Fine—dim sunshine.
	4		30.01	...	0.443	66	55	11	4.811	4.910	6.912	67	..	....	....	..	..	0.77	....	ditto	cumulo-strati and haze	Hazy sunshine.
	11		30.01	...	0.429	56	54	2	4.753	4.770	5.068	..	50	44	....	..	53	0.12	....	ditto	cirri	Very fine.
12	10		29.96	....	0.443	62	55	7	4.845	4.910	6.126	..	..	....	....	..	53	0.47	....	ditto	cumulo-strati	Overcast, but fine.
	4		29.88	....	0.443	64	55	9	4.828	4.910	6.506	67	..	....	....	..	..	0.79	....	brisk	ditto and stratus	Ditto ditto.
	11		29.86	.. :	0.443	57	55	2	4.893	4.910	5.235	..	53	52	....	..	52	0.15	....	ditto	overcast	Very dull and close.
13	10		29.83	−0.17	0.429	61	54	7	4.707	4.770	5.950	..	..	....	....	..	52	0.59	....	ditto	cumulo-strati	Overcast and dull.
	5		29.84	....	0.415	61	53	8	4.546	4.616	5.950	63	..	....	....	..	..	0.66	....	ditto	ditto ditto	Ditto ditto.
	11		29.85	....	0.401	55	52	3	4.444	4.468	4.910	..	52	49	....	..	51	0.22	....	ditto	ditto ditto	Ditto ditto.
14	10		29.87	....	0.474	65	57	8	5.157	5.235	6.614	..	..	....	....	..	52	0.73	....	ditto	ditto heavy	Fine—sun at intervals.
	4		29.90	....	0.507	68	59	9	5.487	5.570	7.316	69	..	....	....	..	..	0.67	SE	calm	ditto	Ditto ditto.
	11		29.92	....	0.542	62	61	1	5.940	5.950	6.126	..	59	57	....	..	55	00.7	....	ditto	overcast	Very oppressive, some drops of rain.
15	10		29.97	....	0.524	65	60	5	5.710	5.761	6.614	..	..	....	....	..	59	0.36	SW	calm	mist and stratus	Overcast and oppressive.
	4		30.00	....	0.578	73	63	10	6.195	6.310	8.533	78	..	....	....	..	..	0.87	....	little	cumuli and cirri	Fine.
	10		30.03	+0.20	0.524	61	60	1	5.751	5.761	5.950	..	53	45	....	..	55	00.7	....	calm	none	Clear star-light.
16	10		29.99	....	0.560	72	62	10	6.014	6.126	8.270	..	..	....	....	..	55	1.08	W	brisk	stratus	Very dark.
	5		29.96	....	0.655	75	67	8	6.910	7.013	9.091	76	.	....	....	..	..	0.50	SE	ditto	ditto and nimbi	Thunder-showers—very oppressive.

	10		29.92	....	0.616	66	65	1	6.601	6.614	6.912	..	60	56	....	..	56	0.08	....	little	overcast	Hot and oppressive.
17	10		29.66	....	0.655	68	67	1	7.001	7.013	7.316	..	..	....	....	..	58	0.40	....	brisk	stratus	Hard rain.
	4		29.56	....	0.635	66	66	..	6.912	....	....	75	..	....	0.20	..	..	....	....	ditto	ditto	Hard rain all day.
	11		29.52	....	0.490	59	58	1	5.387	5.402	5.570	..	54	47	....	..	60	0.07	NW	little	none	Very fine.
18	10		29.40	....	0.542	61	61	..	5.950	....	....	..	..	....	....	..	60	.. .	W	ditto	stratus	Steady pouring rain.
	4	☾	29.39	−0.64	0.560	62	62	..	6.126	....	....	74	..	....	0.43	..	..	....	SW	little	stratus and scud	Pouring showers.
	11		29.41	....	0.474	57	57	..	5.235	....	....	..	53	48	....	..	59	....	....	ditto	ditto ditto	Moon dull—few stars.
19	10		29.45	....	0.524	69	60	9	5.671	5.761	7.541	..	..	....	....	..	60	0.69	W	ditto	cumuli-cirri haze	Very fine and hot.
	4		29.54	....	0.542	61	61	7	5.877	5.950	7.316	72	..	....	0.74	..	..	0.68	....	brisk	ditto ditto and nimbus	Very fine—oppressive—hard showers.
	11		29.61	....	0.507	59	59	..	5.570	....	....	..	55	48	....	..	58	....	....	calm	few—mist	Fine.
20	10		29.69	....	0.524	70	60	10	5.659	5.761	7.776	..	..	....	....	..	59	0.78	....	little	cirri-cumuli, haze	Very fine and hot.
	5		29.72	....	0.524	71	60	11	5.650	5.761	8.027	72	..	....	....	..	..	1.13	....	brisk	ditto ditto ditto	Ditto and very oppressive—thunder shower.
	11		29.81	..	0.542	62	61	1	5.939	5.950	6.126	..	55	52	....	..	59	0.07	....	calm	few	Fine—stars bright—showers.
21	10		29.89	....	0.524	63	60	3	5.730	5.761	6.310	..	..	....	....	..	68	0.21	NW	ditto	stratus and mist	Very close and oppressive.
	5		29.91	....	0.524	68	60	8	5.684	5.761	7.316	70	..	....	0.42	..	..	0.60	....	little	cumuli stratus and mist	Shower—very fine.
	11		29.94	....	0.524	62	60	2	5.740	5.761	6.126	..	54	46	....	..	57	0.14	....	calm	overcast	Close and oppressive.
22	10		29.97	....	0.401	70	52	18	4.324	4.468	7.776	..	..	....	....	..	54	1.64	....	brisk	cumulo-strati	Very fine and exhilarating.
	5		29.97	....	0.401	64	52	12	4.370	4.468	6.506	70	..	....	....	..	..	1.01	....	ditto	ditto and cumuli	Overcast.
	10		29.97	....	0.401	58	52	6	4.420	4.468	5.402	..	54	51	....	..	54	0.36	....	little	overcast	A few drops of rain.
23	10		30.00	....	0.401	58	52	6	4.420	4.468	5.402	..	..	....	....	..	54	0.36	....	ditto	cumuli and cumulo-strati	Fine.
	4		30.00	....	0.429	63	54	9	4.690	4.770	6.310	64	..	....	....	..	..	0.60	....	ditto	ditto ditto	Ditto.
	10		30.02	....	0.415	57	53	4	4.586	4.616	5.235	..	52	46	....	..	54	0.24	....	ditto	ditto ditto	A few drops of rain.
24	10		30.03	....	0.458	65	56	9	4.984	5.068	6.614	..	..	....	....	..	55	0.63	....	ditto	ditto ditto	Fine.
	4		30.01	+0.62	0.542	74	61	13	5.811	5.950	8.807	75	..	....	....	..	..	1.12	....	ditto	ditto ditto	Ditto.
	10		29.99	....	0.507	64	59	5	5.525	5.570	6.506	..	59	55	....	..	56	0.36	....	ditto	ditto ditto	Ditto.
25	10		29.90	−0.11	0.443	69	55	14	4.786	4.910	7.541	..	..	....	....	..	58	1.31	NW	brisk	ditto ditto	Very fine.
	5	○	29.91	....	0.401	65	52	13	4.362	4.468	6.614	73	..	....	....	..	..	1.10	....	brisk	cumuli and cumulo-strati	Fine—light showers.
	11		29.94	....	0.388	57	51	6	4.284	4.330	5.235	..	53	46	....	..	52	0.35	....	little	cumulo-strati	Fine.
26	9		30.05	....	0.388	71	51	20	4.176	4.330	8.027	..	..	....	0.03	..	53	1.84	NW	brisk	cumulo-strati	Very fine.
	5		30.07	....	0.474	71	57	14	5.102	5.235	8.027	75	..	....	....	..	..	1.08	....	little	ditto	Ditto ditto.
	10		30.08	+0.18	0.524	62	60	2	5.741	5.761	6.126	..	61	54	....	..	55	0.14	....	calm	none	Perfectly fine.
27	10		30.06	....	0.542	72	61	11	5.830	5.950	8.270	..	..	....	....	..	57	0.91	W	little	cumulo-strati and mist	Very fine and oppressive.
	5		30.01	−0.07	0.542	74	61	13	5.810	5.950	8.807	76	..	....	....	..	..	1.44	....	brisk	ditto ditto	Ditto ditto
	11		30.02	....	0.524	62	60	2	5.741	5.761	6.126	..	58	51	....	..	54	0.14	....	little	cirri	Very fine—moon bright.
28	10		30.07	+0.06	0.542	77	61	16	5.781	5.950	9.688	..	..	....	....	..	57	1.48	....	ditto	cumulo-strati and haze	Very fine and oppressive.
	5		30.03	−0.04	0.542	74	61	13	5.810	5.950	8.807	78	..	....	....	..	..	1.12	....	ditto	ditto ditto and cirri	Ditto ditto,
	11		30.07	....	0.524	64	60	4	5.721	5.761	6.506	..	61	53	....	..	53	0.29	....	ditto	cirro-cumuli	Fine.
29	10		30.10	....	0.524	71	60	11	5.651	5.761	8.027	..	..	....	....	..	54	0.88	....	ditto	light stratus and mist	Fine and oppressive.
	5		30.13	....	0.507	78	59	19	5.388	5.570	9.992	79	..	....	....	..	..	1.73	....	ditto	cumuli	Very fine.
	10		30.13	+0.10	0.524	67	60	7	5.691	5.761	7.013	..	58	47	....	..	55	0.52	....	ditto	ditto	Ditto ditto.
30	10		30.12	....	0.542	73	61	12	5.821	5.950	8.533	..	..	....	....	..	54	1.01	SW	calm	cumuli and haze	Ditto ditto.
	5		30.07	....	0.560	75	62	13	5.983	6.126	9.091	79	..	....	....	..	..	1.16	S	ditto	ditto and nimbi	Clouds very heavy.
	10		29.99	....	0.597	66	64	2	6.483	6.506	6.912	..	58	51	....	..	56	0.15	....	ditto	ditto ditto	Thunder storm—very violent
31	10		29.90	....	0.560	72	62	10	6.014	6.126	8.270	..	..	....	0.41	..	56	0.84	SE	little	cirri and cirro-cumuli	Fine.
	4		29.90	....	0.597	81	64	17	6.312	6.506	10.993	83	..	....	....	..	..	2.28	SW	brisk	ditto ditto	Ditto.
	10		29.89	....	0.635	72	66	6	6.834	6.912	8.270	..	63	57	....	..	55	0.54	....	little	cumulo-strati	Ditto.
Aug. 1	10	☾	29.86	−0.27	0.507	68	59	9	5.487	5.570	7.316	..	..	....	....	..	55	0.86	....	brisk	....	Ditto.
	5		29.88	....	0.490	72	58	14	5.262	5.402	8.270	74	..	....	....	..	..	1.44	W	brisk	cirri and scud	Ditto.
	11		29.99	....	0.542	61	61	..	5.950	....	....	..	58	53	....	..	51	....	....	little	few and light	Moon sharp and bright.
2	10		30.12	+0.26	0.443	72	55	17	4.757	4.910	8.270	..	..	....	....	..	53	1.31	W	little	cumulo-strati	Very fine—exhilarating.
	5		30.08	....	0.415	71	53	18	4.467	4.616	8.027	76	..	....	....	..	..	1.70	....	brisk	ditto few	Ditto ditto.
	11		30.08	....	0.542	63	61	2	5.929	5.950	6.310	..	62	55	....	..	52	0.14	SE	little	overcast	Dull and close.
3	10		29.99	....	0.635	73	66	7	6.822	6.912	8.533	..	..	....	....	..	56	0.82	SW	brisk	cumulo-strati, strati & haze	Overcast and oppressive.
	3		29.92	....	0.635	72	66	6	6.834	6.912	8.270	76	..	....	....	..	..	0.69	....	ditto	ditto ditto ditto	Dull, and sprinklings of rain.
	10		29.84	....	0.578	66	63	3	6.277	6.310	6.912	..	65	63	....	..	57	0.36	.. .	ditto	stratus and scud	Sprinklings of rain—clearing.
4	10		29.75	−0.37	0.597	71	64	7	6.427	6.505	8.027	..	..	....	....	..	59	1.16	....	ditto	ditto ditto and haze	Hard rain and thunder in the night—fine.
	4		29.82	....	0.597	71	64	7	6.427	6.506	8.027	74	..	....	....	..	..	0.59	....	little	cumuli	Threatening.
	11		29.86	....	0.474	60	57	3	5.203	5.235	5.761	..	53	51	....	..	55	0.20	....	calm	ditto	Smart and short showers.
5	10		29.88	....	0.415	65	53	12	4.514	4.616	6.614	..	..	....	0.70	..	54	1.03	....	brisk	ditto	Fine.
	4		29.90	....	0.415	68	53	12	4.491	4.616	7.316	71	..	....	....	..	..	1.63	....	high	ditto and stratus	Beautifully clear.
	11		29.92	+0.17	0.474	59	57	2	5.211	5.235	5.570	..	57	55	....	..	55	0.13	....	calm	ditto ditto	Hard rain—dark.

METEOROLOGICAL JOURNAL—*continued.*

Date. 1820.	Hour.	Moon's Age.	Pressure. Of the whole Atmosphere.	Pressure. Continuous rise and fall of Barometer.	Pressure. Of the Vapour.	Temperature. Of the Air.	Temperature. Of the Vapour.	Temperature. Difference.	Weight in grs. of Vapour in the space of a cubic foot. As expanded by the existing temperature.	Weight in grs. of Vapour in the space of a cubic foot. Maximum at the temperature of the Vapour.	Weight in grs. of Vapour in the space of a cubic foot. Maximum at the temperature of the Air.	Temperature. Highest.	Temperature. Lowest.	Temperature. Of a good radiator on the ground.	Quantity of rain.	De Luc's Hygrometer. Dry.	De Luc's Hygrometer. Moist.	Force of evaporation in grs. from a surface 6 ins. diameter.	Wind. Direction.	Wind. Force.	Prevailing Clouds.	Observations.
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17		18	19	20	21	22
Aug. 6.	10		29.74	....	0.401	68	52	16	4.339	4.468	7.316	..	.	..	..	..	59	1.73	SW	brisk	cirro-cumuli & cumulo-strat.	Very fine.
	4		29.65	— 0.27	0.507	68	59	9	5.487	5.570	7.316	68	..	..	0.11	..	..	1.05	....	high	ditto and stratus	Incessant rain.
	10		29.67	....	0.474	59	57	2	5.211	5.235	5.570	..	56	52	..	..	55	0.21	....	ditto	ditto ditto	Showers—clearing.
7	10		29.80	....	0.458	65	56	9	4.984	5.068	6.614	..	..	..	0.12	..	55	0.81	W	brisk	ditto ditto	Fine.
	4		29.94	....	0.415	67	53	14	4.498	4.616	7.013	70	..	..	..	..	..	0.96	....	little	cumuli and ditto	Ditto, but overcast.
	10		30.00	....	0.388	56	51	5	4.291	4.330	5.068	..	49	41	..	..	53	0.28	....	calm	ditto ditto	Ditto—stars dim.
8	10		30.02	+ 0.37	0.429	66	54	12	4.665	4.770	6.912	..	..	..	..	..	55	1.07	....	brisk	cirri and cirro-strati	Fine.
	5	●	29.88	....	0.401	68	52	16	4.339	4.468	7.316	69	..	..	..	..	..	1.41	SW	ditto	cirro-cumuli & cumulo-strat.	Very fine and fresh.
	11		29.83	— 0.19	0.443	61	55	6	4.853	4.910	5.950	..	58	51	..	..	53	0.63	....	high	overcast	Dull.
9	10		29.97	....	0.415	72	53	19	4.455	4.616	8.270	..	..	..	..	..	55	1.83	W	brisk	cumulo-strati	Very fine.
	5		30.05	....	0.388	71	51	20	4.175	4.330	8.027	73	..	..	..	..	..	1.84	....	ditto	none	Perfectly fine—exhilarating.
	11		30.15	....	0.388	58	51	7	4.276	4.330	5.402	..	54	47	..	..	53	0.41	....	little	none	Ditto ditto.
10	10		30.26	+ 0.43	0.474	67	57	70	5.139	5.235	7.013	..	..	..	..	..	55	1.02	....	ditto	haze	Very fine.
	5		30.23	....	0.474	71	57	14	5.105	5.235	8.027	73	..	..	..	..	..	1.08	....	ditto	ditto and cumulo-strati	Ditto ditto.
	11		30.23	— 0.03	0.542	63	61	2	5.928	5.950	6.310	..	57	47	..	..	53	0.14	SW	ditto	haze and cirro-strati	Fine—stars—hazy
11	10		30.26	....	0.524	68	60	8	5.671	5.761	7.316	..	..	..	..	..	56	0.60	....	ditto	haze and light stratus	Very fine.
	4		30.26	+ 0.03	0.578	76	63	13	6.163	6.310	9.385	76	..	..	..	..	..	1.21	NW	ditto	ditto	Ditto ditto.
	11		30.25	....	0.507	64	59	5	5.525	5.570	6.506	..	53	43	..	..	54	0.36	....	ditto	none	Ditto ditto.
12	9		30.23	....	0.474	65	57	8	5.156	5.235	6.614	..	..	..	..	..	54	0.56	....	ditto	ditto	Ditto ditto.
	4		30.20	....	0.578	74	63	11	6.184	6.310	8.807	75	..	..	..	..	..	0.98	....	ditto	ditto	Ditto ditto.
	11		30.18	....	0.415	53	53	..	4.616	....	....	..	55	54	..	..	53	....	....	ditto	cirro-strati	Ditto ditto—foggy smell.
13	10		30.15	....	0.429	63	54	9	4.690	4.770	6.310	..	..	..	..	..	52	0.60	....	ditto	ditto	Ditto ditto.
	5		30.10	....	0.524	74	60	14	5.616	5.761	8.807	75	..	..	..	..	..	1.19	....	ditto	cumulo-strati	Ditto ditto.
	10		30.10	....	0.490	58	58	..	5.402	....	....	..	55	45	..	..	54	....	S	ditto	none	Ditto ditto.
14	10		30.04	....	0.524	70	60	10	5.660	5.761	7.776	..	..	..	..	..	59	0.78	SW	ditto	cumuli and cirri	Ditto ditto.
	4		30.00	....	0.351	77	48	29	3.736	3.940	9.688	77	..	..	..	..	..	2.25	W	ditto	ditto ditto	Beautifully clear.
	10		29.97	....	0.458	56	56	..	5.068	....	....	..	55	44	..	..	52	....	....	calm	none	Ditto ditto.
15	9		29.90	....	0.429	56	54	2	4.753	4.770	5.068	..	..	..	..	..	52	0.12	....	ditto	cirro-cumuli and haze	Ditto ditto.
	5		29.80	....	0.616	75	65	10	6.494	6.614	9.091	77	..	..	..	..	..	1.21	SW	brisk	stratus and cumulo-strati	Fine and close.
	11		29.80	....	0.524	65	60	5	5.710	5.761	6.614	..	64	60	0.02	..	56	0.46	....	ditto	overcast	Dull and close.
16	10		29.79	— 0.47	0.542	71	61	10	5.846	5.950	8.027	..	..	..	..	..	58	1.04	....	ditto	stratus and scud	Overcast and dull—some drops.
	5		29.83	....	0.560	75	62	13	5.979	6.126	9.091	76	..	..	..	..	..	1.49	....	ditto	ditto ditto	Ditto ditto.
	12		29.86	+ 0.07	0.616	68	65	3	6.579	6.614	7.316	..	65	60	..	..	57	0.31	....	ditto	ditto ditto	Ditto—stars at intervals, and showers.
17	10	☾	29.79	....	0.524	71	60	11	5.650	5.761	8.027	..	..	..	..	..	58	1.38	W	high	stratus and cumulo-stratus	Overcast, but fine.
	5		29.78	— 0.08	0.616	72	65	7	6.527	6.614	8.270	75	..	..	..	..	..	0.97	....	ditto	ditto ditto	Very fine.
	11		29.85	....	0.415	60	53	7	4.555	4.616	5.761	..	57	49	..	..	54	0.44	....	little	few	Fine and fresh.
18	10		29.86	....	0.401	62	52	10	4.385	4.468	6.126	..	..	..	..	..	52	0.64	....	calm	light stratus and haze	Fine.
	4		29.86	+ 0.08	0.388	70	51	19	4.183	4.330	7.776	72	..	..	..	..	..	1.33	....	little	cumulo-strati and ditto	Ditto.

	10		29.84	....	0.375	59	50	9	4.124	4.195	5.570	..	50	42	..	..	49	0.53	....	calm	cumulo-strati and ditto	Fine.
Aug. 19	10		29.78	−0.08	0.401	63	52	11	4.377	4.468	6.310	..	..	..	..	..	52	0.71	....	little	cumuli	Very fine.
	4		29.79	....	0.458	62	56	6	5.010	5.068	6.126	68	..	..	..	..	..	0.41	SW	ditto	ditto and nimbi	Thunder shower.
	11		29.84	....	0.351	51	48	3	3.919	3.940	4.330	..	42	35	..	..	53	0.10	NW	calm	none	Perfectly fine.
20	10		29.92	....	0.375	59	50	9	4.124	4.195	5.570	..	..	..	..	..	..	0.53	...	little	cirro-strati	Ditto ditto.
	3		29.93	....	0.339	64	47	17	3.694	3.815	6.506	65	..	..	..	..	..	1.03	....	calm	cumuli	Very fine.
	11		29.94	+0.06	0.351	56	48	8	3.884	3.940	5.068	..	51	42	..	..	..	0.43	....	ditto	cirri	Halo round the moon.
21	10		29.87	....	0.474	58	57	1	5.229	5.235	5.402	..	..	..	..	..	..	0.06	NE	little	stratus	Beginning to rain.
	4		29.84	....	0.490	58	58	..	5.402	....	....	60	50	50	..	..	..	..	....	brisk	ditto very dark	Steady rain.
	10		29.84	....	0.415	53	53	..	4.616	....	....	..	..	..	..	..	..	..	....	high	ditto ditto	Ditto ditto.
22	..		29.82	....	0.388	58	51	7	4.276	4.330	5.402	..	..	..	0.42	..	..	0.64	....	ditto	ditto ditto	Overcast and dull.
	5		29.82	−0.12	0.388	57	51	6	4.284	4.330	5.235	59	..	..	..	..	..	0.54	NE	high	cumulo-strati	Overcast and dark.
	11		29.90	....	0.351	51	48	3	3.919	3.940	4.330	..	47	42	..	..	..	0.24	....	brisk	cirro-cumuli	Fine.
23	10		30.09	....	0.339	59	47	12	3.723	3.815	5.570	..	..	..	..	..	..	0.86	....	ditto	cumulo-strati	Very fine and exhilarating.
	5		30.12	....	0.328	59	46	13	3.309	3.699	5.570	60	..	..	..	..	..	0.93	....	ditto	ditto ditto	Overcast and dull.
	11		30.14	....	0.388	53	51	2	4.315	4.330	4.616	..	52	48	..	..	..	0.11	....	little	ditto	Fine, but overcast.
24	10	○	30.15	+0,33	0.328	63	46	17	3.581	3.699	6.310	..	..	..	..	..	..	1.00	SW	ditto	light stratus and haze	Very fine.
	5		30.12	....	0.351	65	48	17	3.818	3.940	6.614	67	..	..	..	..	..	1.06	....	ditto	cirri and cumuli	Ditto ditto.
	11		30.05	....	0.388	54	51	3	4.307	4.330	4.770	..	51	42	..	..	..	0.16	....	calm	none	Stars and moon bright.
25	10		29.88	....	0.443	61	55	6	4.853	4.910	5.950	..	.	..	..	..	..	0.51	....	brisk	stratus	Overcast and dull.
	4		29.87	....	0.560	65	62	3	6.093	6.126	6.614	67	..	..	..	..	..	0.28	....	ditto	ditto	Ditto ditto
	10		29.80	....	0.542	61	61	..	5.950	....	....	..	53	50	..	..	..	..	....	ditto	ditto	Steady rain.
26	10		29.73	....	0.401	63	52	11	4.377	4.468	6.310	..	..	..	0.5	..	..	0.93	....	ditto	cumuli	Clearing.
	4		29.73	−0.42	0.524	60	60	..	5.761	....	....	68	..	..	..	..	..	..	....	ditto	ditto and stratus	Showery.
	10		29.75	....	0.429	55	54	1	4.762	4.770	4.910	..	47	42	..	..	..	0.08	....	ditto	cumulo-strati	Fine.
27	10		29.77	....	0.339	58	47	11	3.736	3.815	5.402	..	..	..	..	..	..	0.77	....	ditto	ditto and cumuli	Ditto.
	5		29.77	+0.04	0.524	60	60	..	5.761	....	....	66	..	..	0.16	..	..	..	....	ditto	ditto ditto	Showers—hard rain.
	11		29.71	....	0.429	57	54	3	4.744	4.770	5.235	..	51	47	..	..	..	0.23	....	ditto	stratus and ditto	Rain.
28	10		9.55	−0.22	0.363	59	49	10	3.992	4.068	5.570	..	..	..	0.02	..	..	0.58	W	ditto	cumuli	Fine.
	4		29.60	....	0.363	60	49	11	3.985	4.068	5.761	63	..	..	..	..	..	0.65	NW	ditto	ditto	Ditto.
	11		29.67	....	0.339	49	47	2	3.798	3.815	4.068	..	43	36	..	..	..	0.09	....	little	none	Ditto.
29	10		29.70	....	0.363	59	49	10	3.992	4.068	5.570	..	..	..	..	..	..	0.58	....	ditto	cumuli	Ditto.
	5		29.72	....	0.339	63	47	16	3.700	3.815	6.310	69	..	..	..	..	..	0.95	NW	little	cumuli and cirro-cumuli ..	Very fine.
	11		29.79	....	0.375	54	50	4	4.164	4.195	4.770	..	48	39	..	..	..	0.21	....	ditto	cumulo-strati	Overcast.
30	10	☽	29.91	....	0.351	61	48	13	3.815	3.940	5.950	61	..	..	..	..	..	0.77	....	ditto	ditto and haze	Very fine.
	5		29.94	....	0.388	58	51	7	4.276	4.330	5.402	..	..	..	..	..	.	0.41	....	ditto	ditto and stratus, and ditto	Fine, but overcast.
	11		30.01	....	0.375	51	50	1	4.187	4.195	4.330	..	47	39	..	..	..	0.05	....	ditto	stratus in the horizon	Fine.
31	10		30.06	....	0.429	62	54	8	4.698	4.770	6.126	..	..	..	..	..	..	0.53	W	ditto	cumuli&cumulo-strati&haze	Very fine.
	5		30.07	....	0.388	58	51	7	4.276	4.330	5.402	64	..	..	..	..	..	0.53	N	brisk	cirro-strati	Ditto ditto.
	11		30.09	+0.54	0.375	50	49	1	4.060	4.068	4.195	..	47	39	..	..	..	0.05	E	little	none	Moon and stars perfectly bright.

Art. XIV. **METEOROLOGICAL DIARY** for the Months of June, July, and August, 1820, kept at Earl Spencer's Seat at Althorp, in Northamptonshire. The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

For June, 1820.

		Thermometer		Barometer		Wind	
		Low	High	Morn.	Eve.	Morn.	Eve.
Thursday	1	42	62,5	29,39	29,39	W	WbS
Friday	2	46	65	29,39	29,48	SW	W
Saturday	3	43	59	29,57	29,61	W	W
Sunday	4	46	57	29,74	29,78	W	WbN
Monday	5	37	67	29,84	29,80	W	W
Tuesday	6	48	58,5	29,76	29,93	WNW	NE
Wednesday	7	44	64	29,93	29,89	W	WNW
Thursday	8	43	63	29,84	29,80	WbW	NW
Friday	9	52	60,5	29,70	29,62	NW	NW
Saturday	10	41,5	60	29,62	29,62	WNW	WNW
Sunday	11	38,5	57,5	29,46	29,40	SW	SW
Monday	12	45	64	29,59	29,75	NE	NE
Tuesday	13	38	67	29,75	29,75	W	NW
Wednesday	14	48	62	29,92	29,92	NNE	W
Thursday	15	49	62,5	29,70	29,77	NW	N
Friday	16	44	61	29,80	29,80	W	W
Saturday	17	49	71	29,83	29,86	W	W
Sunday	18	45	67	29,90	29,78	WbN	SW
Monday	19	52	69	29,60	29,60	SW	W
Tuesday	20	49	62	29,48	29,67	W	WbN
Wednesday	21	44	69	29,73	29,80	WbN	W
Thursday	22	43	70	29,98	30,00	WbS	W
Friday	23	49	72	30,00	30,00	W	W
Saturday	24	45	82	30,09	30,09	W	WSW
Sunday	25	52	85	30,20	30,20	W	W
Monday	26	55	83,5	30,27	30,27	W	NW
Tuesday	27	56	86	30,27	30,25	E	NNE
Wednesday	28	58	84,5	30,22	30,12	W	WbN
Thursday	29	57	76	30,07	29,97	EbS	ENE
Friday	30	57	66,5	29,89	29,90	NE	NE

For July, 1820.

		Thermometer		Barometer		Wind	
		Low	High	Morn.	Eve.	Morn.	Eve.
Saturday	1	47	67	30,10	30,12	NNE	NE
Sunday	2	44,5	66,5	30,05	29,90	WbS	WbN
Monday	3	47	66	29,87	29,80	WbN	WbN
Tuesday	4	50	62,5	29,84	29,90	NNW	NNW
Wednesday	5	49	67	30,00	30,00	NNE	NE
Thursday	6	45	66	30,02	30,06	NW	E
Friday	7	44	67,5	30,06	30,03	NE	NE
Saturday	8	54	63	30,09	30,09	NE	NE
Sunday	9	51	65	30,07	30,06	NE	E
Monday	10	54	65	30,03	29,97	E	EbS
Tuesday	11	43	70	29,97	29,90	ESE	ESE
Wednesday	12	45	69	29,88	29,79	SE	NE
Thursday	13	50	60,5	29,77	29,77	E	ENE
Friday	14	51	66	29,79	29,79	E	EbN
Saturday	15	56	75	29,80	29,87	E	E
Sunday	16	50	72	29,87	29,79	SE	E
Monday	17	59	67,5	29,54	29,37	E	WSW
Tuesday	18	52	66	29,28	29,2J	S	W
Wednesday	19	50	70	29,40	29,50	NW	W
Thursday	20	49	75,5	29,59	29,65	W	E
Friday	21	56	73	29,77	29,81	NW	NW
Saturday	22	46	71	29,81	29,78	NW	W
Sunday	23	54	69	29,85	29,85	NW	W
Monday	24	51	70,5	29,85	29,83	W	W
Tuesday	25	58	72	29,76	29,76	W	W
Wednesday	26	49	70,5	29,87	29,90	W	W
Thursday	27	56	76	29,90	29,88	W	WbS
Friday	28	56,5	74	29,88	29,90	W	NW
Saturday	29	47	76	29,94	29,94	NW	W
Sunday	30	49	79	29,94	29,84	E	SE
Monday	31	53	82	29,76	29,70	SW	SW

For August, 1820.

		Thermometer		Barometer		Wind	
		Low	High	Morn.	Eve.	Morn.	Eve.
Tuesday	1	62	77,5	29,60	29,71	WbS	W
Wednesday	2	51	77	29,81	29,93	SW	W
Thursday	3	61	71	29,87	29,73	SW	SW
Friday	4	63,5	76	29,60	29,62	SbW	W
Saturday	5	53,5	74,5	29,70	29,70	WSW	W
Sunday	6	57	68	29,54	29,44	WbS	SW
Monday	7	55	68,5	29,52	29,79	W	W
Tuesday	8	46	70	29,84	29,69	W	W
Wednesday	9	54	74	29,76	29,94	W	W
Thursday	10	48	76	30,11	30,17	WbS	W
Friday	11	49	77,5	30,17	30,10	W	W
Saturday	12	47	76	30,10	30,05	NW	W
Sunday	13	45	74	30,05	29,90	W	W
Monday	14	48	76	29,88	29,80	W	SW
Tuesday	15	58,5	76,5	29,78	29,68	SW	SW
Wednesday	16	61,5	75,5	29,63	29,66	SW	SW
Thursday	17	65	74	29,60	29,65	SW	NW
Friday	18	53	71	29,71	29,68	NW	NW
Saturday	19	41	70,5	29,64	29,64	NW	NW
Sunday	20	42,5	69	29,74	29,74	NW	NW
Monday	21	48	63,5	29,77	29,73	NE	NE
Tuesday	22	48,5	61	29,73	29,79	NE	NE
Wednesday	23	46	62	29,90	30,00	NNW	NW
Thursday	24	36	69	30,01	29,91	NW	WbS
Friday	25	52,5	66	29,74	29,60	SW	SW
Saturday	26	52	69	29,53	29,53	W	W
Sunday	27	48	67	29,59	29,57	WbS	W
Monday	28	51	64	29,36	29,40	W	W
Tuesday	29	45,5	69	29,47	29,59	W	W
Wednesday	30	40	68	29,70	29,80	W	ESE
Thursday	31	44,[illegible]	65,5	29,93	29,98	E	SE

Art. XIV. *Astronomical and Nautical Collections.*—No. III.

i. Tables *subservient to the Calculation of* Lunar Occultations.

1. A Table *of the* Places *of all* Stars, *not below the fourth Magnitude, that are liable to* Lunar Occultations.

Extracted from Mr. Pond's Observations, Vol. II.

1820. Jan 1.				AR			Ann. Var.	N. P. D.			Ann. Var.	N. E. Angle of ☽'s Orbit with Meridian.	
				H.	M.	S.	s.	°	′	″	″	☽ asc.	☽ desc.
1	ε	71	♓	0	53	36,49	+3,11	83	4	49,0	—19,5	62°	72°
2	ζ	86	♓	1	4	19,90	3,11	83	22	41,1	19,3	62	72
3	η	99	♓	1	21	51,80	3,19	75	35	2,7	18,8	67	69
4	ν	106	♓	1	32	4,13	3,10	85	25	33,4	18,5	66	70
5	ο	110	♓	1	35	54,82	3,15	81	45	3,9	18,4	64	74
6	2ξ	73	Ceti	2	18	35,78	3,16	82	21	3,8	16,7		
7	μ	87	Ceti	2	35	13,10	3,20	80	39	3,7	15,6		
8	δ	57	♈	3	1	20,89	3,39	70	57	37,3	14,2	69	79
9	f	5	♉	3	20	56,73	3,28	77	41	11,3	12,9		
10	η	25	♉	3	36	47,43	3,53	66	27	32,7	11,8	73	79
11	A	37	♉	3	54	4,14	3,52	68	25	3,8	10,5	73	83
12	γ	54	♉	4	9	33,51	3,40	74	48	53,0	9,4		
13	1δ	61	♉	4	12	33,87	3,43	72	53	14,7	9,1	76	82
14	2δ	64	♉	4	13	43,73	3,43	72	58	51,0	9,1	77	83
15	2κ	67	♉	4	14	42,45	3,54	68	13	10,8	9,0	75	85
16	ε	74	♉	4	18	7,01	3,49	71	13	38,4	8,6	76	86
17	α	87	♉	4	25	36,02	3,43	73	51	39,2	8,0	80	81
18	ι	102	♉	4	52	20,54	3,57	68	40	33,9	5,9	78	88
19	β	112	♉	5	14	55,07	3,78	61	33	16,5	3,9	84	86
20	ζ	123	♉	5	26	53,36	3,57	68	58	35,9	3,0	81	91
21		132	♉	5	37	58,24	3,67	65	30	15,1	— 2,0	83	93
22	κ	44	Aur.	6	3	54,24	3,82	60	26	43,6	+ 0,2		
23	η	7	♊	6	4	0,64	3,63	67	27	2,2	0,2	85	95
24	μ	13	♊	6	12	3,09	3,63	67	24	13,8	1,2	86	96
25	ν	18	♊	6	18	16,35	3,55	69	40	59,7	1,5	87	95
26	ε	27	♊	6	32	51,07	3,69	64	42	1,3	2,8	88	98
27	ζ	43	♊	6	53	25,56	3,56	69	10	28,5	4,5	90	100

1820. Jan. 1.				AR.			Ann. Var.	N. P. D.			Ann. Var.	N.E. Angle of ☽'s Orbit with Meridian.	
				H.	M.	S.	s.	°	′	″	″	☽ asc.	☽ desc.
28	δ	55	♊	7	9	21,65	3,59	67	41	42,5	5,9	91	101
29	ι	60	♊	7	14	32,08	3,74	61	51	12,5	6,3		
30	υ	69	♊	7	24	49,05+	3,71	62	42	46,5+	7,1	96°	98°
31	κ	77	♊	7	33	34,03	3,63	65	10	44,1	7,9	95	103
32	γ	43	♋	8	32	51,08	3,49	67	53	27,3	12,4	100	108
33	δ	47	♋	8	34	26,42	3,43	71	11	26,2	12,7	100	110
34	1α	60	♋	8	46	4,97	3,29	77	41	30,7	13,2	104	106
35	2α	65	♋	8	48	37,81	3,29	77	27	4,3	13,5	104	106
36	ξ	5	♌	9	22	13,98	3,25	77	54	27,3	15,4	104	112
37	ο	14	♌	9	31	31,91	3,21	79	17	35,2	15,9	105	111
38	π	29	♌	9	50	41,36	3,18	81	5	44,6	16,9	106	112
39	η	30	♌	9	57	30,29	3,28	72	21	48,0	17,2	108	112
40	α	32	♌	9	58	46,43	3,21	77	9	23,0	17,3	105	115
41	ρ	47	♌	10	23	19,32	3,16	79	46	10,0	18,2	106	116
42	σ	77	♌	11	11	51,08	3,10	82	59	5,6	19,6	108	118
43	(ι	78	♌)	11	14	31,00	3,11	78	28	42,0	19,6		
44	τ	84	♌	11	18	40,61	3,09	86	9	10,3	19,7	108	118
45	υ	91	♌	11	27	43,87	3,07	89	49	48,1	19,9	109	117
46	(β	5	♍)	11	41	18,84	3,12	87	13	14,7	20,0	108	118
47	η	15	♍	12	10	41,82	3,07	89	39	52,7	20,0	108	118
48	γ	29	♍	12	32	32,52	3,02	90	27	35,9	19,9	111	117
49	ϑ	51	♍	13	0	38,17	3,09	94	34	26,5	19,0	108	118
50	α	67	♍	13	15	43,25	3,15	100	13	2,4	19,0	107	117
51	i	68	♍	13	17	13,20	3,15	101	46	2,6	19,0	108	116
52	κ	98	♍	14	3	18,18	3,17	99	24	57,0	17,2	106	114
53	λ	100	♍	14	9	22,88	3,22	102	32	10,4	17,0	105	115
54	2α	9	♎	14	40	56,08	3,30	105	17	9,2	15,2	103	113
55	4ζ	35	♎	15	22	45,78	3,36	106	13	5,91	12,8	100	110
56	γ	38	♎	15	25	28,31	3,32	104	10	48,0	12,1	102	108
57	κ	43	♎	15	31	35,29	3,43	109	5	8,1	12,2	99	109
58	λ	45	♎	15	42	53,87	3,45	109	37	8,3	11,4	98	108
59	ϑ	46	♎	15	43	35,17	3,38	106	11	29,6	11,3	107	109
60	π	6	♏	15	47	58,66	3,59	115	35	8,0	11,0	94	104
61	ψ	48	♎	15	48	7,19	3,33	103	45	0,4	11,0		

1820. Jan. 1.				AR. H.	M.	S.	Ann. Var. s.	N. P. D. °	′	″	Ann. Var. ″	N.E. Angle of ☽'s Orbit with Meridian. ☽ asc.	☽ desc
62	δ	7	♏	15	49	42,26	+3,51	112	5	55,6	+10,9	97°	107°
63	1β	8	♏	15	54	56,11	3,47	109	18	8,7	10,5	97	107
64	2β	8	♏	15	54	59,58	+3,47	109	17	55,5	+10,5	97	107
65	ν	14	♏	16	1	32,69	3,46	108	58	57,3	10,0	96	106
66	σ	20	♏	16	10	15,74	3,61	115	8	58,0	9,3	98	104
67	α	21	♏	16	18	23,01	3,66	116	1	14,7	8,6	97	103
68	φ	8	Oph.	16	20	50,67	3,41	106	12	35,5	8,5	99	101
69	τ	23	♏	16	24	41,41	3,71	117	49	50,1	8,2	—	
70	A	36	Oph.	17	4	17,43	3,70	116	19	24,5	4,9		
71	ϱ	40	Oph.	17	10	13,21	3,56	110	54	27,4	4,4	91	101
72	ϑ	42	Oph.	17	10	57,73	3,67	114	48	27,8	4,4	90	100
73	B	44	Oph.	17	15	22,77	3,64	113	59	50,0	+ 4,0	90	100
74	1μ	13	♐	18	2	59,86	3,59	111	5	37,4	—0,2	85	95
75	2μ	15	♐	18	4	28,73	3,59	110	46	11,0	0,3	85	95
76	δ	19	♐	18	9	28,02	3,84	119	53	30,0	0,7		
77	λ	22	♐	18	16	51,53	3,70	115	30	31,6	1,2	84	94
78	1ν	32	♐	18	43	17,80	3,62	112	57	15,7	3,6	81	91
79	σ	34	♐	18	44	5,88	3,73	116	30	21,5	3,6	82	90
80	ο	39	♐	18	53	53,29	3,59	111	59	35,7	4,6	81	91
81	τ	40	♐	18	55	41,89	3,75	117	55	15,8	4,7	84	86
82	π	41	♐	18	59	3,14	3,57	111	17	53,8	4,9	80	90
83	ψ	42	♐	19	4	29,57	3,68	115	33	17,7	5,5	80	88
84	d	43	♐	19	7	5,76	3,51	109	15	42,4	5,7		
85	b	59	♐	19	45	53,11	3,69	117	38	10,0	8,8		
86	β	9	♑	20	10	53,23	3,38	105	20	23,9	10,8	76	80
87	ϑ	23	♑	20	55	48,76	3,37	107	56	23,0	13,8	69	79
88	ι	39	♑	21	26	59,04	3,37	110	15	54,8	15,7	70	74
89	γ	40	♑	21	30	6,08	3,34	107	28	6,9	15,8	68	76
90	δ	49	♑	21	37	5,43	3,33	106	56	13,1	16,2	67	75
91	ϑ	43	♒	22	7	19,36	3,17	98	40	27,8	17,5	66	74
92	2τ	71	♒	22	40	3,13	3,19	104	32	48,3	18,8		
93	λ	73	♒	22	43	13,09	3,13	98	32	0,5	18,9	63	73
94	φ	90	♒	23	4	59,51	3,10	97	0	58,2	19,4	62	72
95	ω	28	♓	23	50	4,03	3,05	84	7	55,1	20,0		

2. A TABLE, *showing the Logarithms of the Corrections in Seconds, to be applied with the proper Signs of the Sines.*

STARS.	ABERRATION. AR. in Time. L. Sin. (☉'s Long. +		N. P. D. in Space. L. Sin. (☉'s Long. +		SOLAR NUTATION. AR. in Time. L. Sin. (2 ☉'s Long. +		N. P. D. in Space. L. Sin. (2 ☉'s Long. +		LUNAR NUTATION. AR. in Time. L. Sin. (☊'s Long. +		N. P. D. in Space. L. Sin. (☊'s Long. +	
	S. D. M.		S. D. M.		S. M. D.		S. D. M.		S. D. M.		S. D. M.	
1 ε ♓	8 15 49)	+.0973	2 12 48)	+.8963	6 3 9)	+8.792	11 15 27)	+9.602	6 3 50)	+.0486	11 12 45)	+.8648
2 ζ ♓	8 12 52	.0989	2 13 35	.8899	6 2 59	8.793	11 12 33	or	6 3 37	.0493	11 9 8	.8690
3 η ♓	8 8 7	.1115	1 22 59	.8925	6 6 15	8.806	11 7 52	9.62	6 7 40	.0627	11 3 43	.8759
4 ν ♓	8 5 24	.1002	2 20 46	.8822	6 1 58	8.794	11 5 9	for all.	6 2 23	.0490	11 0 37	.8803
5 ο ♓	8 4 25	.1038	2 9 17	.8705	6 3 30	8.798	11 4 8		6 4 16	.0545	10 29 28	.8820
6 2ξ Ceti	7 23 20	.1086	2 11 39	.8457	6 2 54	8.801	10 23 0		6 3 34	.0554	10 17 42	.9058
7 μ Ceti	7 19 6	.1128	2 7 18	.8182	6 3 20	8.806	10 18 46		6 4 6	.0509	10 13 4	.9175
8 δ ♈	7 12 33	.1365	1 5 54	.7586	6 5 55	8.833	10 12 12		6 7 19	.0896	10 6 39	.9303
9 f ♉	7 7 40	.1254	2 0 16	.7489	6 3 31	8.818	10 7 22		6 4 13	.0740	10 1 57	.9404
10 η ♉	7 3 49	.1556	0 16 35	.6955	6 5 58	8.851	10 3 30		6 7 22	.1078	9 28 23	.9483
11 A ♉	7 0 23	.1521	0 23 34	.6326	6 4 52	8.847	9 29 20		6 6 2	.1031	9 24 43	.9559
12 γ ♉	6 25 53	.1379	1 24 48	.6335	6 3 5	8.830	9 25 38		6 3 50	.0865	9 21 38	.9615
13 1δ ♉	6 25 10	.1426	1 26 38	.5983	6 3 22	8.836	9 24 56		6 4 11	.0927	9 20 39	.9633
14 2δ ♉	6 24 57	.1425	1 17 7	.5948	6 3 19	8.836	9 24 39		6 4 8	.0922	9 20 38	.9633
15 2κ ♉	6 24 39	.1549	0 21 54	.5595	6 4 9	8.850	9 24 24		6 5 1	.1067	9 20 23	.9642
16 ε ♉	6 21 58	.1466	1 8 53	.5606	6 3 29	8.841	9 23 36		6 4 21	.0980	9 19 49	.9652
17 α ♉	6 22 9	.1422	1 23 26	.5806	6 2 49	8.834	9 21 51		6 3 31	.0904	9 18 18	.9673
18 ι ♉	6 15 51	.1582	0 26 40	.3800	6 2 39	8.852	9 15 36		6 3 19	.1074	9 12 58	.9758
19 β ♉	6 10 39	.1844	10 21 8	.3963	6 2 20	8.914	9 10 21		6 2 58	.1328	9 8 40	.9810
20 ζ ♉	6 7 53	.1594	1 11 15	.1608	6 1 0	8.869	9 7 36		6 1 39	.1089	9 6 23	.9820

21	132	♉	6 5 16	.1709	11 9 21	.9499	6 0 59	8.936	9 5 4	6 1 27	.1208	9 4 15	.9852
22	κ	Aur.	5 29 29	.1914	8 27 22	.3284	5 29 47	8.919	8 29 6	5 29 43	.1376	8 29 28	.9841
23	η	♊	5 29 25	.1650	3 15 43	9.5507	5 29 28	8.896	8 29 5	5 29 9	.1147	8 29 31	.9847
24	μ	♊	5 27 34	.1647	4 18 54	9.6867	5 29 30	8.896	8 27 14	5 29 8	.1153	8 28 4	.9841
25	ν	♊	5 26 4	.1579	3 24 33	.0967	5 29 19	8.851	8 25 49	5 29 13	.1073	8 26 46	.9840
26	ε	♊	5 22 40	.1735	6 25 27	.1217	5 28 29	8.867	8 22 27	5 28 12	.1233	8 24 7	.9831
27	ζ	♊	5 17 56	.1582	4 25 35	.2990	5 27 57	8.851	8 17 43	5 27 31	.1078	8 20 15	.9801
28	δ	♊	5 14 21	.1614	5 12 26	.5163	5 27 10	8.854	8 14 1	5 26 24	.1118	8 16 56	.9752
29	ι	♊	5 13 9	.2005	6 29 13	.5148	5 26 12	8.874	8 12 49	5 25 13	.1307	8 16 12	.9751
30	υ	♊	5 10 42	.1772	6 13 23	.5325	5 25 50	8.872	8 10 25	5 24 56	.1269	8 14 7	.9720
31	κ	♊	5 8 34	.1724	5 28 46	.5240	5 25 48	8.821	8 8 21	5 24 53	.1180	8 12 26	.9693
32	γ	♋	4 24 27	.1503	5 7 30	.7065	5 24 58	8.846	7 24 9	5 22 42	.1030	7 29 56	.9452
33	δ	♋	4 24 6	.1407	4 24 11	.7818	5 24 51	8.837	7 23 47	5 23 41	.0938	8 0 22	.9442
34	1α	♋	4 21 10	.1250	4 0 19	.7536	5 26 21	8.816	7 20 55	5 25 49	.0739	7 27 5	.9377
35	2α	♋	4 20 33	.1249	4 1 15	.7552	5 26 14	8.817	7 20 18	5 25 30	.0742	7 26 20	.9370
36	ξ	♌	4 12 11	.1190	4 1 24	.8069	5 25 46	8.813	7 11 53	5 24 47	.0696	7 18 8	.9182
37	ο	♌	4 9 29	.1149	3 27 26	.8161	5 26 7	8.809	7 9 31	5 25 12	.0648	7 15 49	.9148
38	π	♌	4 4 53	.1088	3 22 26	.8315	5 26 33	8.803	7 4 37	5 25 43	.0597	7 10 48	.9017
39	η	♌	4 3 7	.1236	4 17 44	.8575	5 23 6	8.818	7 2 50	5 21 13	.0771	7 8 49	.8979
40	α	♌	4 2 49	.1141	4 4 24	.8414	5 24 56	8.803	7 2 30	5 23 45	.0661	7 8 32	.8972
41	ρ	♌	3 26 18	.1069	3 26 38	.8671	5 25 44	8.802	6 26 4	5 24 39	.0584	7 1 28	.8850
42	σ	♌	3 13 30	.0981	3 18 1	.8983	5 26 46	8.792	6 13 5	5 25 59	.0487	6 16 26	.8645
43	ι	♌	3 12 35	.1034	3 29 9	.9139	5 24 42	8.796	6 12 22	5 23 26	.0531	6 15 17	.8628
44	τ	♌	3 11 30	.0950	3 9 56	.9025	5 28 13	8.789	6 11 14	5 27 44	.0451	6 14 10	.8624
45	υ	♌	3 9 0	.0935	3 0 44	.9058	5 29 56	8.785	6 8 47	5 29 50	.0426	6 11 13	.8602
46	β	♍	3 5 25	.0934	3 7 13	.9052	5 28 41	8.786	6 5 5	5 28 20	.0431	6 6 29	.8563
47	η	♍	2 27 21	.0932	3 1 5	.9064	5 29 50	8.786	5 27 6	5 29 45	.0426	5 26 50	.8577
48	γ	♍	2 21 24	.0935	2 29 8	.9058	6 0 14	8.786	5 21 0	6 0 13	.0429	5 15 12	.8610

A TABLE *of Corrections*, continued.

STARS.	ABERRATION. AR. in Time. (S. M. D.)	L. Sin. (☉'s Long. +	ABERRATION. N. P. D. in Space. (S. D. M.)	L. Sin. (☉'s Long. +	SOLAR NUTATION. AR. in Time. (S. D. M.)	L. Sin. (2 ☉'s Long. +	SOLAR NUTATION. N. P. D. in Space. (S. D. M.)	L. Sin. (2 ☉'s Long. +	LUNAR NUTATION. AR. in Time. (S. D. M.)	L. Sin. (☊'s Long. +	LUNAR NUTATION. N. P. D. in Space. (S. D. M.)	L. Sin. in ☊'s Long. +
49 ϑ ♍	2 13 47)	+.0972	2 18 52)	+.8931	6 2 6)	+8.783	5 13 32)	+9.62	6 2 30)	+.0467	5 10 20)	+.8675
50 α ♍	2 9 44	.1041	2 4 2	.8847	6 4 44	8.777	5 9 30		6 5 30	.0548	5 5 39	.8760
51 i ♍	2 9 25	.1062	1 29 55	.8885	6 5 29	8.775	5 9 6		6 6 18	.0584	5 5 9	.8744
52 κ ♍	1 27 19	.1094	2 6 19	.8495	6 4 0	8.771	4 26 57		6 4 34	.0610	4 21 43	.8963
53 λ ♍	1 25 39	.1143	1 27 22	.8452	6 5 21	8.765	4 25 23		6 5 55	.0661	4 18 32	.9007
54 2α ♎	1 17 31	.1246	1 19 4	.7908	6 6 6	8.754	4 17 19		6 6 27	.0771	4 11 33	.9186
55 4ζ ♎	1 6 41	.1337	1 16 24	.7172	6 5 32	8.744	4 6 55		6 5 35	.0854	4 1 8	.9418
56 γ ♎	1 6 30	.1294	1 24 11	.7251	6 4 40	8.749	4 6 16		6 4 52	.0796	4 1 0	9.422
57 κ ♎	1 5 3	.1418	1 4 46	.6899	6 6 25	8.734	4 4 45		6 6 13	.0939	3 29 29	.9447
58 λ ♎	1 2 20	.1452	1 2 57	.6621	6 6 13	8.730	4 2 1		6 5 58	.0968	3 27 5	.9507
59 ϑ ♎	1 2 9	.1369	1 17 41	.6760	6 4 55	8.740	4 1 51		6 4 56	.0870	3 26 58	.9513
60 π ♏	1 1 4	.1744	0 7 7	.6826	6 8 30	8.708	4 0 47		6 7 27	.1157	3 25 57	.9527
61 ψ ♎	1 0 58	.1323	1 27 59	.6923	6 3 57	8.747	4 0 45		6 4 5	.0806	3 25 50	.9527
62 δ ♏	1 0 41	.1530	0 21 41	.6471	6 6 56	8.720	4 0 22		6 6 23	.1050	3 25 37	.9539
63 1β ♏ } 64 2β ♏ }	0 29 22	.1457	1 4 36	.6269	6 5 39	8.728	3 29 6		6 5 22	.0972	3 24 33	.9559
65 γ ♏	0 27 52	.1460	1 6 34	.6115	6 5 17	8.727	3 27 33		6 5 3	.0969	3 23 11	9.591
66 σ ♏	0 25 43	.1662	0 4 52	.6026	6 7 7	8.703	3 25 28		6 6 8	.1170	3 21 20	.9619
67 α ♏	0 23 49	.1701	11 28 56	.5853	6 6 59	8.697	3 23 32		6 5 49	.1209	3 19 37	.9652
68 φ Oph.	0 23 15	.1416	1 22 0	.5894	6 3 44	8.734	3 22 58		6 4 38	9.9914	3 19 13	.9663

69	τ	♏	0 23 35	.1784	11 19 9	.5943	6 7 17	8.688	3 22 4	6 5 56	.1271	3 18 25	.9672
70	A Oph.		0 13 4	.1769	11 11 11	.3664	6 4 4	8.685	3 12 49	6 3 19	.1243	3 9 27	.9791
71	ρ	♏	0 11 38	.1584	10 26 40	.2856	6 2 40	8.710	3 11 27	6 2 31	.1079	3 9 28	.9791
72	ϑ	♏	0 11 33	.1708	11 21 24	.2727	6 3 18	8.692	3 11 16	6 2 49	.1207	3 9 25	.9798
73	B Oph.		0 10 31	.1683	0 0 0	.2095	6 2 53	8.696	3 10 15	6 2 31	.1201	3 9 27	.9791
74	1μ	♐	11 29 33	.1603	3 3 20	9.9304	6 29 50	8.707	2 29 19	5 29 55	.1099	2 29 35	.9847
75	2μ	♐	11 29 18	.1597	3 6 15	9.9840	5 29 46	8.708	2 28 52	5 29 51	.1087	2 29 17	.9848
76	δ	♐	11 28 9	.1922	2 21 2	.3596	5 29 9	8.663	2 27 50	5 29 26	.1391	2 28 32	.9844
77	λ	♐	11 26 25	.1749	7 19 20	9.9597	5 28 48	8.686	2 26 8	5 29 6	.1242	2 26 59	.9835
78	1ν	♐	11 20 21	.1650	5 17 42	.1677	5 27 22	8.699	2 20 3	5 27 49	.1152	2 22 7	.9818
79	σ	♐	11 20 11	.1764	6 28 47	.2820	5 26 50	8.684	2 19 52	5 27 27	.1263	2 21 56	.9813
80	ο	♐	11 17 52	.1614	5 7 35	.2730	5 26 56	8.706	2 17 37	5 27 23	.1117	2 20 7	.9797
81	τ	♐	11 17 30	.1823	7 0 40	.4120	5 25 34	8.678	2 17 12	5 26 34	.1311	2 19 45	.9792
82	π	♐	11 16 41	.1588	5 1 57	.3216	5 26 47	8.709	2 16 24	5 27 13	.1090	2 19 5	.9788
83	ψ	♐	11 15 30	.1731	6 10 29	.3820	5 25 31	8.691	2 15 9	5 26 23	.1226	2 18 2	.9781
84	d	♐	11 14 49	.1526	4 17 30	.4106	5 26 48	8.719	2 14 33	5 27 10	.1021	2 17 32	.9768
85	b	♐	11 5 44	.1755	6 8 10	.6216	5 22 7	8.692	2 5 27	5 23 37	.1262	2 9 56	.9647
86	β	♑	10 29 50	.1351	4 8 45	.6645	5 25 33	8.742	1 29 29	5 25 32	.0851	2 4 41	.9545
87	ϑ	♑	10 18 45	.1352	4 20 50	.7466	5 23 16	8.743	1 18 30	5 23 14	.0877	1 24 35	.9334
88	ε	♑	10 14 38	.1315	4 19 19	.7777	5 21 21	8.776	1 10 40	5 22 50	.0846	1 20 38	.9248
89	γ	♑	10 10 12	.1282	4 18 33	.8090	5 22 40	8.753	1 9 53	5 22 20	.0816	1 16 10	.9131
90	δ	♑	10 8 22	.1257	4 16 38	.8191	5 22 47	8.755	1 8 6	5 22 22	.0789	1 14 19	.9095
91	ϑ	♒	10 0 29	.1071	3 22 16	.8552	5 26 15	8.753	1 0 16	5 25 37	.0573	1 6 3	.8938
92	2τ	♒	9 21 57	.1119	4 7 50	.8942	5 23 10	8.773	0 21 37	5 22 11	.0634	0 26 29	.8770
93	λ	♒	9 21 4	.1021	3 22 8	.8813	5 26 5	8.778	0 20 47	5 25 21	.0535	0 25 30	.8740
94	φ	♒	9 15 11	.0987	3 18 5	.8944	5 26 43	8.782	0 14 56	5 26 2	.0484	0 18 34	.8663
95	ω	♓	9 2 54	.0949	2 16 3	.9219	6 2 47	8.786	0 2 42	6 3 22	.0423	0 5 12	.8576

3. Occultations *for the different Places of the* Moon's Node.

Selected from Bode, Berl. Jahrb. 1780.

☊ ☊ in ♓, XI^s.

7° ε♓, ηf, β♉ κAu. ιυ♊, γ ♋, στ ♌ βαι ♍, π, σα ♏, τ ♐, γδ♑, λφ ♒

24 ε♓, ηf, β♉, κ Au. ιυ♊, γ♋, ϱστ ♌ β, αι♍, π, σατ♏, τ ♐, γδ♑, λφ ♒

21 ε♓, ηf, β♉, κ Au. ιυ♊, γ♋, ϱστ ♌ β, αι ♍; π, σατ ♏, τ ♐, γδ♑, λφ ♒

18 ε♓, ηf, β♉, κ Au. υκ♊, γ♋, ϱτ ♌ β, αι ♍, π, σατ ♏, τ ♐, γδ♑, λφ ♒

15 , ηf, β♉, υκ♊, γ♋α, ϱτ ♌, αι ♍, π, σατ♏, στ ♐, γδ♑, λφ ♒

12 , ηf, β♉, υκ♊, γ♋α, ϱτ ♌, αι ♍, π, σατ ♏, στ ♐, γδ♑, λφ ♒

9 , ηf, β♉, κ♊, γ♋α, ϱτ ♌, ι♍, π, σατ ♏, στψ ♐, γδ♑, λ♒

6 , ηf, β♉, κ♊, γ♋α, ϱτ ♌, ι♍, π, σατ♏, στψ ♐, γδ♑, λ♒

3 , ηf, β♉, κ♊, γ♋α, ϱτ ♌, ι♍, π, σατ♏, στψ ♐, , λ♒

0 , ηf, β♉, κ♊, γ♋α, ϱτυ ♌, ι♍, π, σατ♏, σψ ♐, ϑ♑, λ♒

☊ ☊ in ♒, X^s.

27° , ηf, β♉, κ♊, α, ϱυ ♌, ι♍, π, σατ♏, σψ ♐, ϑ♑, ,

24 , ηf, β♉, κ♊, α, ϱυ ♌, ι♍, π, σατ♏ A Oph., σψ ♐, ϑ♑, ,

21 , ηf, β♉, κ♊, δ♋α, ϱυ ♌, ι♍, π, σα♏ A Oph., σψ ♐, ϑ♑, ,

18 , ηf, β♉, εκ♊, δ♋α, υ ♌, ι♍, π, σα♏ A Oph., σψ ♐, ϑ♑, ,

15 , ηf, β♉, εκ♊, δ♋, υ ♌, , π, σα♏ A Oph., λσψ ♐, ϑ♑, ,

12 , ηf ♉, , εκ♊, δ♋, υ ♌, , π, σα♏ A Oph., λσψ ♐, ϑ♑, ϑ♒

9 , ηf ♉, , εκ♊, δ♋, υ ♌, , π, σα♏ A Oph., λσψ ♐, ϑ♑, ϑ♒

6 , ηf ♉, , εκ♊, δ♋, υ ♌, , π, σα♏ A Oph., λσψ ♐, ϑ♑, ϑ♒

3 , ηf ♉, , εκ♊, δ♋, υ ♌, , π, σα♏ A Oph., λψ ♐, ϑ♑, ϑ♒

0 , ηf ♉, , εκ♊, δ♋, υ ♌, , π, σα♏ A Oph., λ ♐, , ϑ♒

☊ ☊ in ♑, IX^s.

27° , ηf ♉, , εδκ♊, δ♋, υ ♌, , π, σα♏ A Oph., λ ♐, , ϑ ♒

24 , ηf ♉, , εδκ♊, δ♋ξπ, υ ♌, , , σα♏, A ϑ Oph., λ ♐, , ϑ♒

21 , ηf, 132 ♉, εδκ♊, ξοπ ♌, , , σα♏ A ϑ Oph., λιυ ♐, , ϑ ♒

18 , ηf, 132 ♉ , ιδκ ♊ , ξοπ ♌ , , , σαϰ ♏ A ϑ Oph. , λιν ♐ , , ϑ ♒
15 , ηf, 132 ♉ , δκ ♊ , ξοπ ♌ , , , σ♏ A ϑ B Oph. λινο ♐ , , ϑ ♒
12 , ηf, 132 ♉ , δκ ♊ , ξοπ ♌ , , , σ♏ ϑ B Oph. , ινο ♐ , , ϑ ♒
9 , ηf, 132 ♉ , δκ ♊ , ξοπ ♌ , , , δ, σ♏ ϑ B Oph. , ινοπ ♐ , , ϑ ♒
6 , ηf, 132 ♉ , μδκ ♊ , ξοπ ♌ , , , δ♏, ϑ B Oph. , ινοπ ♐ , , ,
3 , ηf, 132 ♉ , ημδκ ♊ , ξοπ ♌ , , , δ♏, ϑ B Oph. , ινοπ ♐ , , ω♓
0 , f, A 132 ♉ , ημδκ ♊ , ξοπ ♌ , , , δ♏, ϑ B Oph. , ινοπ ♐ , , ω♓

☊ in ♐, VIII^s.

☊
27° , , A 132 ♉ , ημζ ♊ , ξοπ ♌ , , , δ♏, ϑ B Oph. , ινοπ ♐ , , ω♓,
24 , δ♈, A 2κ132 ♉ , ημζ ♊ , ξοπ ♌ , ι♍ , δ♏ , B Oph. , ινοπ ♐ , ,
21 , δ♈, A2κ ♉ , ημζ ♊ , ξοπ ♌ , ι♍ , δ♏, B Oph. , οπa ♐ , ,
18 , δ♈, A2κ ♉ . ημζ ♊ , ξοπ ♌, ι♍, δ♏, B Oph. οπd ♐ , β♑ , ,
15 , δ♈, A2κ ♉ , ημζ ♊ , οπ ♌, ι♍, δ♏, B Oph. , 1.2μπd ♐ , β♑ , ,
12 , δ♈, A2κι ♉ , ημνζ ♊ , οπ ♌, ι♍, δ♏, , 1.2μπd ♐ , β♑ , ,
9 , δ♈, A2κιζ ♉ , ημνζ ♊ , 1α♋οπ ♌ , ι ♍, δ ♏, ρ Oph., 1.2μπd ♐ , β ♑ , ,
6 , δ♈, A2κιζ ♉ , ηνζ ♊ , 1. 2α♋οπ ♌ , ι♍, κλ♎, ρ Oph. , 1.2μd ♐ , β♑ , ,
3 , δ♈, A2κιζ ♉ , νζ ♊ , 1.2α♋οπ ♌, ι♍ , κλ♎ , ρ Oph. , 1.2μd ♐ , β ♑ , ,
0 , δ♈ , 2κιζ ♉ , νζ ♊ , 1.2α♋οπ ♌, αι♍ , κλ ♎, β♏ ρ Oph. , 1.2μd ♐ , β♑ , ,

☊ in ♏, VII^s.

☊
27° ζ♓, δ♈, 2κιζ ♉ , ν ♊ , 1.2α♋οπ ♌ , αι♍, κλ♎, βν♏, ρ Oph. 1.2μd ♐ , β♑ , ,
24 ζ♓, , ιζ ♉ , ν ♊ , 1.2α♋οπ ♌ , , αι♍ , κλ♎, βν♏ ρ Oph. , 1.2μd ♐ , β♑ , ,
21 ζ♓, , ιζ ♉ , ν ♊ , 1.2α♋οπ, υ ♌, αι♍, ακλ♎, βν♏ ρ Oph. , 1,2μd ♐ , β ♑ , ,
18 ζ♓ , , ειζ ♉ , ν ♊ , 1.2α♋οπ, υ ♌, αι♍, ακλ♎, βν♏ ρ Oph. , 1,2μd ♐ , β ♑
15 ζ♓ , , ειζ ♉ , ν ♊ , 1.2α♋οπ, υ ♌, α♍, ακλ♎, βν♏ ρ Oph. , 1.2μd ♐ , β♑ , ,
12 ιζ♓ , , εζ ♉ , ν ♊ , 1.2α♋οπ, υ ♌, α, λ♍ακλ♎, βν♏ ρ Oph. , 2μd ♐ , β♑ , ,
9 ιζ♓, , εζ ♉ , ν ♊ , 1.2α♋οπ, υ ♌ , α, λ♍α4ζ♎, βν♏ ρ Oph. , 2μd ♐ , β♑ , ,
6 ιζ♓, , ι ♉ , , ν ♊ , 1.2α♋οπ, υ ♌, α, λ♍α4ζ♎. βν♏, , β ♑ , ,

3 εζ♓, , ε ♉, , 1.2α♋οπ, υ♌, α, λ♍α4ζ♎, βν♏, , β♑, ,
0 εζ♓, , 1δε ♉, , 1.2α♋οπ, υ♌, α, λ♍α4ζϑ♎, ν♏, , β♑, ,

☊ — ☊ in ♎, VI^s.

27° ε♓, , 1.2δε ♉, , 1.2α♋οπ, υ♌, , λ♍α4ζϑ♎, ν♏, , β♑, ,
24 ε♓, , 1.2δε ♉, , 1.2α♋οπ, υ♌, , λ♍α4ζϑ♎, , , β♑, ,
21 ε♓, , 1.2δε ♉, , 1.2α♋οπ, υ♌, λ♍4ζϑ♎, , , β♑, ϑ♒
18 εο♓, , 1.2δε ♉, , 1.2α♋ξοπ, υ♌, , λ♍4ζϑ♎, , , β♑, ϑ♒
15 εο♓, , 1.2δε ♉, , 1.2α♋ξοπ, υ♌, , λ♍4ζϑ♎, , , β♑, ϑ♒
12 ο♓, , 1.2δ♉, , 1.2α♋ξοπ♌, , , κ♍, 4ζγϑ♎, φ Oph., , β♑, ϑ♒
9 ο♓, , 1.2δα ♉, , 1.2α♋ξοπ, τ♌, ϑ, κ♍, 4ζγϑ♎. φ Oph., β♑, ϑ♒
6 ο♓, , 1.2δα ♉, 1.2α♋ξοπ, τ♌, ϑ, κ♍, γϑ♎, φ Oph., , β♑, ϑ♒
3 ο♓, , 1.2δα ♉, , 1.2α♋ξοπ, τ♌, ϑ, κ♍, γϑ♎, φ Oph., β♑ ϑ♒
0 ο♓, , 1.2δα ♉, , ξοπ, τ♌β, ηϑ, κ♍, γϑ♎, φ Oph., , β♑, ϑ♒

☊ — ☊ in ♍, V^s.

27° ο♓, , γ1.2δα ♉, , ξο, τ♌β, ηϑ, κ♍γϑ♎, φ Oph., , β♑, ϑ♒
24 ο♓, , γ1.2δα ♉, , ξο, τ♌β, ηϑ, κ♍γϑ♎, φ Oph., , β♑, ϑ♒
21 ο♓, , γ1.2δα ♉, , ξο, τ♌β, ηϑ, κ♍γϑ♎, φ Oph., d ♐, β♑, ϑ♒
18 , , γ1.2δα ♉, , ξ, , τ♌β, ηϑ, κ♍γϑ♎, φ Oph., d ♐, β♑, ϑλ♒
15 , , γ1.2δα ♉, , ξ, ϱτ♌β, ηγϑ, κ♍γϑ♎, φ Oph., d ♐, β♑, λφ♒
12 , , γ1.2δα ♉, , ξ, ϱτ♌β, ηγϑ, κ♍γ♎, φ Oph., d ♐, , λφ♒
9 ν♓, μ Cet., γ1.2δα ♉, , , ϱσ♌β, ηγ, κ♍γψ♎, φ Oph., d ♐, , λφ♒
6 ν♓, μ Cet., γ1.2δα ♉, , α, ϱσ♌β, ηγ, κ♍ γψ♎, φ Oph. d ♐, , λφ♒
3 ν♓, μ Cet., γ1.2δα ♉, ν♊, α, ϱσ♌β, ηγ♍, γψ♎, φ Oph., d ♐, , λφ♒
0 ν♓, μ Cet., 1.2δα ♉, ν♊, α, ϱσ♌, ηγ♍, γψ♎, φ Oph., d ♐, , λφ♒

☊ — ☊ in ♌, IV^s.

27° νη♓, 2ξμ Cet., γ1.2δα ♉, ν♊, α, ϱσ♌, γ♍, γψ♎, φ Oph., d ♐, , λφ♒
24 νη♓, 2ξμ Cet., γ1.2δα ♉, νζ♊, α, ϱσ♌, γ♍, γψ♎, φ Oph., d ♐, , λφ♒

21 νη♓, 2ξμ Cet., γ1.2δα ♉, νζ ♊, α, ϱσ ♌, γ♍, γψ♎, φ Oph., 2μd ♐, λφ ♒

18 νη♓, 2ξμ Cet., γ1.2δα ♉, νζ ♊, δ♋α, ϱσ ♌, γ♍, γψ♎, φ Oph. 2μd ♐, ϑ♑, φ ♒

15 νη♓, 2ξμ Cet., γ1.2δα ♉, νζ ♊, δ♋α, σ ♌, γ♍, γ♎, φ Oph., 2μπd ♐, ϑ♑, ,

12 νη♓, 2ξμ Cet., γ1.2δα ♉, νζκ ♊, δ♋α, σ ♌, γ♍, γ♎, φ Oph., 2μπd ♐, ϑ♑, ,

9 νη♓, 2ξμ Cet. γ1.2δα ♉, νζκ ♊, δ♋α ♌, , , γϑ♎, φ Oph., 2μϑ ♐, ϑ♑, ,

6 νη♓, 2ξμ Cet., 1.2δαζ ♉, νζκ ♊, δ♋α ♌, , , γϑ♎, φ Oph., 2μοπ ♐, ϑ♑, ,

3 νη♓, 2ξμ Cet., 1.2δζ ♉, νζδκ ♊, δ♋, , , γϑ♎, φ Oph., 2μοπ ♐, ϑγ♑, ,

0 νη♓, 2ξμ Cet., 1.2δζ ♉, νζδκ ♊, δ♋, , , γϑ♎, φ Oph., 2μοπ ♐, ϑγ♑, ,

☊ ☊ in ♋, III^{s}.

27° νη♓, 2ξμ Cet., 1.2δζ ♉, ηνζδκ ♊, δ♋, , , γϑ♎, ϱ Oph., 2μοπ ♐, ϑγ♑, ,

24 νη♓, 2ξμ Cet., 1.2διζ ♉, ηζδκ ♊, δ♋, , , γϑ♎, ϱ Oph., 2μ1νοπ ♐, ϑγ♑, ,

21 νη♓, 2ξμ Cet., 1.2διζ ♉, ηζδκ ♊, δ♋, , , γϑ♎, ϱ Oph., 2μ1νοπ ♐, ϑγ♑, ,

18 νη♓, 2ξμ Cet., 1.2διζ ♉, ηδκ ♊, , , , γϑ♎, ϱ Oph., 2μ1νοπ ♐, γ♑, ,

15 νη♓, 2ξμ Cet., 1.2διζ ♉, ηδκ ♊, , , , γϑ♎, ϱ Oph., 1νο ♐, γ♑, ,

12 νη♓, μ Cet., 1.2δειζ ♉, ηδκ ♊, , , , γϑ♎, ϱ Oph., 1νο ♐, γ♑, ,

9 νη♓, μ Cet., 1δειζ ♉, ηδκ ♊, η♌, , , γϑ♎, ϱ Oph., 1νο ♐, γ♑, ,

6 νη♓, , ειζ ♉, ηδκ ♊, η♌, , , 4ζγϑ♎, ν♏ ϱ Oph., 1ν ♐, γ♑

3 νη♓, ει ♉, ηκ ♊, η♌, , , 4ζγϑ♎, ν♏ ϱ Oph., 1ν ♐, γ♑, 2τ♒

0 νη♓, , ει ♉, ηκ ♊, η♌, , , 4ζϑ♎, ν♏ ϱ Oph., 1ν ♐, ε♑, 2τ♒

☊ ☊ in ♊, II^{s}.

27° νη♓, , ει132 ♉, ηεκ ♊, γ♋η, ι♌, , κ♍4ζϑ♎, βν♏, 1ν ♐, ε♑, 2τ♒

24 νη♓, , ει132 ♉, εκ ♊, γ♋η, ι♌, , κ♍4ζϑ♎, βν♏, λψ ♐, ε♑, 2τ♒

21 νη♓, , ι132 ♉, εκ ♊, γ♋η, ι♌, , κ♍4ζ♎, βν♏ B Oph., λψ ♐, ε♑, 2τ♒

18 ν♓, , ι132 ♉, εκ ♊, γ♋η, ι♌, , κ♍4ζ♎, βν♏ B Oph., λψ ♐, ε♑, 2τ♒

15 ν♓, , ι132 ♉, εκ ♊, γ♋η, ι♌, , κ♍4ζ♎, βν♏ B Oph., λσψ ♐, ε♑, 2τ♒

12 ν♓, , 2κ132 ♉, εκ ♊, γ♋η, ι♌, , κ♍4ζλ♎, βν♏ ϑ B Oph., λσψ ♐, ε♑, 2τ♒

9 ν♓, , 2κ132 ♉, εκ ♊, γ♋η, ι♌, , κ♍4ζκλ♎, βν♏ ϑB Oph., λσψ ♐, ε♑, 2τ♒

6 , , 2κ132 ♉, εκ ♊, γ♋η, ι♌, , κ♍4ζκλ♎, β♏ ϑB Oph., λσψ ♐, ε♑, 2τ♒

3 , , 2κ132 ♉, εκ ♊, γ♋η, ι♌, , κ♍ κλ♎, β♏ ϑ B Oph., λσψ ♐, ε♑, 2τ♒

0 ο♓, , A2κ132 ♉, εκ ♊, η♌, , γ, κ♍ ακλ♎, β♏ ϑ B Oph., λσψ ♐, ε♑, 2τ♒

☊ ☊ in ♉, I^s.

27° ο♓, , A2κ132 ♉, ευκ ♊, η♌, γ, κ♍ακλ♎, ϑ B Oph., λσψ ♐, ε♑, 2τ♒

24, ο♓, , A2κ ♉, υκ ♊, η♌, γ, κλ♍ακλ♎, ϑ B Oph., λσψ ♐, ε♑, 2τ♒

21 ο♓, , A2κ ♉, υ ♊, η♌, γϑ, λ♍ακλ♎ δ♏, Aϑ Oph., στψ ♐, ε♑, 2τ♒

18 ο♓, , A2κ ♉, υ ♊, η♌, γϑ, λ♍ακλ♎ δ♏, Aϑ Oph., στ ♐, ε♑, 2τ♒

15 ο♓, , A2κ ♉, υ ♊, η♌, γϑ, λ♍ακλ♎ δ♏, Aϑ Oph., στ ♐, ε♑, 2τ♒

12 ο♓, , A ♉, ιυ ♊, η♌, γϑ, λ♍ακ♎ δ♏, Aϑ Oph. στ ♐, ε♑, 2τ♒

9 ο♓, δ♈, A ♉, ιυ ♊, η♌, γϑ, λ♍α♎ δ♏, A Oph. στ ♐, ε♑, 2τ♒

6 ο♓, δ♈, A ♉, ιυ ♊, η♌, γϑ, λ♍α♎ δ♏, A Oph. στ ♐, ε♑, ,

3 ζο♓, δ♈, A ♉, ιυ ♊, η♌, ηγϑ, λ♍α♎ δ♏, A Oph., στ ♐, ε♑, ,

0 ζο♓, δ♈, . ιυ ♊, η♌, ηγϑ, λ♍δ, σ♏ A Oph. τ ♐, ε♑, ,

☊ ☊ in ♈. 0^s.

27° ζ♓, δ♈, , ιυ ♊, η♌, , ηγϑ, λ♍δ, σ♏ A Oph., τb ♐, ε♑, ,

24 ζ♓, δ♈, β ♉, ιυ ♊, η♌, , ηϑ♍, δ, σα♏ A Oph., τb ♐, ε♑, ,

21 ζ♓, δ♈, ηβ ♉, ιυ ♊, η, σ♌, ηϑ♍, δ, σα♏ A Oph. τb ♐, ε♑, ,

18 ζ♓, δ♈, ηβ ♉, ιυ ♊, η, σ♌, η♍, , σα♏ A Oph., τb ♐, ε♑, ,

15 εζ♓, δ♈, ηβ ♉, ιυ ♊, η, σ♌β, η♍, , σα♏ A Oph., τ ♐, ε♑, ,

12 εζ♓, δ♈, ηβ ♉, κAur. ιυ ♊, η, σ♌β, ηα♍, , σα♏ A Oph., τ ♐, ε♑, ,

9 $\iota\zeta$♓, , $\eta\beta$♉, κAur. $\iota\upsilon$♊, , σ♌β, $\eta\alpha$♍, , $\sigma\alpha$♏ A Oph. τ♐, $\iota\delta$♑, ,

6 $\iota\zeta$♓, , $\eta\beta$♉, κAur. $\iota\upsilon$♊, γ♋, σ♌β, $\eta\alpha$♍, , $\sigma\alpha$♏, $\delta\tau$♐, $\iota\gamma\delta$♑, ,

3 ι♓, , $\eta\beta$♉, κAur. $\iota\upsilon$♊, γ♋, σ♌β, α♍, π♏, $\sigma\alpha$♏, $\delta\tau$♐, $\gamma\delta$♑, ,

0 ι♓, , $\eta\beta$♉, κAur. $\iota\upsilon$♊, γ♋, σ♌β, α♍, π♏, $\sigma\alpha$♏, $\delta\tau$♐, $\gamma\delta$♑, ,

4. Explanation *of the Second* Table.

The method of computing the solar nutation, to which these tables are adapted, is neither altogether new, nor always the most convenient: but for the places of single stars, in cases like the present, it possesses some advantages, and may therefore deserve to be further explained and demonstrated, notwithstanding the trifling magnitude of the correction.

The formula employed by Bessel for the right ascension is— ta δ ($''$,3982 sin α sin 2 ☉ + $''$,434 cos α cos 2 ☉) — $''$.9173 sin 2 ☉; and for the declination, — $''$,3982 cos α sin 2 ☉ + $''$,434 sin α cos 2 ☉. But it is known, that in general a sin x + b cos $x = \surd\,(a^2+b^2)$ sin $\left(x + \text{ang ta}\dfrac{b}{a}\right)$. (M. E. §.216): and for the right ascension we have $a = -$ ta δ .3982 sin α — .9173, and $b = -$ ta δ .434 cos α, and ang ta $\dfrac{b}{a}$ = ang cot $\dfrac{a}{b}$ = ang cot $\left(\dfrac{.3982}{.434}\text{ ta }\alpha + \dfrac{.9173}{.434}\text{ cot }\delta\text{ sec. }\alpha\right)$ = ang cot .9175 (ta α + 2.3036 cot δ sec α) : and this angle being found, the factor $\surd\,(a^2+b^2)$ will be obviously equal to its cosecant multiplied by b, or by — .434 ta δ cos α. In the same manner we obtain for the declination $a = -$ .3982 cos α, and $b = +$ .434 sin α, and ang ta $\dfrac{b}{a}$ = ang ta $-\dfrac{.434}{.3982}$ ta α = ang ta ($-$1.0900 ta α): the cosecant being again multiplied by b = .434 sin α, for the factor.

Taking, for an example, the first star of the table, ι ♓, we have $\alpha = 0^{\text{H}}\ 53^{\text{M}}\ 36^{\text{S}},5 = 13^\circ\ 24'$ and $\delta = +\,6^\circ\ 55'$ (11$'$) whence nat. ta α = .2382, log ta α = 9.3770 log sec α = .0120 log cos α = 9.9880, log sin α 9.3650, log cot δ .9161, and log ta 9.0839 : then

AR.

Log	2.3036	.3624
	Cot δ	.9161
	Sec α	.0120
	N 19.522	1.2905
	.2382	
N. ta α	19.7602	1.2958
Log	.9175	9.9626
L cot 3° 9′		1.2584
cosec		1.2590
.434		9.6375
ta δ		9.0839
cos α		9.9880
L. factor (—)		9.9684

Declination.

Log 1.0900	.0374
ta α	9.3770
L. ta 14° 33′	9.4144
cosec	.6000
.434	9.6375
sin α	9.3650
factor +	9.6025

The factor for the AR. being negative, we add 6 signs to the angle, in order that the signs of the sines may be inverted, and we have 6ˢ 3° 9′ for the angle. The angles never varying much from those which belong to the lunar nutation, the two series serve as checks on each other in the computation. For reducing the factors into time we subtract log 15 = 1.1761, or add 8.4614, instead of 9.6375. The angle for correcting the declination is 14° 33′, but the tangent being negative, the true angle to be employed is 12ˢ—(14° 33′) or 6ˢ—(14° 33′), since the tangents are the same for opposite points of the circle: and by reverting to the original expression, we shall find that the latter must be taken in this case: but for the correction of the N. P. D. we take the former, when the declination is north, in order to change the sign of the sine. The factor for the declination is nearly constant for all the stars, and may safely be assumed 9.62, which is equivalent to ″,4 or ″,41 for the whole table, the error seldom amounting to so much as ″,01.

5. *Computation of the Elements for an Almanac.*

i. With the longitude of the moon's node for the beginning and end of each lunation, enter the first table, and put down all the stars liable to occultation at either place of the node, for further examination.

Example. In February, 1822, the node recedes from 10^s $26°$ $5'$ to 10^s $24°$ $49'$, and the stars liable to occultation are, η f β ♉, κ ♊, α ϱ υ ♌, i ♍, π σ α τ ♏, A Ophiuchi, σ ψ ♐, ϑ ♑.

ii. Find, by inspection of the moon's right ascension, the day on which the conjunction with each of these stars will take place, and compute the corrections from the sun's longitude and the place of the node on that day.

Example. Taking υ ♌, of which the AR. in space is $171°$ $56'$, we find that the occultation will happen on the 8th, a little before midnight, when the sun's longitude is 10^s $19°$ $44'$, and that of the ☊ 10^s $25°$ $41'$. The time from 1st Jan. 1820 is 2 $\frac{1}{9.1}$ years, whence the precession is $+ 6^s,47$ and $+ 41'',9$. For the aberration, we add 3^s $9°$ $0'$ and 3^s $0°$ $44'$ to the ☉'s longitude, making 13^s $28°$ $44'$ and 13^s $20°$ $28'$, and the logarithmic sine of $+ 58°$ $44'$ being 9.9318, this being added to .0935 gives .0253, or $+ 1^s,06$, for the correction in AR.; and in the same manner L. sin $50°$ $28'$, or 9.8872, added to .9058, gives .7930 or $+ 6'',2$ for the correction of the N. P. D. The arguments for the solar nutation are 5^s $29°$ $56'$, and 6^s $8°$ $47'$ to which we add 2^s, $9°$ $28'$, making 27^s $9°$ $24'$, or 3^s $9°$ $24'$, the sine of which is the cosine of $9°$ $24'$, and $9.9941 + 8.785 = 8.779$, giving $+ {}^s,06$ for the AR.; and 27^s $18°$ $15'$ gives us $9.978 + 9.62 = 9.60$, and the correction for the N. P. D. is $'',4$. The lunar nutation is found from the arguments 5^s $29°$ $50'$ and 6^s $11°$ $13'$, making with 10^s $25°$ $41'$, 16^s $25°$ $31'$ and 17^s $6°$ $54'$, of which the logarithmic sines are 9.753 and 9,594, to be added to .0426 and .8602, making 9.797 and .454, and giving $+ {}^s,62$ and $+ 2'',8$ for the lunar nutations. The result of the whole is,

υ ♌	$11^H\ 27^M\ 43^S,87$ AR.	N. P. D.	89° 49′ 48″1
+	6,47	+	41,9
+	1,06	+	6,2
+	,06	+	,4
+	,62	+	2,8
	11 27 52,08		89 50 39,4
	Or, 171° 58′ 1″,2	Decl.	0 9 20,6 N

iii. Find by inspection of the moon's right ascension, the hour in which the occultation is likely to happen, and compute, by means of first and second differences, the moon's change of right ascension and of declination for that hour.

Example. In the *Connaissance des Tems* for 1822, the right ascensions stand thus:

		1 Diff.	2 Diff.	Mean.
7 M	161° 10′ 12″			
8 N	166 52 41	+ 5° 42′ 29″	— 6′ 13″	
M	172 28 57	5 36 16	4 14	— 5′ 13″
9 N	178 0 59	5 32 2		

Hence it appears, that the occultation will happen about 11 o'clock on the 8th: the change in an hour is 28′ 1″,3 besides the second difference, which at 11^H is 12″, leaving 27′ 49″,3 for the change from 11 to 12. The declinations are,

		1 Diff.	2 Diff.	Mean
7 M	6° 30′ 22″			
8 N	3 32 35	N 2° 57′ 47″	— 0′ 5″	
M	0 34 53	2 57 42	1 43	— 54″
9 N	—2 21 6	2 55 59		

Consequently we have 14′ 48″,5 for each hour, and 2″,1 for the second difference at 11^H, leaving 14′ 46″,4 for the change in the last hour.

iv. To the proportional logarithm of the hourly change of right ascension, add the logarithmic secant of the declination; subtract the sum from the proportional logarithm of the change of declination, and find the angle of which the difference is the tangent, for the polar orbital angle, agreeing nearly with the angle appropriate to the star in the second table.

Example.	Prop. Log. 27′ 49″,3	.8109
	Log. sec. 0° 35′	.0000
		.8109
	P. L. 14′ 46″,4	1.0858
	Log. ta 62° 2′	0.2749

v. The logarithmic cosine of this orbital angle added to the proportional logarithm of the hourly motion in declination will give the hourly motion in the orbit, differing but little from the hourly change of longitude.

Example.	Log. cos. 62° 2′	9.6711
	P. L.	1.0858
	P. L. 31′ 30″,3	0.7569

We therefore write ☽'s H. M. 31′ 30″, S. 62° 2′ E.

vi. It will also be necessary to find the difference of declination at the time of the conjunction in right ascension, since this element will be required for the subsequent computation with regard to any particular observation.

Example. The moon's AR. at 11ᴴ is 172° 1′ 7″,7, that is, 3′ 6″,5 beyond the star; then

As 27′ 49″,3	P. L. A. C.	1892	Again, as 1ᴴ	P.L. A.C.	5229
To 1ᴴ	P. L.	4771	To 6ᴹ 42ˢ	P. L.	1.4290
So is 3′ 6″,5	P. L.	1.7627	So is 14′ 46″,4		1.0858
To 6ᴹ 42ˢ		1.4290	To 1′ 39″		2.0377

Now the declination at 11ᴴ is 49′ 39″,4, and consequently at 10ᴴ 53ᴹ 18ˢ, 51′ 18″,4, which exceeds that of the star, 9′ 20″,6 by 41′ 57″,8, the difference of declination. We therefore write 1822, Feb. 8, ☽ υ ♌, ☌ in AR. 10ᴴ 53ᴹ 18ˢ, ☽ 41′ 57″,8 N.

6. *Computation of a visible Occultation.*

i. Find the moon's altitude for the instant of conjunction in right ascension, and for an hour earlier or later, and compute the parallactic angle, formed by the circle of declination with the vertical circle: this angle subtracted from the *polar orbital* *angle,* given by the previous computation, or added to it, according to the relative situation of the angles, will give the *parallactic orbital angle,* formed by the orbit with the vertical circle.

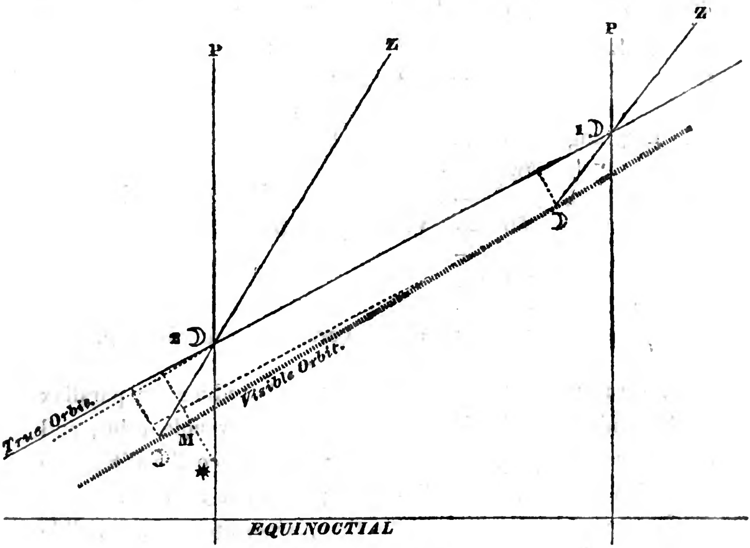
Example. In the case of the occultation of *υ* ♌, since the moon passes the meridian of Paris on the night of the 8th of February, at $14^H\ 5^M$, it is obvious that she will be in the east at 11^H, and that the depression by parallax will accelerate the occultation. We therefore calculate the altitude for $10^H\ 53^M\ 18^S$ the time of conjunction, and for $9^H\ 53^M\ 18^S$. Now, the moon's right ascension, in the former case, is 171° 58′ 1″, or $11^H\ 27^M\ 52^S$, in the latter 171° 30′ 9″, or $11^H\ 26^M\ 1^S$: the sun's $21^H\ 28^M\ 26^S$ and $21^H\ 28^M\ 16^S$ respectively: the differences $10^H\ 0^M\ 34^S$, and $10^H\ 2^M\ 15^S$, added to the sun's horary angles, make $20^H\ 53^M\ 52^S$, and $19^H\ 55^M\ 33^S$ W., or $3^H\ 6^M\ 8^S$ and $4^H\ 4^M\ 27^S$ E., for the moon's horary angles: and her declinations are 51′ 19″ and 1° 6′ 6″ N. Then from the Requisite Tables, XVI., we have

1. Log rising	4.49425	2. Log rising	4.71336
L. cos 48° 50′ 14″	9.81835		9.81835
51′ 19″	9.99995	cos 1° 6′ 6″	9.99992
n 20538	4.31255	n 34012	4.53163
N.S. 66937 Mer A.	42° 1′ 5″	N.S. 67255 M. A.	42° 15′ 52″
N.S. 46399 Alt	27 38 42	N.S. 33243 Alt.	19 25 0
L. sec. Alt	.05264		.02543
sin H. A. 46° 32′	9.86080	L. sin 61° 7′	9.94229
cos lat	9.81835		9.81835
sin P. A. 32° 38′	9.73179	37° 40′	9.78607

Now the moon being east of the meridian, the vertical circle passes upwards to the west of the circle of declination, and downwards to the S. E., so that the parallactic angles must be subtracted from 62° 2′ the angle S.E. of the moon's orbit, thus,

	62° 2′	62° 2′
	− 32 38	37 40
Par. orb. angle	29 24	24 22

ii. To the proportional logarithm of the horizontal parallax, properly reduced, add the logarithmic secant of the apparent altitude, obtained from the approximate parallax in altitude; and first the secant and then the cosecant of the parallactic orbital angle in each position. The first results will give the parallax reduced to the true orbit, and their difference will give the correction of the hourly motion in the orbit. The difference of the second results will be the tangent of the angular correction, if the corrected hourly motion be made radius.



Example. The proportional logarithm of the equatorial parallax at 12^H is 5092, increasing to 5115 in 12 hours; consequently at 10^H 53^M it is 5090, at 9^H 53^M, 5088, and adding 8 for the ellipticity, since the latitude is between 47° and 51° (Astr. Coll. II. ii. XIII.), we have 5098 and 5096. Then

P. L.		5098		5096
Log sec	27° 39′	.0527	19° 25′	.0254
P. L.	49′ 18″	5625	52′ 31″	5350
Log sec	26° 49′ 42″	.0495	18° 32′ 29″	.0231
P. L.		5593		5327
Log sec	29° 24′	.0599	24° 22′	.0405
P. L.	43′ 15″	6192	48′ 6″	.5732
Log cosec	29° 24′	3090	24° 22′	.3845
P. L.	24′ 23″	8683	21′ 47″	9172

The differences are 4′ 51″ and 2′ 36″, and the hourly motion becomes 26′ 39″, instead of 31′ 30″; hence

P. L.	26′ 39″	8296	
	2′ 36″	1.8403	
Log tang	5° 34′	8.9893	
	62 2		
	56 28		Visible polar orbital angle.
	29 24		
	23 50		Visible parallactic orbital angle.

iii. By means of the angles thus found, reduce the parallax at the conjunction in right ascension to the visible orbit, and to the direction perpendicular to it; and reduce the difference in declination to the same directions; the sum or difference of the former results, reduced into time, according to the visible hourly motion, will give the time of the nearest approach and the sum or difference of the latter will give the nearest distance. The hourly motion may be reduced to the visible orbit by increasing it in the ratio of the radius to the secant of deviation.

Example.

P. L. par in alt.	5593	P. L. H. M.	8296
Log sec 23° 49′	.0387	Log sec 5° 36′	.0021
P. L. 45′ 26″	5980	P. L. corr. H. M.	8275
		P. L. 1^{H}	4771
Log cosec 23° 50′	3935		
			3504
P. L. 20′ 4″	9528	P. L. 22′ 14″	9083
P. L. diff. decl. 41′ 58″	6324	P. L. 49^{M} 49^{S}	5579
Log sec 56° 28′	2577	10 53 18	
P. L. 23′ 11″	8901	10 3 29 Middle.	
Log cosec 56° 28′	.0791		
P. L. 34′ 59″	7115		

45′ 25″	34′ 59″	
23 11	20 4	
22 14	14 55	Nearest distance.

iv. It may be necessary, in some cases, to repeat the calculation of the effect of parallax at the time thus found, in order that the true visible motion during the interval may be employed, instead of the hourly motion, and the calculations of the two last precepts may be repeated with the correction of the visible orbit thus obtained: or the whole computation may be verified by any of the common methods. The nearest distance, compared with the moon's apparent diameter corrected for the altitude, readily gives the length of the star's path behind the moon, and the visible hourly motion the corresponding time.

Example. Supposing 14′ 55″ the correct distance, the moon's semidiameter being 15′ 16″ at noon, and diminishing 9″ in 24 hours, it will be 15′ 12″ at 10^{H}, and the augmentation for altitude 21° being 6″, we have 15′ 18″ for the corrected semidiameter.

Dist.	14′ 55″		
Semid.	15 18		
Sum	30 13	P. L.	.7750
Diff.	23	P. L.	2.6717
			3.4467
		Half sum	1.7233
P. L. for		H. M.	3504
P. L. semiduration		7^M 38^S	1.3729
	10 3 29		
Immersion	9 55 51 ;	10^H 11^M 7^S Emersion.	

The *Connaissance des Tems* makes the distance 14′½, and the whole duration 10^M only.

ii. Errors *of the* Lunar Tables; *deduced from* 4068 *Observations, computed by order of the* Board of Longitude.

MAYER and MASON.	Mean Error. Long.	Mean Error. Lat.	Greatest Error. Long.	Greatest Error. Lat.
1783	± 23″,7	18″,8	67″,2	74″,0
1784	25,5	14,0	61,9	49,8
1785	35,6	13,0	72,6	44,4
1786	28,2	14,7	69,3	65,4
1787	30,5	13,0	86,5	111,5
1788	38,8	13,1	89,7	114,5
MASON.				
1789	28,9	7,4	61,2	57,1
1790	28,4	6,7	52,2	28,0
1791	28,4	7,0	70,7	53,8
1792	24,8	6,8	50,7	21,8
1793	24,7	10,2	51,3	41,1
1794	21,0	11,4	56,9	39,2
1795	24,9	13,4	60,0	56,6
1796	24,5	15,6	54,5	37,5
1797	25,0	16,6	53,8	80,4
1798	23,0	16,5	56,9	57,6
1799	25,8	19,4	58,3	58,3
1800	27,0	17,7	55,3	56,0
1801	30,9	17,4	65,4	54,6
1802	32,1	17,5	64,1	79,6
1803	34,6	16,8	86,7	76,5
1804	36,0	10,1	67,0	43,1
LAPLACE and MASON.				
1805	+ 34,1 − 0,0	8,3	61,1	30,4
1806	+ 36,7 − 0,0	11,1	67,6	41,0

	Mean Error. Long.	Mean Error. Lat.	Greatest Error. Long.	Greatest Error. Lat.
1807	± 27″,3 + 25,5	7″,8	55″,5	36″,4
1808	± 26,7 + 25,7	6,0	59,9	32,7
1809	± 8,1	5,3	36,6	30,9
1810	± 7,4	7,2	25,5	35,2
1811	± 7,0	9,6	19,5	43,4
1812	± 8,8	10,2	28,7	34,8
LAPL. and BURG.				
1813	± 6,5	8,3	17,2 3 ; 15,2*	31,7
1814	± 5,1	7,7	20,8 3 ; 12,3*	30,9
1815	± 4,8	6,3	34,3 3 ; 13,4*	23,7
1816	± 5,6	6,5	24,5 3 ; 17,0*	18,0
1817	± 5,3 − 0,5	6,7	15,1 3 ; 10,4*	20,1
1818	± 6,4 − 2,3	6,7	16,3 3 ; 13,6*	29,5
1819	± 6,1 − ,2	7,0	27,6 3 ; 13,8*	24,7
LAPL. and BURCKHARDT.				
1817	± 4,7 + 3,1	4,7	13,7 3 ; 11,2*	13,5
1818	± 5,0 + 2,3	5,2	21,4 3 ; 14,5*	25,5
1819	± 4,5 + 3,0	4,6	22,3 3 ; 12,5*	20,8

* Greatest mean of three consecutive errors.

Art. XIV. *Miscellaneous Intelligence.*

I. Mechanical Science.

§ Optics, the Arts, &c.

1. *On the Structure of the Diamond.*—Whilst making experiments on the optical structure of amber, Dr. Brewster was led to compare it with the diamond. He found some singular analogies in the two substances, but one diamond which he examined presented a new phænomenon of a most unexpected kind, which Dr. Brewster observes, is the only fact in the natural history of this body that promises to throw light upon its origin and mode of formation. The same phænomenon occurs also with amber. It is "the existence of small portions of air within both substances, the expansive force of which has communicated a polarizing structure to the parts in immediate contact with the air. This structure is displayed by four sectors of polarized light encircling the globule of air, and can be produced artificially either in glass or in gelatinous masses, by a compressing force, propagated circularly from a point. It is obvious that such an effect cannot arise from any mode of crystallization; and if any proof of this were necessary, it might be sufficient to state, that I have never observed the slightest trace of it in more than 200 mineral substances which I have examined, nor in any of the artificial salts formed from aqueous solutions. It can, therefore, arise only from the expansive force exerted by the included air on the *diamond* and the *amber*, when *they were in such a soft state as to be susceptible of compression from so small a force.* That this compressible state of the diamond could not arise from the action of heat is manifest from the nature and the recent formation of the soil in which it is found; that it could not exist in a mass formed by aqueous deposition is still more obvious; and hence we are led to the conclusion, rendered probable by other analogies, that the *diamond* originates like amber from the consolidation of, perhaps, vegetable matter, which gradually acquires a crystalline form by the influence of time, and the slow action of corpuscular forces."

These results were obtained with flat diamonds regularly crystallized, but on examining Mr. Allan's collection, Dr. Brewster found one of a perfectly octoëdral form, having the same structure, and containing also an air-bubble of considerable size, which had produced, by its expansion, the polarizing structure already described. *Edin. Phil. Journal,* iii. p. 98.

2. *Optico-Meteorological Question.*—The following subject has been given by the class of mathematics in the Royal Academy of Sciences of Prussia:

"To give a mathematical explanation of the luminous and coloured crowns, which are observed at times round the sun and moon, conformable to experiments on light and the constitution of the atmosphere, and in accordance with observations of the phænomena made with as much precision as possible."

Mémoires cannot be received after March, 1822. The prize is fifty ducats, and will be adjudged at the public sitting, on the anniversary of Leibnitz, on the 3d of July following.

3. *Double Refraction of Minerals.*—The means of ascertaining the double refraction of substances are very simple; the two following are quoted by M. Soret:—"Two plates of tourmaline, cut parallel to the axis of the crystal, and placed crossways, so as to absorb all the light, form the apparatus. The substance to be examined is to be placed between the plates; if it is doubly refractive, the light re-appears through the tourmalines; if not, all remains dark."

The other method is to place the crystal to be examined over a hole in a card, and examine the light transmitted through it by an achromatic prism of Iceland spar. If the two images produced are differently coloured it indicates double refraction. *Journal de Physique,* xc. p. 353.

4. *Restoration of the White in Painting.*—M. Thenard has lately applied his oxygenated water with great success to the arts. It is constantly happening in paintings where the white used is prepared from lead, that sulphurous vapours change the

tint, and render it brown or black. Paintings of the first masters are frequently injured in consequence of this effect, being blotted, as it were, with spots of colour, which spoil the effect. An artist at Paris, who possessed a design of Raphael's thus disfigured, and who was too careful of the original work to re-paint the places, applied to M. Thenard for chemical assistance. That chemist during his attempts, remembering the effect of oxygenated water in converting the black sulphuret of lead into a white sulphate, sent some water, very slightly oxygenated, to the artist, who, by applying it with a pencil, instantly removed the spots, and restored the white. The fluid was so weak as to contain not more than five or six times its volume of oxygen, and had no taste. Paper coloured by a slight tint of bistre was not at all altered by it.—*Annales de Chimie*, xiv. p. 221.

5. *Distant Visibility of Mountains.*

	Authorities.	Distance in Miles.
Himáláya mountains..............	Sir W. Jones	244
Mount Ararat, from Derbhend......	Bruce	240
Mowna Roa, Sandwich Isles (53 leagues)		180
Chimborazo (47 leagues)		160
Peak of Teneriffe from South Cape of Lanzerota		135
Peak of Teneriffe from ship's deck....		115
Peak of the Azores	Don M. Cagigal	126
Temaheud	Morier	100
Mount Athos	Dr. Clarke	100
Adam's Peak		95
Ghaut at the back of Tellichery		94
Golden Mount from ship's deck		93
Pulo Pera from the top of Penang ..		75
The Ghaut at Cape Comorin		73
Pulo Penang from ship's deck		53

The last six observations, and that of the Peak of Teneriffe, were made by a writer in the *Calcutta Monthly Journal.* The first of the Peak of Teneriffe is from *Humboldt's Personal Nar-*

rative, i. p. 98. The Peak of the Azores from the same volume, p. 99, note. *Edin. Phil. Journal,* iii. p. 192.

6. *Earthen-ware Reflectors.*

To the EDITOR *of the* QUARTERLY JOURNAL *of* SCIENCE, *&c.*

SIR—I have great pleasure in informing you, that we have adopted some of the reflectors proposed by Mr. Millington, and described in your fifth volume, to the gas lamps in our city, and find them attended with such advantages, as to induce me to write to you on the present occasion. They are made of earthen-ware, with the common white glaze, are about eleven inches diameter, and cost about seven shillings a dozen. They not only considerably increase the light, but materially contribute to the protection of the head of the lamp, by preventing its being unsoldered, or injured, by the flame; and I am persuaded, that if known, they would be duly appreciated, and generally adopted. I am, Sir, yours sincerely,

CHARLES WILKINSON.

Bath, April 20, 1820.

7. *Gas Tube.*—Mr. Phipson, of Birmingham, in order to obviate the effects produced by the action of gases on copper or brass pipes, through which they pass, has adopted the plan of lining them with lead. A tube is formed of rolled copper, by drawing it through a plate, and the edges are soldered together, so as to form a safe joint, the superfluous solder is dressed off, and the tube again drawn; a piece of lead pipe is then drawn through a plate on a mandrill, of the diameter of the tube required, and placed within the copper pipe; then by passing through it a conical mandrill attached to a rod, the lead pipe is forced against the inner surface of the copper tube, so as to leave them in perfect contact; or sometimes a lead pipe is prepared on a mandrill, of the diameter of the tube required, and a copper pipe, already soldered, drawn over it; they are then passed both together on a mandrill through a draw-plate, so as to bring the two into complete contact. The lead pipe is proof against the action of gas, and the copper pipe, at the same time that it supports and defends it, makes a better appearance.

II. Chemical Science.

§ Chemistry.

1. *Veratrine, a new Vegetable Alcali.*—M. M. Pelletier and Caventou, have been actively engaged in searching amongst those vegetables which possess strong powers over the animal system, for new alcalis or other principles; they have lately succeeded in finding one in the *veratrum sabadilla,* or cevadilla, the *veratrum album,* or white hellebore, and the *colchicum autumnale,* or meadow saffron. It is an alcali, and has been named *Veratrine.*

The substance principally acted upon, was the seed of the cevadilla. Ether separated from them a yellow, greasy, unctuous, acid substance, being a mixture of elaine, stearine, and a peculiar acid. The acid was obtained by distilling the substance, saturating the portion that passed over with barytes, evaporating to dryness which produced a white salt, adding phosphoric acid which caused the appearance of crystals, and distilling at a low heat, when the acid passed over. This acid is crystallizable, soluble in water, fusible and volatile at a low temperature, soluble in ether and alcohol, and forms salts with bases. It has been called the *Cevadic acid.*

The seeds were then acted on by boiling alcohol, which, as it cooled, deposited a little wax. The solution was evaporated down to an extract, re-dissolved in water and again concentrated by evaporation, during which, a portion of colouring matter fell. Acetate of lead was poured into the solution, and an abundant yellow precipitate fell, leaving the fluid nearly colourless. The excess of lead was thrown down by sulphuretted hydrogen, and the filtered liquor concentrated by evaporation, was treated with magnesia and again filtered. The precipitate boiled in alcohol gave a solution which, on evaporation, left a pulverulent matter, extremely bitter, and with decidedly alcaline characters. It was at first yellow, but by solution in alcohol and precipitation by water, was obtained in a fine white powder.

The precipitate by the acetate of lead, on examination, gave gallic acid, hence it is concluded that the new alcali existed in the seed as a gallate.

Veratrine was found in the other plants before-mentioned. It is white, pulverulent, has no odour, but excites violent sneezing. It is very acrid but not bitter; it produced violent vomitings in very small doses, and according to some experiments, a few grains may cause death. It is very little soluble in cold water, boiling water dissolves about $\frac{1}{1000}$ part, and becomes acrid to the taste. It is very soluble in alcohol, and rather less soluble in ether. It fuses at 50° C=122° Fahrenheit, and then appears like wax; on cooling it becomes an amber-coloured translucid mass: heated more highly it swells, decomposes, and burns; decomposed by oxide of copper, it gives no trace of nitrogen. It acts on test papers like an alcali, and forms uncrystallizable salts by evaporation; these salts appear like a gum. The super-sulphate only seems to present crystals. Strong solutions of these salts are partially decomposed by water; veratrine is thrown down and the solution becomes acid. The sulphate appears composed of

Veratrine	93.723	100
Sulphuric acid	6.227	6.6441

but excess of acid was present, and supposing it an acid salt, and in analogy with those of brucine, the neutral salt will be

Veratrine	100
Sulphuric acid ..	3.322

The muriate contains

Veratrine	95.8606	100
Muriatic acid ..	4.1394	4.3181.

Iodine, by acting on veratrine, produces an hydriodate and an iodate; chlorine produces a muriate and a chlorate.

Ultimately, cevadilla was found to contain elaine, stearine, cevadic acid, wax, acid gallate of veratrine, yellow colouring matter, gum, lignin, and ashes.

White hellebore on analysis, in the same manner gave very nearly the same results. Starch was found in it in addition to the other principles, and the ashes varied a little in the salts they contained. Both contained carbonate of lime, phosphate of lime, carbonate of potash, silex; the first also, muriate of potash; and the white hellebore sulphate of lime.

The colchicum was examined precisely in the same manner, and gave the same results, with the addition, however, of inuline. The ashes were in such small quantity as to be neglected.—*Annales de Chimie*, iv. p. 69.

Dr. Meissner, also claims to have discovered in cevadilla, a new alcali. He prepares it by making a tincture of the seeds with moderately strong alcohol, evaporating till a resinous matter remains, and rubbing the matter down with water; to the brown liquid obtained by filtration subcarbonate of potash is added, till precipitation ceases; the precipitated matter is then well-washed in water and dried.—*Journal de Pharmacie, Mai,* 1820.

2. *Benzoic Acid.*—M. Vogel has found this acid in the tonquin-bean between the skin and the kernel. It occurs in the form of crystals; these melt, at a moderate heat, into a transparent fluid, which on cooling forms a crystallized mass. At a higher heat it sublimes and forms fine brilliant needles, which are similar in smell to the bean. A concentrated solution of them in alcohol reddens litmus paper, and becomes milky with water. They form a salt with ammonia which precipitates iron of a brown colour. In fine, they are crystallized benzoic acid.

M. Vogel has also found the same substance in the flowers of the *trifolium melilotus officinalis.* He obtained it by digesting the flowers in pure boiling alcohol; on cooling a fatty substance falls, and in two or three days long crystals of benzoic acid. To separate these substances they are digested in boiling water, and the liquid filtered, the acid passes through in solution, and when slightly evaporated yields the benzoic acid in crystals.

3. *Antiseptic Power of the Pyroligneous Acid.*—Results of some experiments, made by Mr. W. Ramsey.—See *Edinburgh Phil. Journal*, iii. p. 21.

A number of herrings were cleaned on the 10th of July, 1819; and without being salted, were immersed for three hours in distilled pyroligneous acid, specific gravity 1012. When withdrawn they were softened, and not so firm as fish taken out of common pickle. They were hung up in the shade; July and August were hot months, but the herrings had no signs of putrefaction about them, but had a very wholesome smell combined with that of the acid. One being broiled the empyreumatic smell was very strong. The rest, after six months, were in complete preservation.

It was afterwards found that the period of immersion had been too long. If the fish are simply dipped in acid of specific gravity 1012, and dried in the shade, it is sufficient for their preservation; and such herrings, when boiled, are very agreeable, and have not the disagreeable empyreuma of the former.

A number of haddocks were cleaned, split, and slightly sprinkled with salt for six hours; then being drained, they were dipped for about three seconds in pyroligneous acid, and hung in the shade for eight days. On being broiled, they were of an uncommonly fine flavour, delicately white, and equal to the highly esteemed Finnan haddock.

Herrings were cured in the same way as the haddocks. After being dried in the shade for two months, they were equal in quality and flavour to the best red herrings. The fish retained the shining and fresh appearance they had when taken from the sea.

A piece of fresh beef was dipped for one minute in pyroligneous acid of specific gravity 1012, in July 1819. On March 4, 1820, it was as free from taint as when first immersed. No salt was used in this experiment. A piece of beef was dipped at the same time in pure vinegar, of specific gravity 1009. It was perfectly free from taint on the 18th of November. This experiment indicates antiseptic powers in pure vinegar; some haddocks were cured with it, which remained free from taint, but when cooked had an insipid taste.

When beef is partially salted, and then steeped for a short time in the pyroligneous acid, after being drained and cooked, it has the same flavour as Hamburgh beef. Mr. Ramsey has no doubt, that with proper modifications, the use of the acid may be extended to the preservation of every species of animal food.

In order to ascertain whether the volatile oil in the pyroligneous acid, or the acid itself, was the agent which prevented putrefaction, Mr. Ramsey dipped haddocks and fresh beef in pure vinegar of specific gravity 1009. When fish were allowed to remain in the vinegar a few minutes, he observed that the muscular fibre was immediately acted on, a partial solution of the fish took place, and the acid became milky. When vinegar of a stronger quality was used, the fish was entirely dissolved, particularly if aided by heat. Both fish and beef which were dipped in vinegar, of specific gravity 1009, and which were afterwards dried in a summer heat, remained for a long time after perfectly free from taint.

Mr. Stodart has repeated some of these experiments, and especially those relating to the haddocks, with perfect success in London.

4. *Purification of Pyroligneous Acid.*—M. Stotze, of Halle, has discovered a method of freeing the vinegar of wood from its impurities, by treatment with sulphuric acid, manganese, and common salt, and subsequent distillation. He obtained a prize from the Royal Society of Gottingen for the discovery.

He has also added to the verifications of the antiseptic powers of pyroligneous acid, and has converted bodies into mummies by continued treatment with it.

5. *Acids of Manganese.*—The results of some experiments made by Dr. G. Forshhammer, on the chamæleon mineral, induce that chemist to believe that there are two acids which may be formed by the combination of manganese and oxygen. These exist in combination in the green and red chamæleon; the first being a compound of manganeseous acid, and the other of manganesic acid. M. M. Chevillot and Edwards were the first who described the property manganese has of forming an acid compound with oxygen, but they considered the red chamæleon as a neutral salt, and the green chamæleon as a sub-salt of the same acid.

Dr. Forshhammer's experiments are published in the *Annals of Philosophy*, 16, p. 130. Some of them have been selected and abridged beneath. A solution of green chamæleon was prepared by igniting potash with per-oxide of manganese, and dissolving in water. After solution a deut-oxide remained, oxygen having been transferred to that part of the metal dissolved. The green solution soon became red, and deposited deut-oxide of a brown colour.

Solution of pure potash added to the red solution of chamæleon, did not change the colour; but the least drop of alcohol immediately made it green, and if enough were given, a beautiful colour was produced. If too much were added, all colour disappeared, and deut-oxide of manganese remained.

Hence the difference between the green and red chamæleons appears to depend upon the quantity of oxygen combined with the metal. In forming green chamæleon, a part of the per-oxide used is reduced to deut-oxide, and a part raised to the state of an acid, forming the green compound with potash; this green compound gradually changes; the acid in it divides into two parts, one of which is reduced to deut-oxide, yielding up its oxygen to the other which forms an acid more oxygenated than the first, existing in the red compound. Alcohol, when added, deprives this acid of oxygen, converting it first into the acid in the green chamæleon, and then into deut-oxide.

Solution of potash, containing the powder of carbonate of manganese, was poured into a portion of the red solution; the colour became green, and the prot-oxide of the carbonate became deut-oxide. If too much of the carbonate were added, the whole became deut-oxide.

The acid in the green chamæleon, Dr. Forshhammer has called manganeseous acid; that in the red manganesic acid. The manganeseous acid is very easy of decomposition, when combined with potash it forms a sub-manganesite, and whenever the potash is saturated, or its action weakened, the manganeseous acid is decomposed into deut-oxide of manganese, and manganesic acid: hence the changes of the chamæleon. A green solution exposed to the air turns red by the absorption of carbonic acid, which precipitates deut-oxide of manganese. Acids have the same effect, and even water also in great quantity, by weakening the power of the alcali. When the red colour is changed to green, it is in consequence of the presence of some body absorbing oxygen, as alcohol, prot-oxide of manganese, *&c.*; and Chevreul has shewn that even filtering through paper will have the same effect.

In attempts to get the manganesic acid pure, a solution of the manganesiate of potash was prepared by passing carbonic acid through the green solution, and when filtered, sulphuric acid was added to it; this spontaneously evaporated, gave crystals of sulphate, manganesiate, and bi-sulphate of potash. As in this experiment it was evident that if sulphuric acid at all decomposed the manganesiate, it only did so to form bi-sulphate of potash; it proved that no pure manganese acid could be obtained in this manner. A solution of sub-manganesite of potash was then prepared, and nitrate of lead in solution added to it, which threw down a brown powder, and instantly destroyed the green colour. This brown powder was washed, and then carefully mixed with sulphuric acid, diluted with ten times its weight of water, a portion of the brown powder being preserved to neutralize any excess of acid on the powder. The powder must not be dried before the acid be added to it, and long digestion is necessary for the complete combination of the acid and the lead. In this process the prot-oxide of lead is changed into per-oxide by the manganeseous acid, which itself becomes deut-oxide; and the two combine, forming the brown powder. On adding sulphuric acid the per-oxide of lead is decomposed to form sulphate of lead, and its oxygen combining with part of the deut-oxide of manganese converts it into manganesic acid; this is obtained by decantation, in a separate state.

The manganesic acid thus obtained is in solution 1; it is of a beautiful red colour, and sharp unpleasant taste; it stains the skin and other animal and vegetable substances of a fine brown colour; it destroys the colour of litmus paper rendering it brown by depositing deut-oxide, and would bleach well but that the oxide deposited is of a brown colour itself. When evaporated and heated it decomposes, forming brown oxide of manganese, and giving a smell like that of an excited electrical machine, and the same smell is produced from it by exposure to the sun. When heated with muriatic acid it is entirely decomposed and reduced to prot-oxide, which combines with the acid.

Alcali is not required for the formation of manganesic acid; hydrated per-oxide of lead, sulphate of manganese, and sulphuric acid, will produce it on mixture, but it is then difficult to free it from sulphuric acid. Also, when chlorine prepared from the per-oxide of manganese is passed into a solution of potash, the chlorine volatilizes a small portion of per-oxide of manganese, and converts it, when acted upon by the potash, into manganesic acid, which combines with the alcali. Again, when chlorine was passed through water containing per-oxide of manganese, and potash added to this saturated water, after separation from the excess of oxide, a small quantity of deut-oxide was precipitated, and the solution became red.

All the oxides of metals seem either to form soluble compounds of a red colour with the manganesic acid, or to decompose it, in which case the oxide added is converted into per-oxide, and the acid into deut-oxide. The brown powder thus obtained by lead is soluble in cold nitric acid with a brown colour, when heated it turns red. Hence the brown powder is a chemical compound of the per-oxide of lead, (acting as an acid,) and the deut-oxide of manganese, for neither of these bodies alone are soluble in nitric acid: when heated to the temperature of boiling water, the oxygen of the per-oxide goes to the manganese, and converts it into manganesic acid.

By igniting nitrate of barytes with oxide of manganese, and washing the green powder thus obtained with boiling water, a manganesite of barytes was obtained. It has a beautiful emerald green colour, and when dry, is very little altered by the air.

The manganeseous acid was analyzed by adding to a certain quantity of the green chamæleon, nitrate of lead; this threw down the brown powder, which contained both the manganese and the oxygen of the manganeseous acid, and prot-oxide of lead. This was distilled with certain precautions with bi-sulphate of potash, and the oxygen evolved estimated, it equalled 4.82 French duodecimal cubic inches, at a temperature of 10° centigrade, equal, according to Biot, to 0.1303057 grammes, (1.7127 gr.). The salt in the retort was then dissolved by water, the sulphate of lead separated, and the manganese precipitated by carbonate of potash; this heated gave 0,282 grammes, (4,356 gr.) deut-oxide of manganese, these are considered as equal to 0,261 prot-oxide, which were united with 0.1303 of oxygen in the acid.

Dr. Forshhammer then states the composition of 4 oxides of manganese. The sub-oxide 100 metal +20.576 oxygen; the prot-oxide 100 metal +31.29 oxygen; the deut-oxide 100 metal +42,04 oxygen; and the per-oxide 100 metal +62.819 oxygen; the numbers for the oxygen being nearly as 2, 3, 4, and 6.

The manganeseous acid consists of 100 metal, and 96.847, oxygen, which is nearly as the number 9. But Dr. Forshhammer thinks that from the action of water, *&c.*, the green chamæleon already contains a small portion of manganesic acid, and that the manganesite of potash in solution is, when pure, of a blue colour; a colour sometimes obtained in preparations of chamæleon. In this case the proportion of oxygen would be less, and is placed by Dr. Forshhammer at 8.

The manganesic acid was thus analyzed. A solution of green manganesite of potash was made, and converted by carbonic acid into manganesiate, in doing which 136 of deut-oxide fell. The manganesiate was decomposed by alcohol, and gave 214 of deut-oxide. In the preceding analysis ,282 deut-oxide, +,1093 oxygen, gave manganeseous acid; hence ,136+,214=,350 deut-oxide would have ,1354 oxygen in the acid of the green manganesite used above; and this same quantity of oxygen, with the ,214 of deut-oxide, would exist in the red manganesiate after the action of the carbonic acid. The ,214 equals ,1506 manganese, and ,0634 oxygen, which, added to the other quantity of oxygen, gives ,1988. Hence manganesic acid is composed of 100 metal +132 oxygen, which approaches to the number 12; and considering the difficulty of the analyses, and small quantities of the substances, may be considered a near approximation to the truth.

6. *On the Ferro-prussiates.*—M. Berzelius, in a letter to M. Berthollet, says, that in the ferro or ferrugineous prussiates,

the iron is always in the state of protoxide, and that the other base contains twice as much oxygen as the prot-oxide of iron. The acid of these salts is the prussic, composed as M. Gay Lussac has described. Those ferruginous prussiates which effloresce, such as those of potassa, baryta, and lime, lose their water in a vacuum at the temperature of the air. The effloresced salt is no longer a prussiate but a double cyanuret, containing neither oxygen nor hydrogen. When the double cyanuret of iron and potassium, or of iron and barium, are burnt by black oxide of copper, the resulting gas contains three volumes of carbonic acid gas, and two volumes of nitrogen; one volume of carbonic acid gas remains with the base, and forms with it a kind of double salt, composed of carbonate and cuprate of potassa and baryta. The double cyanuret of iron and lead, gives gas in the proportion of two volumes of carbonic acid to one volume of nitrogen. In these combustions, only traces of water are obtained, which are inseparable from the pulverized substances. The ferruginous prussiate of ammonia cannot be reduced to a cyanuret. It is composed of prussiate of prot-oxide of iron and prussiate of ammonia. When distilled, it gives prussiate of ammonia, and a little water, formed by the conversion of the prussiate of iron to cyanuret; the cyanuret then becomes decomposed, and gives nitrogen gas, leaving a carburet of iron composed of four atoms of carbon and one of iron. When this carburet is heated red, it takes fire, and appears to burn as if in oxygen gas, though it is surrounded by nitrogen, and suffers no alteration. The incandescence is analogous to that exhibited by oxide of chrome, oxide of iron, zirconia, *&c.*, when heated red. The same phænomena may be remarked in the distillation of nearly all the ferruginous metallic prussiates, but with none of them is it so brilliant as with the ferro-prussiate of ammonia. Nearly all the ferruginous prussiates dissolve in concentrated sulphuric acid without decomposition. On attracting water from the air, the acid frequently deposits crystals of a combination of sulphuric acid with the prussiate an acid salt with two bases and two acids. M. Berzelius thought at first, that these salts were formed of cyanurets and sulphuric acid, but as the acid prussiate of the prot-oxide of iron (ferro-prussic acid of Porret,) produces the same phænomenon, it appeared evident that the bases were oxidated, and that the cyanuret was combined with hydrogen. These results make part of a long Mémoire which will appear in the *Mémoires de l'Académie.—Annales de Chim.* XIV. p. 190.

7. *Preparation of Phosphorus.*—M. Julien Javal, in preparing phosphorus lately, observed, that failure took place to a certain extent, when phosphoric acid was used, in consequence of the volatility of this substance at high temperatures. On making the phosphoric acid into bi-phosphate of lime, the process went on well again. As a practical result from his experiments, he advises that on adding the sulphuric acid to the burnt bones in the usual way, only two of the former should be put to five of the latter, in which case a proper bi-phosphate of lime will be obtained. If, by any accident, the prepared bi-phosphate should contain an excess of acid, or rather free acid, then he finds it necessary to cover over the mixture of it and charcoal, when in the retort, with a stratum of charcoal alone, and to get this part of the retort hot before the lower. With these precautions the phosphoric acid which rises is decomposed in passing through the hot charcoal, but otherwise it will be condensed, unacted on.

8. *Metallic Vegetations.*—" M. Goldsmith places a few filings of copper and of iron on a glass plate, at a certain distance one from the other. He then drops a little nitrate of silver on each parcel; the silver soon begins to precipitate, whilst the iron and copper oxidize, and become coloured; then, by a small wooden point, the ramifications are arranged at will, whilst the flame of a taper being placed under the plate, increases the evaporation, facilitates the re-action of the substances, blackens the lower side of the plate, and thus forms as it were a design."—*Annales de Chim.* 14. p. 84.

9. *Muriate of Potash in Rock Salt.*—In consequence of

Dr. Wollaston's discovery of the existence of muriate of potash in sea-water, Dr. Vogel has been induced to search for it in common salt, either from salt springs, or from its native beds. Salt from Hallein and from Berchtesgaden, and the brine from Rosenheim, all yielded a precipitate with the solution of platinum. The simple solution will in no case do this, but when evaporated, until much of the salt has fallen down, then a precipitate may be produced. In testing sea-water for potash, it is also necessary to evaporate till almost all the salt has fallen down, and then no difficulty occurs in detecting the potash by muriate of platinum.

10. *Iodine in Marine Animals.*—M. Chevreul, whilst engaged in some experiments in animal chemistry, discovered the presence of iodine in the bones of the head of the crab, and of the large lobster, (homard,) but could find none in those of the common lobster.

11. *Fulminating Mercury.*—Between 100 and 150 grains of fulminating mercury, on a piece of paper, were lying on a wooden waiter an inch and a half thick, and covered by a glass jar, abundance of glass apparatus lying about. A small portion of the same powder was fired at a few feet distance by a hot coal, and burnt without explosion. By some unknown means, that under the jar *exploded*; it slightly raised the jar without breaking it, but the jar broke in falling, and it disturbed none of the apparatus around. The wood was perforated by a hole as large as the hand.

12. *Test for Copper.*—M. Pagenstecher of Berne has described a very sensible test for the presence of copper. The tests already possessed by chemists for this metal are very delicate, and not few; but still every addition to this branch of chemical knowledge is important. He says " if we drop into a newly-prepared tincture of guaiacum wood, a concentrated solution of a salt of copper, the mixture instantly assumes a blue colour. This effect does not take place when the solution is very weak, as when there is not above half a grain of the

salt to an ounce of water, but then by the addition of a few drops of prussic acid, the blue colour is immediately developed of great purity and intensity. This colour is not permanent, but soon passes to a green, and, at length, totally disappears. In want of prussic acid, distilled laurel water, or that of plum or black-cherry kernels, may be employed. This re-action succeeds, when the proportion of salt to the fluid is not more than a $\frac{1}{45000}$: in this proportion no other test, whether the prussiates of potash, soda, or ammonia, will develope the least indication of the presence of copper."

In using tincture of guaiacum, as a test for copper, care must be taken that no other bodies are present which turn it blue. *Annales Générales des Sciences, &c.*

13. *Process for procuring pure Zirconia.*—Powder the zircons very fine, mix them with two parts of pure potash, and heat them red hot in a silver crucible for an hour. Treat the substance obtained with distilled water, pour it on a filter, and wash the insoluble part well; it will be a compound of zirconia, silex, potash and oxide of iron. Dissolve it in muriatic acid, and evaporate to dryness, to separate the silex. Re-dissolve the muriates of zirconia and iron in water; and to separate the zirconia which adheres to the silex, wash it with weak muriatic acid, and add it to the solution. Filter the fluid, and precipitate the zirconia and iron by pure ammonia; wash the precipitates well, and then treat the hydrates with oxalic acid, boiling them well together, that the acid may act on the iron, retaining it in solution whilst an insoluble oxalate of zirconia is formed. It is then to be filtered, and the oxalate washed, until no iron can be detected in the water that passes. The earthy oxalate is when dry of an opaline colour; after being well washed, it is to be decomposed by heat in a platinum crucible.

Thus obtained, the zirconia is perfectly pure, but is not affected by acids. It must be re-acted on by potash as before, and then washed until the alcali is removed. Afterwards dissolve it in muriatic acid, and precipitate by ammonia. The hydrate thrown down, when well washed, is perfectly pure, and easily soluble in acids.

This process belongs to M. M. Dubois and Silveira. See *Annales de Chim.* XIV. p. 110.

14. *On Artificial Gems.*—M. Douault-Wieland, in an experimental memoir on the preparation of artificial coloured stones, has given the following directions and proportions, as better than those before known.

The base of all artificial stones is the *strass (paste)*, which he called *fondant*, when uniting it to metallic oxides to form the imitations. When worked alone it imitates brilliant and rose diamonds.

The paste is composed of silex, potash, borax, oxide of lead, and sometimes arsenic. The silex should be perfectly pure; if obtained from rock crystal that is the case; if obtained from sand, though of the whitest kind, it ought first to be washed with muriatic acid, and then with water. The crystal, sand, or flint, should be heated red hot, quenched in water, dried, powdered fine, and sifted. The potash should not be mixed with other salt; it ought to be the finest pearlash, or else pure potash, by alcohol. The borax of the markets gives a brown glass; the crystallized boracic acid, from the borax of Tuscany, should be preferred. The oxide of lead should be perfectly pure; if it contains an atom of tin, the glass will be milky. Red lead is preferable to the best litharge, or even to the ceruse of Clichy. It should be analyzed before being used. The arsenic should also be pure.

Hessian crucibles are better than those of porcelain, for though they sometimes colour the matter more, they do not break or run so soon. Either a potter's or a porcelain furnace, may be used, and the fusion should be continued 24 hours; the more tranquil and continued it is, the denser the paste, and the greater its beauty. The four following receipts have given good paste:

No. I.

	Grains.		Grains.
Rock crystal....	4056	Borax	276
Minium........	6300	Arsenic........	12
Pure potash	2154		

No. II.

	Grains.		Grains.
Sand..........	3600	Borax	360
Ceruse of Clichy	8508	Arsenic	12
Potash........	1260		

No. III.

Rock crystal	3456
Minium	5328
Potash	1944
Borax..........	216
Arsenic	6

No. IV.

Rock crystal ..	3600
Ceruse of Clichy	8508
Potash........	1260
Borax........	360

TOPAZ.

Very white paste	1008
Glass of antimony	43
Cassius purple	1

Or,

Paste	3456
Oxide of iron, called Saffron of Mars	36

RUBY.

M. Douault-Wieland succeeded in obtaining excellent imitations of rubies, by acting on the topaz matter. It often happened that the mixture for topazes gave only an opaque mass, translucent at the edges, and in thin plates of a red colour. One part of this substance being mixed with 8 parts of paste, and fused for 30 hours, gave a fine yellowish crystal like paste, and fragments of this fused before the blow-pipe, gave the finest possible imitation of rubies. The result was always the same.

The following are proportions also for rubies :

Paste	2880
Oxide of manganese	72

EMERALD.

Paste	4608
Green oxide of pure copper	42
Oxide of chrome	2

SAPPHIRE.

Paste	4608
Oxide of cobalt	68

This mixture should be carefully luted in a Hessian crucible, and remain 30 hours in the fire.

AMETHYST.

Paste	4608
Oxide of manganese	36
Oxide of cobalt	24
Purple of Cassius	1

BERYL, OR AQUA MARINA.

Paste	3456
Glass of antimony	24
Oxide of cobalt	$1\frac{1}{2}$

SYRIAN GARNET, OR ANCIENT CARBUNCLE.

Paste	512
Glass of antimony	256
Cassius purple	2
Oxide of manganese	2

In all these mixtures, the substances should be mixed by sifting, fused very carefully, and cooled very slowly, being left on the fire from 24 to 30 hours.

M. Lançon has also made many experiments on the same subject. A few of his proportions are as follows:

PASTE.

Litharge	100
White sand	75
White tartar, or potash	10

EMERALD.

Paste	9216
Acetate of copper	72
Per-oxide of iron, or saffron of Mars	1.5

AMETHYST.

Paste	9216
Oxide of manganese	from 15 to 24
Oxide of cobalt	1

15. *Spontaneous Combustion of Cloth.*—About twenty-five pieces of cloth, each of which contained nearly thirty ells,

were deposited upon wooden planks in a cellar at Lyons, on the 8th of July, 1815, in order to conceal them from the armies which then over-ran France. In the manufacture of the cloth, 25lbs. of oil were used for a quintal of wool, and the cloth was quite greasy, each piece weighing from 80lbs. to 90lbs. The cellar had an opening to the north, which was carefully shut up with dung, and the door was concealed by bundles of vine props, which freely admitted the air. On the morning of the 4th of August, an intolerable smell was felt, and the person who entered the cellar was surrounded with a thick smoke which he could not support. A short time afterwards he re-entered with precaution, holding a stable lantern in his hand, and he was astonished to perceive a shapeless, glutinous mass, apparently in a state of putrefaction. He then removed the dung from the opening, and as soon as a circulation of air was established, the cloth took fire. In another corner of the cellar lay a heap of stuffs which had been ungreased and prepared for the fuller, but they had suffered no change. The above particulars were carefully established by M. Cochard. Comte rendu des Travaux de la Soc. Roy. d'Agriculture, &c. de Lyons pour, 1817.—*Edinburgh Journal.*

16. *Evaporation of Spirits.*—Mr. Ritchie, of Perth, has published a curious statement respecting the evaporation of mixtures of alcohol and water. He commences by the following theorem,—"The degrees of cold, induced by the evaporation of spirits of different degrees of strength, are proportional to the strength of those spirits, reckoning from the degree of cold induced by the evaporation of water." This is established by the following experiments:—

"Having made three very delicate hygrometers, according to Leslie's construction, I moistened the bulb of one of them with strong whisky, the bulb of another with a mixture of equal quantities of the same spirits and water, and the bulb of the third with water. I watched the descent of the fluids in the stem till each had gained its maximum of cold, and marked the cold induced by the water 40, by the dilute spirits

64, and by the strong spirits 88. Now the difference between 40 and 64, is 24, and between 40 and 88, is 48. Hence the following proportion 24 : 48 : : strength of the dilute : strength of the strong spirits. This I have tried with different proportions of spirits and water, in different states of the atmosphere, and found the same property uniformly obtain. The experiment requires to be performed with great delicacy and care, as the spirits soon acquire their maximum, after which the fluid in the stem begins to ascend."—*Annals of Philosophy*, xvi. p. 215.

17. *Electrical Experiment.*—The following experiment is described by Professor Moll. Place a thin piece of tin foil vertically between two horizontal and insulated rods of brass, each terminated by a knob, and distant from each other between one and two inches, then pass from one to the other a strong charge of a large electrical battering. The plate of tin will be found pierced by two holes, with their burs in opposite directions. That the experiment may succeed, the tin foil should be thin, and the charge strong, otherwise only two impressions will be seen on the plate.

18. *Improvement in Dyeing.*—Cloths which are dyed in the piece, frequently present at the edges a line corresponding to the middle of the piece of cloth, more lightly dyed than the surface. This sometimes produces a disagreeable effect. From the Count de la Boulaye-Marsillac's experiments, it appears to be occasioned by the water which remains in the wool before immersion in the dye stuff, from the previous wetting process. This water either excludes or dilutes the dye, and renders it less effectual within the cloth than at the surface. To obviate this the Count makes the cloth pass through rollers within and at the bottom of the dye-vat, by which the mere water is very completely expelled. and the dye takes its place. In passing the cloths backwards and forwards in the dye-vat, they are made each time to go through the rollers.

Scarlet cloths thus dyed are so intense in colour as to appear less bright than common scarlet, but this is obviated by adding turmeric or fustic to the bath.

19. *Test for Baryta and Strontia.*—Baryta and strontia may readily be distinguished from each other by the following process:—Make a solution of the earth, whichever it may be, either by nitric, muriatic, or some other acid, which will form a soluble salt with it; add solution of sulphate of soda in excess, filter and then test the clear fluid by subcarbonate of potash; if any precipitate falls the earth was strontia, if the fluid remains clear it was baryta.

20. *On Meteoric Stones, by M. Laugier.*—Among the substances which enter into the composition of aërolites, three may be considered as characteristic: sulphur, nickel, and chrome, the silex, iron, magnesia, and manganese, being common to other lapideous mixtures. Of these three substances the sulphur is least important, because of common occurrence in pyrites: the remarkable circumstance attending it is its constant union with nickel. The nickel has had most importance attached to it, partly because it occurs in greater quantity than the chromium, and partly because found in those masses of iron called meteoric.

The chrome has been considered as of least consequence, because of the smallness of its quantity, and because it has been said to be wanting in some aërolites, as in that from Stannern, in Moravia. But if it be shewn that an aërolite contains *no* nickel, whilst the stone from Moravia does contain chrome, will it not be proper to consider chromium as the most important character of these peculiar bodies?

M. Laugier has drawn this consequence from a comparative examination of the stone which fell recently at Jonzac, and that of Moravia, specimens of which were given him by M. M. Haüy and Brongniart.

The stone from Jonzac fell on the 13th of June, 1819, that from Moravia on the 22d of May, 1808. Both present the physical characters of aërolites, differing only in one of them. Meteoric stones are generally covered by a compact uniform dull black crust of a certain thickness; the crust of these two stones, on the contrary, is light grey, shining and glassy.

The mode of analysis was by successive treatment with alcali and acid. The stone from Jouzac, thus examined, gave no nickel, and when other essays were made to discover this metal, they all failed. Hence it is concluded that there was none in the stone, and from the facility of finding the metal when present, there can be no doubt that the conclusion is well founded.

It is composed of

Oxide of Iron	36
Silex	46
Alumina	6
Lime	7.5
Oxide of Manganese ..	2.8
Magnesia	1.6
Sulphur	1.5
Chrome	1
	102.4

the excess resulting from oxidation of the metals.

All the meteoric stones which Mr. Laugier examined, were found to contain chromium. He first found it in the stone from Verona, which fell in 1663, and it was not long before its existence was ascertained in the stone from Moravia, which contains a $\frac{1}{100}$ part. It was found too in the native iron of Siberia.

In testing for the chrome M. Laugier observes, that " if muriatic acid be immediately added to the alcaline solution, there remains no symptoms of the chrome; but that, if before adding the acid, the alcaline solution be boiled with access of air, and long enough to precipitate all the oxides of maganese and iron, the yellow colour of chromate of potash will remain, however small its quantity, and nothing is required afterwards but to saturate the alcali by nitric acid, and to add a solution of nitrate of mercury."

It results from M. Laugier's experiments, that among the meteoric stones known, one contains *no* nickel, whilst all contain chrome; and hence he concludes that of the two, chrome is the most important character.—*Mémoires du Muséum*, vi. p. 233.

III. Natural History.

§ Medicine, &c.

1. *Remedy for Bronchocele.*—The *Bibliothèque Universelle*, for July 1820, contains a paper by Dr. Coindet, on a new remedy for the goitre, which, from his experience, appears to be very effectual. From the circumstance that burnt sponge formed the basis of all successful remedies as yet used for this disease, Dr. Coindet considered that iodine might be the particular substance that was useful; and, in consequence, applied it in different forms. One preparation was a solution of forty-eight grains of hydriodate of potassa, equivalent to thirty-six grains of iodine, in an ounce of water.

Sometimes iodine is dissolved in this solution, to increase the force of the remedy in very difficult cases.

Another preparation, called tincture of iodine, was made by dissolving forty-eight grains of iodine in an ounce of alcohol of 35 (S. G. 842).

The quantity for an adult was ten drops of one of these preparations, in half a glass of syrup of capillaire and water, taken early in the morning, fasting; a second dose was given at ten o'clock; and a third in the evening, or at bed-time. At the end of the first week, fifteen drops were given in place of ten, three times a day; and, in a few days after, when the effect seemed evident on the tumours, it was increased to twenty drops. This quantity has rarely been increased, and was generally sufficient to dissipate the larges goitrest.

After about eight days' treatment the skin becomes less tense, and apparently thicker. The tumour softens, as becomes evident to the touch; the goitreous tumours, if there are several, become more distinct and separate; they soften and gradually dissolve. In many cases the nucleus, or part which is organically deranged, becomes harder, diminished in size, and isolated; sometimes they become moveable; a circumstance of great advantage in those cases where an operation is necessary.

In some cases the cellular structure, which pervaded the tumour, remains swelled, and feels to the touch like an empty cyst. Frequently also the goitre disappears only partially, but to an extent sufficient to be neither inconvenient nor a deformity. In many cases it is dissolved, destroyed, and dissipated in from six to ten weeks, so as to leave no traces of its previous existence.

That the effect of the remedy might be obtained free from any other effect, all local applications were avoided, which either by compression, or from the saline substances they contained, could produce any interfering result.

2. *Antidote for Vegetable Poisons.*—M. Drapiez has ascertained, by numerous experiments, that the fruit of the *feuillea cordifolia* is a powerful antidote against vegetable poisons. This opinion has long been entertained by naturalists, but it has not before been verified by experiments made in any part of Europe. M. Drapiez poisoned dogs with the rhus toxicodendron, hemlock and nux vomica; all those that were left to the effects of the poison died, but those to whom the fruit of the *feuillea cordifolia* was administered recovered completely, after a short illness. To see whether this antidote would act in the same way applied externally to wounds, into which vegetable poisons had been introduced, he took two arrows, which had been dipped in the juice of manchenille, and slightly wounded with them two young cats; to one of these he applied a poultice, composed of the fruit of the *feuillea cordifolia*, while the other was left without any application. The former suffered no inconvenience except from the wound, which speedily healed; while the other, in a short time, fell into convulsions, and died.

It would appear from these experiments, that the opinion entertained of the virtues of this fruit, in the countries where it is produced, is well founded. It would deserve in consequence to be introduced into our *Pharmacopœias* as an important medicine; but it is necessary to know, that it loses its virtues if kept longer than two years after it is gathered.—*Medical Journal.*

3. *Vegetable Antidotes to Poison.*—Dr. Chisholm, in a paper read to the Society at Geneva, states, that the juice of the sugar-cane is the best antidote known for arsenic. It has been tried upon various animals in the West Indies with complete success, and always succeeds. Its power in the island of Nevis is generally known.

Dr. Chisholm also mentions the singular powers of a plant, well known to the Indians, as a remedy for the ophthalmia; it is called *akouscrounie* and *warannie* by them, and *eye-root* by the white people. It grows in *la Guyane*, in the neighbourhood of Demerara, in a sandy soil, and is a species of bignonia, which Dr. C. has since called ophthalmica. An Indian prepares the remedy from the root of the plants, by first stripping off the brown epidermis, and then separating a fibrous pulpy part immediately beneath; this he presses on cotton so as to collect the juice, and then by means of a paper funnel conveys a drop or two of it into the eye. This is repeated once a day for three or four days, in which time the cure is generally completed. Dr. Chisholm had occasion in his own practice to apply this remedy in three cases; and having only the dry root, he rasped off the outside, and then made a strong infusion of the part beneath. Six drops of this infusion were introduced into each eye once a day, and in six days' treatment they were perfectly cured, though they had suffered for many weeks previously.

4. *On the Poison of the Viper.*—M. Configliacchi has been engaged on experiments with this poison. It was obtained by pressing the vesicles behind the canine teeth into a watch-glass, and applied by means of a needle.

He established, in the most positive manner, that this poison had no effect, except when introduced into the blood-vessels; flour-pills were dipped into the poison, and swallowed by birds without producing any injury to them.

One object was to ascertain the effect of Voltaic electricity on birds poisoned by this venom. Some birds dead, but still warm, were subjected, with others killed, either by breaking the neck, suffocation, or decapitation, to the powers of a pile of

24 pair of plates. excited by a solution of sulphate of alumine, one pole being connected with the spinal marrow, and the other with a muscle of the knee. The result was, that the irritability of the muscles was considerably diminished in those animals killed by the poison, its duration not being more than one-fourth of that of the other animals, or even one-sixth of that of the decapitated birds. It was also so weak, that four times the quantity of plates did not produce an equal effect in them.

Another result was, that, when poisoned birds not yet dead, were submitted to voltaic action, their death was hastened. The mean of three experiments gave six minutes as the difference between the death of poisoned birds electrified, and not electrified.

It was also ascertained that birds poisoned by prussic acid, more or less strong, as laurel-water, concentrated to various degrees, gave the same results, except that the duration either of the pain or of the irritability of the muscles was much shorter than with the viper poison.

5. *Cure for the Hydrophobia.*—The number of remedies for this dreadful malady, of which accounts have lately been given, is rather remarkable. Dr. Lyman Spalding, one of the most eminent physicians of New York, announces, in a small pamphlet, that for above these 50 years the *Scutellaria Lateriflora* has proved to be an infallible means for the prevention and cure of the hydrophobia, after the bite of mad animals. It is better applied as a dry powder than fresh. According to the testimonies of several American physicians, this plant, not yet received as a remedy in any European *Materia Medica*, afforded perfect relief in above a thousand cases, as well in the human species as in the brute creation (dogs, swine, and oxen.) The first discoverer of the remedy is not known; Drs. Derveer father and son,) first brought it into general use.—*Phil Mag.* lvi. p. 151.

6. *Substitute for Peruvian Bark.*—M. Ré, professor of Materia Medica at the Veterinary School at Turin, has announced that the *Lycopus Europæus* of Linnæus is a complete succedaneum

for Peruvian bark. It is called by the peasants of Piedmont, where it is found in great abundance in marshy places, the Herb of China.

7. *Plantain-Root a Febrifuge.—Switzerland.*—Dr. Perrin has lately read to the Society of Natural Sciences, of which he is a member, observations made on the febrifugal virtues of the roots of the plantain *(plantago major, minor, et latifolia, Linn.)* He is of opinion that it may be employed with advantage in intermittents. The question may easily be decided, as the plant is common every where.

8. *Medical Prize Question.*—A satisfactory answer not having been given to the question, "Can the existence of Idiopathic fever be doubted," proposed last year by the Société de Médicine of Paris, it is re-proposed, the greatest latitude being given to candidates, in the choice and developement of their opinions.

The prize will be a gold medal, of 300 francs value; but, as a further stimulus, the society will, if there be opportunity, award gold medals, of 100 francs value, to the mémoires which may most nearly obtain the prize, and silver medals of emulation.

The concourse will close on the 30th September, 1821. The mémoires, written in French or Latin, to be sent, carriage-free, before then, to the Secrétaire Générale de la Societé de Médicine, Rue St. Avoie, No. 39.

9. *Prize Question.*—The Academy of Sciences of Paris, propose the following:—" To follow the developement of the triton, or aquatic salamander, in its different degrees from the egg to the perfect animal, and to describe the change which it undergoes interiorly, principally in respect to its osteology and the distribution of its vessels."

The Prize, of the value of 300 francs, will be adjudged in the public sitting of 1822. The utmost term for the transmission of Mémoires is January 1, 1822.

10. *Prize Question.*—The Society of Sciences at Copenhagen, have proposed the following:—

"Quibus naturæ legibus regitur primaria evolutio corporum animalium, et formam sive regularum, normalem, sive abnormem adsciscant?"

The author of the best answer to this question will receive a gold medal, of the value of fifty ducats. The Memoirs should be addressed, with the usual forms, before the end of December 1820, to the Secretary of the Society, Professor Oersted at Copenhagen.

§ Mineralogy, Geology, Meteorology, &c.

1. *Chromate of Iron in Shetland.*—It has been recently asserted that the chromate of iron was discovered in Shetland by Dr. Hibbert. Without wishing to undervalue Dr. Hibbert's labours, we must, in justice to Dr. Traill, remark that he pointed out the existence of this mineral in Unst, many years ago. It is true that he calls it magnetic iron ore, but the existence of a chromate was then unknown; nor indeed, we believe, had chrome been discovered at the time Dr. Traill wrote his account. The substance, as we understand, is so abundant that the ground is strewed with it, so that it could not be overlooked.

2. *Boracic Acid.*—Dr. Pleischl, of Prague, has given the following as the composition of crystalline hydrated boracic acid:—

Dry acid..........	54
Water............	45

During the experiments made to ascertain these proportions, Dr. Pleischl endeavoured also to ascertain the action of dry boracic acid on dry chloride of barium. The results coincided with those of Gay-Lussac and Thenard; no decomposition took place nor was any new compound found.

3. *Fluoric Acid in Mica.*—M. Rose, of Berlin, at present working in the laboratory of M. Berzelius, has ascertained that

all the kinds of mica, contain fluoric acid. Two species from Sweden contained a considerable quantity.

4. *Tremolite.*—M. C. G. Retzius analyzed the tremolite, and found it to contain—

Silex	54.26
Lime	23.16
Magnesia	7.56
Carbonate of Lime	13.86
Loss	1.16
	100.

5. *Volcanoes of Tartary.*—M. Abel Remusat, in a letter to M. Louis Cordier, relating to the origin of the sal ammoniac, obtained by the Calmucs, and by them distributed through Asia, quotes the following passage from the Japanese edition of the Chinese Encyclopædia, in the king's library, which not only describes the source of this salt, but also two active volcanoes in the interior of Tartary.

"The salt, named (in Chinese) *nao-cha,* and also salt of Tartary and volatile salt, is obtained from two volcanic mountains in Central Tartary. One is the volcano of Tourfan *, which has given to this town (or rather to a town three leagues to the east of Tourfan,) the name of Ho-Tcheou, or town of fire. The other is the white mountain in the country of Bisch-Balikh †. These two mountains continually emit flame and smoke. There are cavities in them in which a greenish liquid collects, which, when exposed to the air, changes into a salt, which is the *nao-cha;* the people of the country collect it for the preparation of leather.

A column of smoke may be continually seen coming from the Tourfan, which, in the night, is replaced by a flame similar to that of a flambeau. Birds, and other animals, illuminated by it, appear of a red colour. This mountain is called the *Hill of*

* Lat. 43°. 30'., long. 87°. 11'. according to P. Gaubil.

† A town situated on the river Ili, to the S. W. of the lake of Balgasch which the Chinese name the *hot sea.* The latitude of the lake of Balgasch, is 46°.; long. 76°. 11'. according to P. Gaubil.

Fire. Sabots, or wooden shoes, are worn by those who collect the *nao-cha*, for shoes of leather would be soon burnt.

The people of the neighbourhood also collect the mother-waters, which they boil in vessels, and obtain from them the sal ammoniac in lumps or loaves, like those of common salt. The whitish *nao-cha* is considered the best. The nature of the salt is very penetrating. It is suspended in a stove to make it very dry, and ginger is added to it to preserve it. Exposed to cold, or to moisture, it deliquesces and is lost."

M. Remusat adds in his observations, that it is a curious fact, and very little known, that there are two volcanoes actually in combustion in the central regions of Asia, 400 leagues from the Caspian, which is the nearest sea to them.—*Annales des Mines,* v. p. 135.

6. *Temperature of Lakes.*—From observations made by M. De la Bèche, on the lakes of Thun and Zug, in Switzerland, the following temperatures were found to exist at different depths. The lake of Thun is about midway, between a point called the Nase, and the village of Leissingen.

	Fahr.
At a depth of 105 brasses the temperature was	41°5
.... 50	41°5
.... 15	42°
At surface	60°

The bottom of the lake was sandy, and the water not very transparent; the thermometer and weight disappeared at the depth of 7 or 8 feet.

The observations on the lake of Zug were made about a league from the town, and in the direction of Mount Riggi.

	Fahr.
At a depth of 38 brasses the temperature was	41°
.... 25	41°
.... 15	42°
At surface	58°

The water of this lake was clear, and the lead brought up very small gravel from the bottom.

17. *Organic Remains.*—M. de la Buhe, in a letter to Professor Pictet, says, " I was much surprised to see in this collection (belonging to Professor Meissner, of Berne,) the teeth of a mastodon, and those of other animals of less size, enveloped in the coal of Alpnach (if I do not deceive myself,) near the lake of Zurich. Mr. Meissner informed me that the stratum of coal occurred in banks of sandstone gres. This is a circumstance which ought to draw the attention of the Swiss geologists. The fact is certain. The teeth are black, and appear strongly impregnated with bitumen.

18. *Falling of a Mountain.*—On the 8th of July last, at four o'clock in the morning, a part of the mountain *Sichen-Rethrenberge*, near Moselle, in the circle of Cochereim, ten leagues from Coblentz, fell into the river. A movement almost insensible, but neverless progressive, has been observed for many years of this enormous mass. The damage occasioned by it is almost incalculable ; more than 20 vineyards have disappeared.

A neighbouring mountain, called *der Kessel*, threatens also to fall. Enormous crevices occur in it near the middle, and at the summit, and the lower part descended three feet on the morning of the 8th, with the loosening and falling of many large pieces. It is feared that this mountain falling into the Moselle will stop it up, and cause sad distress.

19. *Earthquake.*—A strong shock of an earthquake was felt at Inspruck, on the 17th of July. It lasted four seconds. It is curious that the shock happened at the very hour of the day on which the people of the place were assembled together in prayer, which, according to a vow made in 1670, was to be made annually, in consequence of a dreadful shock which happened at that time.

10. *Red Snow.*—The red snow has appeared this year much sooner than usual, and, though at two leagues from the convent of St. Bernard, there was no place without snow, except some steep rocks ; yet it was decidedly red at the foot of inclined places, and began to reunite in the channels formed by the

waters. It appears evident that it cannot be attributed to any vegetable powder.—*Tableau des Observation, &c. May,* 1820.—*Bib. Universelle.*

11. *Regions of perpetual Snow.*—The following Table is from the termination of a *Mémoire*, by M. Alex. de Humboldt, on the limit of perpetual snow in the Himâlâya Mountains and the equatorial regions. See *Annales de Chim.*, XIV. p. 5.

Parts of the world, where the Mountains rise above the Limit of perpetual Snow:

Equator. —*Andes of Quito.* (Africa?)

10° of lat.—Sierra de Merida, Sierra de Santa Marta, (Mont Al Komri?)

20° of lat.—*Plain of Mexico, Mowna Roa* of the Sandwich Isles, High Peru (New Holland?)

30° of lat.—*Himâlâya,* Atlas near Morocco, *Etna? Sierra Nevada de Grenade,* Cote de Caramania, Chili, (New Holland?)

Heights of perpetual Snows measured.

Andes of Quito (lat. 1°—1° 30′) 2,460 toises; Volcano of Puracé near Popayan (lat. 2° 18′) 2,420 t.; Tolima (lat. 4° 46′) 2,380 t. (?) Nevados of Mexico (lat. 18° 59′—19° 12′) 2,350 t. No perpetual snows on the peak of Teneriffe (lat. 28° 17′) 1,908 t.; Himâlâya, (lat. 30° 40′—31° 4′) southern side, 1,950 t.; northern side 2,605 t. (?) Sierra Nevada of Grenada, the summit not the inferior limit (lat. 37° 10′) 1,780 t.; Etna (lat. 37° 30′) but only spots of snow 1,500 t.; the summit, which also perhaps does not enter the curve of perpetual snow, 1,719 t.; Caucasus (lat. 42°—43°) 1,650 t.; Pyrenees (lat. 42°,5—43°) 1,400 t.; Swiss Alpes (lat. 45¾°—46½°) 1,370 t.; Carpathian Mountains (49° 10′) 1,330 t.; Norway (lat. 61°—62°) 850 t.; (lat. 67°) 600 t.; (lat. 70°) 550 t.; (lat. 71½°, but under the influence of summer fogs, 366 t.

The heights of the places printed in Italics have been measured.

IV. General Literature.

1. *Modern Greek Literature*—M. Koumas, first professor in the great college at Smyrna, and distinguished by his learning among the Greeks, has just published at Vienna the two last volumes of his *Course of Philosophy*. The whole work is a methodical abstract of all the best compositions of the German philosophers. Its object is to instruct the Greeks in modern philosophy, and its circulation is likely to be very considerable.

The printing-office established at Chios has commenced its operations, and is now in full activity. Its first production is an excellent discourse of M. the Professor Bambas, read the year before last, at the opening of the course of the great college of Chios. This work is so elegant in its typography, that it might seem to come from the presses of London or Paris. This office will gradually spread through Greece a number of valuable works that may contribute to the regeneration of this once classical land.

A college on a large scale is about to be founded at Zagori, in the province of Epirus. The voluntary donations for this establishment amount already to 60,000 francs. M. Neophytos Doucas, a learned Greek ecclesiastic himself gave the sum of 10,000 francs.

2. *Philology.*—M. Frederick Adelung, counsellor of state to the Emperor of Russia, has lately published, in 153 pages, *A View of all known Languages and their Dialects.* In this View, we find in all, 987 Asiatic, 587 European, 276 African, and 1,264 American languages and dialects enumerated and classed: a total of 3,064. This remarkable publication is only the introduction to a *Bibliotheca Glottica,* on which this indefatigable philosopher has been long employed.

3. *Ancient Latin Manuscripts.*—Baron Niebuhr, Prussian ambassador to the Holy See, has again discovered and published several ancient MSS. hitherto unknown. They are chiefly

fragments of Cicero's orations, pro M. Fonteio et pro C. Rabirio; a fragment of the 91st book of Livy; two works of Seneca, &c. Baron Niebuhr has dedicated this edition to the Pope, by whose favour he was enabled to discover these literary treasures in the library of the Vatican.

4. *Excavations at Pompeia.*—A public edifice has recently been discovered near the forum of Pompeia, which is supposed to be the Chalcidicum, and an inscription importing that the edifice was built at the expense of the Priestess Eumachia. A few days after the above discovery, a statue of the same priestess was found in perfect preservation. This statue is said to very far surpass in grace, elegance, and grandeur, all the works of art that had previously been dug from the ruins of this town.

5. *Population, &c., of Paris.*—From a work lately published in Paris, it appears, that that city contains 714,000 inhabitants, of which 25,000 are not domiciled. The consumption of bread annually is 113,880,000 kilogrammes (251,336,719lbs.); of oxen, 70,000; of heifers, 9,000; calves, 78,000; sheep, 34,000; swine, 72,000; eggs, 74,000,000; pigeons, 900,000; fowls, 1,200,000; wine, 870,000 hectolitres (22,968,000 gall.)

6. *Population of Sweden.*—According to the last census, taken in 1819, the population of the kingdom of Sweden amounted to 2,543,412 inhabitants. The amount of the registers of what is called the civil state of Stockholm, for the year 1819, has produced a result unfavourable for the population. The births were 2,329, and the deaths 3,238; a diminution has therefore taken place of 909 individuals. Almost one-half of the children are born out of marriage; out of three children one has invariably died. The marriages have been 504, and the divorces 24.

7. *Population of Glasgow.*—An actual survey, to determine the population of Glasgow, was completed in February, 1820. The following is an abstract of the information derived from it:

Population of the ten parishes within the Royalty		75,169
Barony Parish.		
Anderson district	7,113	
St. Vincent St. and Blythswood estate district	7,941	
Port Dundas district	7,598	
Calton and Mile-end district	15,616	
Bridge-town district	13,593	51,861
Gorbal's parish, including Hutcheson-town, Lauries-town and Trades-town		21,768
		148,798

As several thousand persons had left the population district for want of work during the few months which preceded the enumeration, and as some of these persons may be expected to return, the population may be fairly stated at 150,000 persons.

8. *Statistics of America.*—The superficies of the territory of the United States, from the Atlantic to the great ocean, is estimated at 2,257,000 square miles, and the population at 11,000,000. The proportion of whites to blacks has increased as follows, since the year 1790. In that year there were 27 blacks to 100 whites; in 1800, the proportion was 20 to 100; and in 1810, only 19 to 100. The number of emigrants that arrived in the different states in 1794 was about 10,000; in 1817, 22,240, of whom 11,977 were British or Irish. From the British possessions in America, there arrived, in the same year 2,901 individuals.

9. *Carmine.*—M. Von Grotthus says, that carmine may be freed from its yellow shade, by treatment first with ammonia, and then with acetic acid and alcohol.

10. *Death of an Elephant.*—An elephant had been brought to Geneva for exhibition some months ago, and was found to be remarkably obedient and docile. In removing this animal from place to place, it was not confined in a caravan, but passed openly by the streets and roads, attended by three conductors, and no accident had as yet happened in this way; but, on removing it from Geneva, the animal became ungo-

vernable, pursuing its guardians, and endeavouring to do mischief. It returned towards Geneva again, and, by various means, was got into a place of security; and then its proprietor, intimidated by a former accident, resolved to have it put to death. The first intentions were to poison it, and, for this purpose, three ounces of prussic acid were mixed with ten ounces of spirits, and given to it. The animal took the bottle, and drank the liquor; but, after the lapse of some time, did not seem at all affected by it. Three balls were then prepared, each containing one ounce of arsenic, mixed with sugar and honey, and were eaten by the elephant. This poisoning commenced at five o'clock in the morning, and, at the end of an hour, not the slightest effect was produced on the animal. Finding these means ineffectual, orders were given, and the animal shot with a four-pound ball in the head.

After a while, the animal was dissected for the museum, but the muscular parts were given to the people, who took it home as food. Between three and four hundred persons ate of it without any fear from the poison, and without any ill effects except from indigestion.

This elephant was from Bengal, was about nine feet high, and ten years of age.

11. *On the Columns of the Athenian Temples, by Thomas Allason, Esq.*

To the EDITOR *of the* JOURNAL *of* SCIENCE *and the* ARTS.

The following passage is extracted from the *Travels* recently published by the Rev. Thomas Hughes: " Amongst the many observations made by Mr. Cockerell, upon the architecture of the Parthenon *I remember one,* which seemed very delicate and curious; it related to the entasis, or swelling of its beautiful and finely-proportioned columns. With a great deal of difficulty he measured them, and found by a straight line stretched from the capital to the base, that this swell, at about one-third of the height, equalled one inch; that in the Temple of Jupiter at Ægina, half an inch, which was in proportion to the other; so that he had no doubt but that there was a general rule on this point with the ancient architects: this protuberance is so delicate, that it must be ascertained by measurement; the eye alone cannot perceive it: the fact had escaped Stuart, and our other most accurate observers."

From this relation it might be too readily concluded that Mr. Cockerell made the discovery therein mentioned, whereas the fact is otherwise. It was myself who first noticed this peculiarity, during an extensive tour through the Morea, with the Messrs. Spencer Stanhopes, in the year 1814, when, having formed my opinion, by a careful examination of the magnificent remains of the ancient structures at Athens*, I communicated the results to Messrs. Cockerell, Fauvel, Baron Haller, Lusieri, and Tupper; the two former gentlemen, however, constantly opposed my ideas upon the subject. It was, indeed, my actual intention to have ascertained the nature and extent of the *entasis* of the columns of all the temples and other ancient buildings at Athens; various unforeseen circumstances, however, deferred its execution, which was finally rendered impracticable by a severe illness, that obliged me to take a very precipitate departure from the Athenian territory, after a residence of about five months. It was thus arranged, that Baron Haller and Mr. Cockerell should themselves measure, and make the necessary experiments upon the columns of the Parthenon, the Temple of Theseus, &c., and transmit the results to Mr. Stanhope and myself.

We both felt so desirous respecting this inquiry that, on our arrival at Naples, we wrote to Mr. Cockerell, reminding him of the obligation he had taken upon himself; it was, however, more than a year afterwards that I received, through Mr. R. Smirke, an admeasurement of the columns of the Parthenon, confirming my opinions in every particular. I shall also here beg to observe, that my remarks respecting the *entasis* of the Greek columns was noticed in a report read by Mons. Barbie du Boccage to the Institute, several months prior to my receiving Mr. Cockerell's communication.

I shall now take the liberty to inquire of the Rev. Thomas

* See note to preface of my work on the Antiquities of Pola.

Hughes, why he relates this circumstance from memory, rather than from the authority of his journal? I may, perhaps, venture to assign the genuine reason. The transactions alluded to, occurred in 1814, twelve months after this gentleman had left Athens; and therefore it was not possible for his journal to contain one tittle of the matter. Why also has the Reverend Gentleman stated, that " this protuberance is so delicate, that it must be ascertained by measurement, the eye alone cannot perceive it?" The reason is obvious, Mr. Cockerell well knew that I had no other means of satisfying myself respecting the *entasis*, than from the most attentive ocular observation. This opinion, therefore, is hazarded, in order to support his pretensions to the discovery, in opposition to mine, as I had not the opportunity to pursue the satisfactory means which he was enabled subsequently to apply.

In requesting your insertion of the foregoing statement, I am not so anxious to claim the merits of a discovery, although unnoticed by so many distinguished travellers, as to direct the attention of professional men and others, to so important a feature, upon which principally depends the effect and beauty of Grecian architecture.

I am, your obedient humble servant,

THOMAS ALLASON.

27, *South Molton-street, August* 21, 1820.

12. *Geological Maps and Works.*—The Geological Map of England, published by Mr. Smith, and the more recent and accurate one of Mr. Greenough, have proved of great use and value to the geological traveller; a set of small, but accurate, county maps, coloured upon the same plan, would prove, perhaps, a more valuable acquisition, and we trust that such a desideratum will soon be carried into effect.

We are glad to understand that a geological map of Scotland will shortly be published by Dr. Mac Culloch, whose accurate and extensive geological investigations in that part of the kingdom, peculiarly fit him for the task.

We are also informed that Dr. Mac Culloch has nearly completed an elementary work on geology, and that he is engaged in a description of Shetland, upon the same plan as that of the Western Isles of Scotland, which he published last year.

13. *Mineralogy of Scotland.*—Prehnite has hitherto only been found in Scotland in trap; it is thus abundant near Glasgow and Dumbarton; it also occurs at Edinburgh, in Fife, and in the isles of Mull, Rasay, Arran, and Sky.

This mineral has lately been found in the north of Scotland, in gneiss; it occurs in the same botryoidal form as in the Kilpatrick hills. It has also been found in the same rock, imperfectly crystallized, in the island of Guernsey.

Hollow spar has been lately discovered, for the first time, in Scotland; it occurs in clayslate, in the vicinity of Balahulish.

Cyanite, which has hitherto only been found in Shetland, and at Boharm, in Banffshire, exists also in Cairnlia, one of the summits of Ben-y-Gloe, and in Glen-Tilt; it is embedded in quartz.

Staurolite has lately been, for the first time, found in the north of Scotland, in mica-slate; the crystals are perfect, and occur both simple and crossed; they are of a large size, but differ in composition from those of Brittany and Switzerland; their colour is dark lead-gray; and their fracture not unlike that of claystone.

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14. *St. George's Medical and Chemical School.*—The Courses will commence on Tuesday, October the 5th.

1.—On the Laws of the Animal Economy, and the Practice of Physic, with Pathological Demonstrations; at nine in the morning, by George Pearson, M.D. F.R.S., senior physician to St. George's Hospital, *&c. &c.*

2.—On Chemistry, in which Medical Jurisprudence will be introduced, by W. T. Brande, Sec. R. S., Professor of Chemistry at the Royal Institution, *&c.*

3.—On Therapeutics, with Materia Medica; at eight in the morning, by George Pearson, M.D. F.R.S., *&c.*

4.—Surgical Lectures, gratuitously, to the Pupils of St. George's Hospital, will be given by Sir Everard Home, Bart.

Proposals and Particulars may be had at No. 9, George-Street, Hanover-Square, and at St. George's Hospital.

Select List of New Publications during the last three Months.

Transactions of Public Societies.

Transactions of the Literary Society of Bombay, with engravings, Vol. II. 4to. 3*l.* 3*s.*

Medical Transactions, published by the College of Physicians in London, with coloured plates, Vol. VIII. 8vo. 12*s.* boards.

Transactions of the Horticultural Society of London, Vol. IV. Part I. 1*l.* 13*s.*

Agriculture.

A New System of Agriculture, without Lime or Dung, or Summer Fallows. By Major-general Beatson. 8vo. 9*s.*

Architecture and the Fine Arts.

The Heraldic Origin of Gothic Architecture; by R. Lascelles, Esq. of the Middle Temple, royal 8vo. 7*s.*

Chronological and Historical Illustrations of the Ancient Architecture of Great Britain. By Mr. Britton. No. 6.

The Architectural Antiquities of Normandy. By J. S. Cotman. Part II. royal folio. 3*l.* 3*s.*

Views of the Remains of Ancient Buildings in Rome and its Vicinity. With coloured plates. 7*l.* 7*s.*

Picturesque Views of the Architectural Antiquities of Northumberland, from original pictures, painted expressly for the work; by Wm. Dixon and Thos. M. Richardson. Part I. Elephant 4to. 12*s.* Atlas 4to. 18*s.*

Remains of a Roman Villa at Bignor in Sussex. By the late Samuel Lysons, Esq. Atlas folio. 34 plates. 12*l.* 12*s.*

Roman Costumes, drawn from Nature, by Pinelli, and on Stone. By C. Hullmandell. No. I. folio, 6*s.* Coloured plates. 12*s.*

Twenty-four Views of Italy, drawn from Nature, on Stone. By C. Hullmandell, folio. 1*l.* 10*s.*

Views, illustrating the Route of the Simplon. Drawn from Nature, by Major Cockburn, and on Stone, by C. Hullmandell. Part I. folio, 8*s.*

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Botany.

Hortus Suburbanus Londinensis: or, a Catalogue of Plants cultivated in the Neighbourhood of London. By R. Sweet, F.L.S. 8vo. 18*s*.

Natural History and Natural Philosophy.

The Natural History of British Quadrupeds, with Figures. By E. Donovan, F.L.S.A. 9*s*.

The Natural History of Ants; by P. Huber. Translated from the French; with Notes. By J. R. Johnson, M.D. 12mo. 9*s*.

A Compendium of the Ornithology of Great Britain; with a Reference to the Anatomy and Physiology of Birds. By John Atkinson. 8*s*.

A Select Cabinet of Natural History. By the late George Shaw.

Geography.

A New and Improved Map of India, from the latest Documents. By John Walker. On one large sheet, 16*s*. mounted, 1*l*. 1*s*.

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An Essay on Involution and Evolution; containing a New Method of ascertaining the Numerical Value of any Junction of an Unknown Quantity. By P. Nicholson. 8vo. 6*s*.

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A Treatise on the Inflammation of the Mucous Membrane of the Lungs. By Charles Hastings. 8vo. 10*s*. 6*d*.

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P 2

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THE

QUARTERLY JOURNAL,

January, 1821.

Art. I. *Observations on the Analysis of Mineral Waters. By* W. T. Brande, Sec. R.S. Prof. Chem. R.I., *&c.*

THE following observations relating to the analysis of mineral waters have been drawn up principally with a view to facilitate the progress of the student, in that very difficult department of analytical chemistry. I have endeavoured to simplify the details by pointing out the readiest methods of recognising and separating the substances which usually occur, and have, therefore omitted the enumeration of the more rare ingredients, or of those which are limited to particular places.

I have not adverted to the mode recommended by Dr. Murray, (*Edinburgh Philosophical Transactions,* VIII.) because I cannot readily admit the existence of incompatible salts to the extent which his principle requires; nor do I think that it materially facilitates the analysis in those cases which present peculiar difficulties to the plan of determining the ingredients by evaporation.

Sect. I. *Of the Tests and Apparatus required in the Examination and Analysis of Mineral Waters.*

Those who have not access to a regular laboratory will find it convenient to arrange the following tests and re-agents in the manner represented in the annexed drawing; (Plate II.) the larger phials should contain about six ounces by measure; the second size, three ounces; and the smallest, one ounce. Of these phials, the greater number should be simply stopped; and a few of them provided with an elongated stopper (22) dipping into the fluid which they contain.

The larger phials may contain the following re-agents :—

Pure sulphuric acid.
—— nitric acid.
—— muriatic acid.
Dilute sulphuric acid, 1 acid + 3 water.
—— nitric acid ditto.
—— muriatic acid ditto.
Solution of potassa.
—— soda.
—— ammonia.
—— carbonate of potassa.
—— carbonate of soda.
—— carbonate of ammonia.
—— oxalic acid.
—— oxalate of ammonia.
—— baryta.
—— acetate of baryta.
—— nitrate of baryta.
—— phosphate of soda.
—— sulphate of silver.
Alcohol.

The smaller phials may contain

Tincture of galls.
Solution of iodine in alcohol.
—— nitrate of silver.
—— ferro-prussiate of potassa.
—— muriate of lime.
—— hydro-sulphuret of ammonia.
—— hydriodate of potassa.
—— soap in alcohol.
Phosphorus.
Sulphate of lime.
Test-papers, turmeric, litmus, violet.
Black flux.
Nitrate of ammonia.

The tray should contain a few Florence flasks (1), Wedg wood and glass basins (2, 3), a platinum and a silver crucibl

(4, 5), a silver capsule (6), some funnels (7), test glasses (8), test tubes (9), and glass rods, filtering paper, a spirit (10), and an Argand Lamp (11), a retort (12), and receiver (13), a copper basin to serve as sand-bath (14), a blowpipe (15), a thermometer (16), a scale of equivalents (17), a dropping bottle (18), a few watch glasses (19), a support for holding glasses over a lamp (20), a small brass stand with rings (21), a tube with a bulb in the centre and a pointed extremity, for drawing up small portions of liquids (23), platinum pincers (24, 25): a small but good balance, with well-adjusted weights, is also requisite, accompanied by a phial and counterpoise for taking specific gravities; and lastly, a small mercurial trough. There should also be a plentiful supply of distilled water, a portion of which should be contained in a dropping bottle.

SECT. II. *Examination of Mineral Waters by Tests.*

1. The term *mineral water* is applied to those natural spring waters which contain so large a proportion of foreign matter as to render them unfit for common domestic use, and to confer upon them a sensible flavour, and specific action upon the animal frame. Their temperature is liable to considerable variation, and is sometimes their principal character, as is the case with the waters of Bath and Buxton; but they are generally so far impregnated with acid or saline bodies, as to derive from them their peculiarities, and in this respect may conveniently be arranged under the heads of *carbonated, sulphureous, saline,* and *chalybeate* waters. The mere taste of the water enables us to determine to which of these subdivisions, it probably belongs.

2. In examining a mineral water, it is of importance to ascertain its specific gravity, which gives us some insight into the proportion of its saline ingredients, its specific weight as compared with pure water, being of course augmented by its foreign contents. Mr. Kirwan *(Essay on Mineral Waters,* p. 145.) has given the following formula for calculating the proportion of saline substances in a water of known specific gravity: "subtract the specific gravity of pure water from that of the water examined, and multiply the remainder by 1.4. The product is

equal to the saline contents in a quantity of the water denoted by the number employed to indicate the specific gravity of distilled water. Thus suppose the specific gravity of the water = 1.079, and that of pure water = 1.000, then $79 \times 1.4 = 110.6$ = saline contents in 1000 of the mineral water."

This is a useful formula, but open to certain objections; and as it is often of considerable importance to acquire a just knowledge of the proportion of foreign bodies in water, it is advisable to conjoin the above method with the following.

3. Evaporate a given weight, say 1000 parts, to dryness, and expose the residue for twenty-four hours to a temperature not exceeding 300° upon a platinum capsule; weigh it, and the mean obtained from this and the former experiment, will give the proportion of dry saline ingredients within an error of two *per cent.* Thus suppose 1000 parts of the above-mentioned water give by evaporation 114.4 dry residue, then $110.6 + 114.4 = 225 \div 2 = 112{,}5$ = quantity of saline matter in a dry state (salts deprived of water of crystallization) existing in the mineral water under investigation.

4. Having by these preliminary operations ascertained the relative *quantity* of foreign matter in the water under examination, the *nature* of the substances present is next to be inquired into.

The substances which have been found in mineral waters are extremely numerous, but those which ordinarily occur, are the following:

Oxygen.
Nitrogen.
Carbonic acid.
Sulphuretted hydrogen.
Carbonate of lime.
Carbonate of magnesia.
Carbonate of iron.
Muriate of magnesia.
Sea salt.
Sulphate of magnesia.
Sulphate of soda.
Sulphate of lime.

a Oxygen and nitrogen exist in the greater number of spring waters in the proportions constituting atmospheric air; the proportion of nitrogen is, however, not unfrequently predominant. These gases give no peculiar flavour to the water.

b Carbonic acid renders mineral waters sparkling and effervescent: it is detected by occasioning a precipitate in aqueous solution of baryta, which dissolves with effervescence in dilute muriatic acid.

c The presence of sulphuretted hydrogen is known by its peculiar disagreeable smell; by the production of a black precipitate on dropping into the water a solution of nitrate of silver; and by the deposition of sulphur on adding a few drops of nitric acid.

d The carbonates are dissolved in the water by excess of carbonic acid, and consequently fall down upon its expulsion by boiling. Carbonate of lime and magnesia are deposited in the form of a white precipitate. Carbonate of iron occasions the separation of a rusty brown ferruginous powder, and the water is blackened by a few drops of tincture of galls.

e Mr. R. Phillips, in his analysis of Bath waters, has shown that the delicacy of galls, as a test for iron, is curiously affected by the presence of certain salts: if the iron be in the state of protoxide, its detection is facilitated by salts with a base of lime, and by alcalis; if in the state of peroxide, lime prevents the action of the test. This is well shown by dissolving a *very* minute portion of protosulphate of iron in a glass of distilled water, and adding a drop of tincture of galls, which occasions no immediate discoloration; but a drop of lime-water, or other alcali, instantly renders the presence of iron evident; so that the quantity of iron present in a water cannot be correctly judged of by the degree of precipitation occasioned in it by tincture of galls.

f Ferro-prussiate of potassa is also a good test to show minute quantities of iron in water, by the blue precipitate which it occasions; its action is aided by previously adding two or three drops of nitric acid to the water; but it is an equivocal test compared with galls.

g The presence of muriatic salts and of chlorides, is indicated by a white cloud on adding sulphate of silver.

h The sulphates, when present in water, afford a white precipitate on the addition of nitrate of baryta, which is insoluble in nitric acid.

i Lime is recognised by a white cloud on dropping oxalate of ammonia into the water. A portion of the precipitate collected upon leaf platinum, and heated before the blowpipe, may be burned into quicklime.

k Magnesia is rendered evident by adding carbonate of ammonia which throws down the lime, and subsequently pouring in phosphate of soda, which, when magnesia is present, carries a portion of it down in the form of a granular precipitate of ammoniaco-magnesian phosphate.

Such are the readiest means of recognising the presence of the various substances that commonly occur, by the action of re-agents or tests; and, having gained such general information, we next proceed to the analysis of the water, in order to ascertain the relative proportions of the gaseous and saline ingredients which it holds dissolved.

Section III. *Analysis of Mineral Waters.*

5. To ascertain the relative proportions of the gaseous contents of water with perfect accuracy, is a very difficult undertaking, and rarely necessary; the following method is sufficiently precise in all ordinary cases of analysis. Provide a Florence flask capable of holding rather more than a measured wine pint, which quantity of the water under examination is to be introduced into it, and a cork carefully fitted to its neck, through a perforation in which is inserted a glass-tube one-eighth inch diameter, rising perpendicularly about eighteen inches, and then bent so as to pass conveniently under the shelf of the mercurio-pneumatic apparatus. (Where a sufficiency of mercury cannot be procured, warm water may be substituted, if only carbonic acid be present, and it may be absorbed by transferring the jar containing it to a solution of potassa.) The flask should be placed over an argand lamp, and heat gradually applied till the water

fully boils. The gas evolved is to be collected in the usual way, in a graduated jar over quicksilver, and submitted to the following examination:—

6. Throw up a small quantity of solution of potassa, which, if carbonic acid be present, will absorb it, and the quantity will be shown by the diminution of bulk.

7. Introduce the remaining air, or a portion of it, into a small bent tube, containing a bit of phosphorus; heat it so as to kindle the phosphorus, and note the diminution of bulk when cold. It is proportional to the oxygen present, and, if equal to one-fifth of the whole bulk, the gas may be regarded as atmospheric air *.

8. If sulphuretted hydrogen be present it may be separated by strong alcoholic solution of iodine, which rapidly absorbs it, and scarcely takes up more than its own volume of carbonic acid gas. Chlorine, added to a mixture of sulphuretted hydrogen and carbonic acid, will also produce the absorption of the former if a little water be present; but it cannot be conveniently used over mercury.

9. During the ebullition it not unfrequently happens that a precipitation ensues, indicating that the substances thrown down were dissolved by carbonic acid; and in that case they should be separated upon a filter A, after which the remaining water may be evaporated to dryness in a glazed porcelain basin; the dry residue transferred to a silver capsule, and perfectly desiccated at a temperature not exceeding 500°. B.

The precipitate A may consist of carbonate of lime, of carbonate of magnesia, or of oxide of iron; or it may be a mixture of the three; dissolve it in dilute muriatic acid, and add oxalic acid which throws down oxalate of lime; separate this by filtration, and saturate the filtrated portion with carbonate of ammonia, which precipitates the peroxide of iron, and having removed this, evaporate the residuary mixture, and expose the

---

* In separating oxygen a solution of nitric oxide in protosulphate of iron may sometimes conveniently be employed, but it does not give so accurate a result as the action of phosphorus.

dry salt to a red heat in a small platinum capsule; the magnesia, if any were present, will remain; if not there will be no residue, for the oxalic acid and muriate of ammonia will be destroyed and volatilized.

100 parts of oxalate of lime indicate 77 of carbonate of lime.

100 parts of red oxide of iron indicate 90 of black oxide, or 143 of carbonate of iron.

When carbonic acid holds iron in solution, the metal is in the state of protoxide, and if air be excluded it requires long boiling to decompose it; for the same reason, if the water be exposed, under the exhausted receiver of the air-pump, it does not readily become brown, as is the case when it is exposed to air; a drop or two of nitric acid facilitates the deposition of the red oxide.

100 parts of pure magnesia are equivalent to 213 of carbonate of magnesia.

10. The dry residue B, is to be digested in six or eight parts of boiling alcohol, specific gravity 0.817, which will take up muriate of magnesia, and in some rare cases (where no sulphates are present) muriate of lime. Filter off the alcoholic solution, and wash the residue C with a little fresh alcohol, which add to the former, and evaporate to dryness D. The dry mass D, exposed for some time to a heat of 500°., is generally pure muriate of magnesia; if it contain muriate of lime, the latter earth may be separated by solution of oxalic acid, in the state of oxalate of lime.

I have found it, in some cases, convenient to convert the muriates of lime and magnesia into sulphates, by pouring upon them excess of sulphuric acid, evaporating to dryness, and heating the dry mass red hot. The sulphate of magnesia may then be almost completely separated from the sulphate of lime, by a small quantity of cold water; or a saturated solution of sulphate of lime may be used, which takes up the sulphate of magnesia, and, of course, leaves the sulphate of lime.

The alcohol will also take up a very minute portion of sea-salt, which, however, is too small to require estimation.

11. The residue C, insoluble in alcohol, may contain sea-salt,

sulphate of soda, sulphate of magnesia, and sulphate of lime; digest it in ten parts of boiling distilled water, which, when cold, will have taken up every thing but sulphate of lime, of which an inappretiable portion only will have been dissolved; separate the solution into two equal portions, *a* and *b*.

To *a* add nitrate of silver, and wash and dry the precipitate, which is chloride of silver, and of which 100 parts indicate 41 of sea-salt.

To *b* add acetate of baryta as long as it occasions a precipitate, which is sulphate of baryta, and which is to be separated, dried and weighed. 100 grains are equivalent to 60.5 of sulphate of soda, and to 51 of sulphate of magnesia.

In order to ascertain the quantity of magnesia present, and consequently the quantity of sulphuric acid belonging to it, evaporate the liquid filtered off the barytic precipitate E to dryness; it will contain sea-salt, acetate of soda, acetate of magnesia, and, probably, a portion of the added acetate of baryta; ignite the dry mass, and wash it to separate the sea-salt and soda; magnesia and carbonate of baryta will remain insoluble, upon which pour dilute sulphuric acid; digest, filter, and evaporate the clear liquor to dryness; it is sulphate of magnesia, equivalent of course to the original portion of the salt; deduct the sulphuric acid contained in it from the whole in the precipitate E, and the remainder will give the quantity united to the soda.

12. To estimate the quantity of sulphate of lime in the water, the residue of the evaporation of one pint may be washed with cold saturated solution of sulphate of lime, which will dissolve every thing but that sulphate, and which may thus be obtained and weighed; or, add oxalate of ammonia to a given quantity of the boiled and filtered water, collect the precipitate, and dry it at a heat of 500°. One hundred grains of this oxalate indicate 104 of dry sulphate of lime.

13. Such are the general components of mineral waters, and the means of ascertaining their relative quantities. Let us suppose the following results have been obtained, with a view to

illustrate the mode of drawing up the analysis. By the process 5, twelve cubical inches of gas have been expelled during the ebullition of a pint of water. The exposure to solution of potassa has occasioned a diminution of eleven cubical inches, which, it having been previously ascertained that no sulphuretted hydrogen was present, may be considered as carbonic acid. The remaining gas thrown up into a tube, containing a portion of phosphorus, and heated, suffers scarcely any diminution, and the phosphorus does not burn: hence it may be regarded as nitrogen. The gaseous contents, therefore, of the water under examination are, in the wine pint—

Carbonic acid	11 cubic inches.
Nitrogen	1 ditto *.

If sulphuretted hydrogen be present, it is best to have recourse to a separate operation to estimate its quantity: for this purpose collect the gas as before, and throw up into it a small quantity of alcoholic solution of iodine. The absorption denotes the quantity of the gas. (8).

14. The next step of the operation relates to the examination of the precipitate, which has been deposited during ebullition, 9. A. Let us suppose the weight of oxalate of lime to be 3 grains, of oxide of iron 1.5 grain, and of magnesia 1 grain, then the above data give

	Grains.
Carbonate of lime	2.2
Carbonate of iron	2.4
Carbonate of magnesia	2.1

15. The alcoholic solution (10) may be diluted and tested by oxalic acid for lime; if absent, evaporate to dryness as directed. Let us suppose the residue to be

Muriate of magnesia ...... 5 grains.

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* Of this nitrogen, a small portion will probably have been derived from the air in the tube connecting the flask with, the pneumatic apparatus; a little practice soon enables the operator to ascertain when it has been expelled; or it may be received entire, and afterwards deducted from the whole produce.

If the quantity of muriate of magnesia be considerable, greater accuracy is ensured by converting it into sulphate, which is done by placing it in a capsule of platinum, pouring upon it sulphuric acid, evaporating to dryness, and heating the dry mass to dull redness. One hundred grains of this dry sulphate of magnesia indicate 94 of muriate of magnesia; hence the water under examination would have given 5.35 grains = 5 grains of muriate.

If the alcoholic solution contain muriate of lime, that earth must be previously separated by oxalic acid; and 100 parts of oxalate of lime are equivalent to 85 of dry muriate of lime.

16. The aqueous solution of the residue (C 11) being divided into two portions, let us suppose the portion (*a* 11) to afford 8.5 of chloride of silver, which indicates of sea salt 3.5 grains = 7 grains in the pint.

17. Let us assume, that the precipitate of sulphate of baryta (*b* 11.) weighs 15 grains, indicating of

Sulphuric acid ...... 5.1 grains.

The process directed in 11 furnishes of

Sulphate of magnesia ...... 3.75 grains,

which contain 2.5 grains of sulphuric acid, and which deducted from 5.1 grains leave 2.6 grains, which are adequate to the formation of

Sulphate of soda ...... 4.65 grains.

So that the pint (the water having been divided into two equal portions) would contain of

Sulphate of magnesia $3.75 \times 2 = 7.5$ grains.
Sulphate of soda .... $4.65 \times 2 = 9.3$ grains.

18. The addition of oxalate of ammonia, or oxalic acid, to a pint of the boiled water (12) furnishes a precipitate of 4.7 grains of oxalate of lime, indicating of

Sulphate of lime ...... 5 grains.

19. To give a general view, therefore, of the components of the mineral water which has thus been examined, we should place them as follows:—

One wine pint contains

	Cubic Inches.
Carbonic acid	11
Nitrogen	1
Gaseous contents	12

	Grains.
Carbonate of lime	2.20
Carbonate of iron	2.40
Carbonate of magnesia	2.10
Muriate of magnesia	5.00
Sea salt	7.00
Sulphate of magnesia	7.50
Sulphate of soda	9.30
Sulphate of lime	5.
Aggregate weight of solid contents	40.50

20. Besides the substances now enumerated, and which may be considered as the most frequently occurring ingredients in mineral waters, there are others occasionally present, of which the following is an enumeration, with the best methods of detecting them.

a Carbonate of soda is known to exist in water, when after having been boiled down to half its bulk, and, if necessary, filtered, it reddens tumeric paper, and restores the blue of litmus reddened by vinegar; it also affords an effervescent precipitate with nitrate of baryta, soluble in dilute nitric acid. This carbonate is incompatible with the soluble salts of lime.

Muriate of lime may also be used to detect the alcaline carbonates, with which it affords a precipitate of carbonate of lime. Carbonate of soda is distinguished from that of potassa, by the latter affording a precipitate in neutral muriate of platinum, which the former does not. Carbonate of ammonia is obviously discoverable by its smell, when acted on by caustic fixed alcali, or lime.

b Silica is detected by evaporating the water to dryness, and boiling the residue in dilute muriatic acid. The silica, if present, remains as a white powder not altered by a red heat, but instantly fusing with a particle of carbonate of soda.

c Boracic acid and borax have been found in certain lakes in India, and in some parts of Italy. To detect boracic acid, evaporate to one-eighth the original bulk of the water, and add carbonate of soda as long as it occasions any precipitate; boil and filter. The filtered liquor will contain borate of soda, with some other salts of the same basis; evaporate to dryness in a platinum crucible, and digest the residue in three or four parts of sulphuric acid, diluted with its bulk of water. If boracic acid be present, it will separate in micaceous crystals.

d Alumina has been found in a few mineral waters in the state of a sulphate. It may be separated by the following process: Evaporate to dryness, digest in alcohol, and re-dissolve the residue in eight parts of water; filter and add oxalic acid, which throws down lime, and which being separated, leaves magnesia and alumina in solution. Carbonate of ammonia throws down the alumina and leaves the magnesia.

Pure ammonia throws down both alumina and magnesia. These earths may be separated by solution of potassa, which dissolves the former but not the latter.

e Manganese is sometimes found in water, but only in very small proportion, so as not to amount to more than a trace. Dr. Scudamore found a trace of manganese in the waters of Tunbridge Wells, and it has never been discovered in larger proportion.

f It has been said that certain nitrates are occasionally present in water, but such solutions can scarcely be called *mineral waters*. If nitrate of lime be present, it will be taken up from the residue of evaporation by alcohol, and may be decomposed by carbonate of potassa, so as to afford carbonate of lime and crystals of nitre.

g It sometimes happens that water contains lead, which may be detected by evaporation to one-eighth its bulk, adding a few drops of nitric acid, and then hydriodate of potassa, which gives a yellow insoluble precipitate; and hydro-sulphuret of ammonia, which forms a deep brown or black cloud. These precipitates may be reduced by heating them before the blowpipe upon charcoal, mixed with a little black flux.

h If vegetable or animal matter be contained in water, it gives it a brown colour, especially when evaporated, It may be destroyed in the dry residue by igniting it with a small addition of nitrate of ammonia.

The following analyses of mineral waters may be advantageously consulted by the student, as containing a variety of useful details, which are necessarily omitted in the above observations.

1. *Analysis of the Hot Springs at Bath*, by Richard Phillips, Esq.
2. *Analysis of the Brighton Chalybeate*, by Dr. Marcet.
3. *Analysis of the Tunbridge Wells Waters*, by Dr. Scudamore.
4. The sixth chapter of Mr. Children's *Translation of* Thenard's *Essay on Chemical Analysis.*

Art. II. *On some Properties of the Catenarian Curve with reference to Bridges by Suspension. In a Letter to the* Editor *from* Davies Gilbert, Esq. F.R.S. *and* M.P.

Dear Sir,

Now that the properties of the Catenarian curve have acquired real importance from the construction of bridges by suspension, I flatter myself, that the following investigation will not be considered as wholly undeserving of attention :—

It is needless to remark, that almost every general principle of mechanics requires to be modified in its reduction to practice. Thus inertia and friction are omitted in the abstract theory of machines. The difference of form between the bridge and its suspending chain, and still more, perhaps, the weight of the links or bars connecting them together, must sensibly alter the mathematical form of the curve : yet, from the catenary alone, can the real principle for constructing these bridges be derived ; and they will probably be found not more remote from practical cases than other general principles, and equally capable of receiving all necessary corrections.

The elements of the Catenarian curve are given in most introductory treatises on fluxions ; but to avoid the necessity of referring to some particular work, they may be stated in a few lines.

Let $a =$ a constant force, estimated in length of the chain, which acts horizontally on A, the apex of the curve:

$z =$ the length of chain or periphery of the curve, between its apex A, and the point of section, by any ordinate EP.:

$y =$ the ordinate:

$x =$ the absciss.

Now the curve being sustained in equilibrio by these forces,

By the weight of the chain acting perpendicularly downwards;

By the force at A acting horizontally; and

By the suspension acting in the direction of the curve at P.

These forces must be represented in magnitude and direction, by the incremental triangle P r p—therefore

$\dot{x} : \dot{y} :: z : a$, consequently $\dot{x}^2 : \dot{y}^2 :: z^2 : a^2$

$$\dot{x}^2+\dot{y}^2 : \dot{x}^2 :: a^2+z^2 : z^2$$

But $\dot{x}^2+\dot{y}^2 = \dot{z}^2$ in all curves; therefore

$$\dot{z}^2 : \dot{x}^2 :: a^2+z^2 : z^2 \qquad \text{And } \dot{x} = \frac{z\dot{z}}{\sqrt{a^2+z^2}}$$

Equation A
$$\begin{cases} \text{N}^\circ\ 1. & x = \sqrt{a^2+z^2}-a \\ \text{N}^\circ\ 2. & z = \sqrt{2ax+x^2} \\ \text{N}^\circ\ 3. & a = \dfrac{z^2-x^2}{2x} \end{cases}$$

Again,

$\dot{x} : \dot{y} :: z : a$, consequently $a\dot{x} = z\dot{y} \therefore \dot{y} = \dfrac{a\dot{x}}{z}$

substituting from Eq. A N° 2

$$\dot{y} = \frac{a\dot{x}}{\sqrt{2ax+x^2}} \qquad \text{And}$$

$$y = a \times h\ L.\ \frac{a+x+\sqrt{2ax+x^2}}{a} = a \times h\ L.\ \frac{a+x+z}{a}$$

or by substituting its value for a from Equation A N° 1, and dividing by $z+x$.

Equation B ......$y = a \times h\ L.\ \frac{z+a}{z-a}$

Thus far see Dr. Hutton. Vince. Mac Laurin, &c.

Now, it is obvious, as there are not any arbitrary quantities, that all catenaries must agree in specie, differing in magnitude alone; and since two Equations only can be deduced from the general properties of the curve, and there are four unknown quantities, no one of them can be exhibited in terms of any other, unless some new Equation is introduced; as in the case of a maximum or minimum, or of an assumed relation in magnitude between either two of the four quantities.

The maximum, with reference to the subject of this inquiry, will evidently take place, when the force of suspension at P acquires a rate of proportional increase equal to that of y, or if b represent this force, when $\frac{\dot{b}}{b} = \frac{\dot{y}}{y}$ But $b^2 = a^2+z^2 =$ $a^2+2ax+x^2$ .... Eq. A N° 2.

$$b = a+x \because \dot{b} = \dot{x} \text{ And } \frac{\dot{x}}{a+x} = \frac{\dot{y}}{y} \text{ consequently}$$

$$\dot{x} : \dot{y} :: a+x : y \text{ But}$$

$$\dot{x} : \dot{y} :: z \quad : a \text{ therefore}$$

$$y = a \times \frac{a+x}{z} \text{ But } y = a \times h\ L.\ \frac{z+a}{z-a} \text{ consequently}$$

$$\frac{a+x}{z} = h\ L.\ \frac{z+x}{z-x} \text{ Or substituting for } z \text{ from Equa. A N° 2}$$

$$\frac{a+x}{\sqrt{2ax+x^2}} = h\ L.\ \frac{\sqrt{2ax+x^2}+x}{\sqrt{2ax+x^2}-x} \text{ and therefore}$$

$$\sqrt{2ax+x^2} \times h\ L.\ \frac{\sqrt{2ax+x^2}+x}{\sqrt{2ax+x^2}-x} - x - a = 0$$

The expression may now be simplified by assuming $a = 1$

Then $\sqrt{2x+x^2} \times h\ L.\ \frac{\sqrt{2x+x^2}+x}{\sqrt{2x+x^2}-x} - x - 1 = 0$ From

whence, by approximations, it will be found, that
$x = 0.81$ very nearly

a and x being now given, z will be found from Equation A, and y from Equation B.

The four quantities and b will therefore stand

$x = 0.81$ .... Log 9.9084850
$a = 1$
$y = 1.1995$ .... Log 0.0790003
$z = 1.5087$ .... Log 0.1786029
$b = 1.81$ .... Log 0.2576786

Angle of suspension 56°.28′, as deduced through the incremental triangle from a z and b.

By applying these deductions to a span of 560 feet, equal to that of the proposed bridge across the Menai Strait,

$a = 233.4$ Feet
$x = 189.1$ Feet
$y = 280$ Feet
$z = 352.2$ Feet
$b = 422.5$ Feet

Where all the quantities must be considered as feet of the suspending chains, augmented proportionally in weight by the horizontal bridge, and by the media of suspension

It is obvious, from these values of x and y, that the curvature is never likely in any practical instance to meet the theoretical maximum.

When x is small in comparison of z, a much easier method may be used than that by approximation, and sufficiently near to the truth.

y has been found equal to $a \times h\ L. \dfrac{z+x}{z-x}$ but when x is small in comparison of z, the h $L.$ of $\dfrac{z+x}{z-x}$ will not differ much from $\dfrac{2x}{z}$ then

$$y = 2a\frac{x}{z} \quad \text{but } a = \frac{z^2-x^2}{2x} \text{Equ. A N° 3}$$

$$y = 2 \times \frac{z^2-x^2}{2x} \times \frac{x}{z} \because y = \frac{z^2-x^2}{z} \because yz = z^2 - x^2 \text{ or}$$

$z^2 - yz = x^2$ By completing the square, &c.

$z = \frac{1}{2}y + \sqrt{\frac{1}{4}y^2 + x^2}$ By using this value for z^2 in

$$a = \frac{z^2 - x^2}{2x} \cdots\cdots\cdots \text{Equ. A N}^\circ 3$$

$$a = \frac{zy}{2x} \quad \text{And since } b^2 = a^2 + z^2$$

$$b = z \times \frac{\sqrt{4x^2 + y^2}}{2x}$$

Now assign to x and y their respective values 25 and 280 feet, as they are given for the Menai Bridge; the quantities will then be found,

$a = 1580$ Feet
$x = 25$
$y = 280$
$z = 282.2$
$b = 1605$ or about $5.7 \times$ by $\frac{1}{2}$ the weight of the chains. bridge, *&c.* or three times their weight nearly.

The angle of suspension 10° 8′.

If x be now doubled, or x and y are taken in the proportion of 50 to 280, the quantities will be,

$a = 808$
$x = 50$
$y = 280$
$z = 288.0$
$b = 858$

The angle of suspension 19° 39′.

In this case the values of a and b, representing the strains at the apex of the curve and at the point of suspension, are very nearly one-half of the former. And from the equations $a = \frac{zy}{2x}$ and $b = z \times \frac{\sqrt{4x^2 + y^2}}{2x}$ it appears, that a, and consequently b, must increase or diminish in the reciprocal proportion to x, as y is supposed constant, and z is found to differ, when x is 25 or 50 by no more than a few feet. If these relations of x and y are taken as the bases of calculation by the strict forms, the results will remain substantially the same, and this general conclusion may safely be deduced from the whole.

That with reference to the strength and safety of suspended bridges, in all cases likely to occur in practice, their points of attachment cannot be too lofty, nor consequently the curvature of the chains too great.

The greatest span of a catenary arch, capable of being formed by iron or steel, on the supposition of these metals supporting the utmost degree of tension theoretically assigned to them, may be estimated in the following manner:

If the tenacity of iron be taken at 50,000 pounds for a square inch, and the specific gravity of iron at 7.8, the modulus of tenacity will be 14814 feet. Put this equal to b. in the expression for a maximum, then y will be found $=$ 9817 feet, and consequently the whole span or $2y =$ 19634 feet, about 3.7 miles, but then $x =$ 6629 feet, or 1.25 miles.

Steel, being supposed to have three times the tenacity of iron, will extend all their movements threefold.

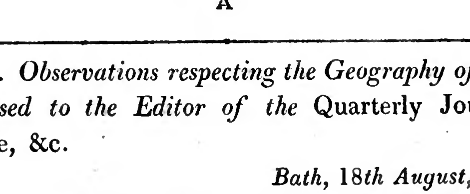
When x and y are equal to each other, they will be 1.16 very nearly, a being unity, and $z = 1.914$.

If $a =$ unity, and x, y, and z are taken indefinitely great,

$$z = 1 + x$$

$$y = h\,L.\ \overline{1+2x}$$

$$b = z$$



Art. III. *Observations respecting the Geography of Plants. Addressed to the Editor of the* Quarterly Journal of Science, &c.

Sir, *Bath*, 18*th August*, 1820.

The accompanying Letter, in Schrader's *Botanical Journal**, appears to me to contain much valuable information upon the new and interesting branch of science to which it relates,

* Jahrbücher der Gewächskunde. Herausgegeben von K. Sprengel, A. H. Schrader und H. F. Link. Ersten Bandes, erstes Heft. 1818.

Two papers upon the subject are, indeed, to be found in the *Amœnitates Academicæ*, entitled *Stationes Plantarum* and *Coloniæ Plantarum.* These may, perhaps, be attributed to Linnæus himself. They are merely slight sketches, which were never filled up. The inquiry appears to have slept afterwards, and it is only very lately that it has been revived by Humboldt, Brown, Wahlenberg, Stromeyer, Ramond, Decandolle, Engelhardt and Parot, *&c. &c.* The principal writer, however, upon this subject, is M. Humboldt, the celebrated traveller. The varied and extensive information of this philosopher is well known, and justly appreciated; but the extreme vivacity and brilliancy of his imagination, and the propensity to generalize, which he manifests upon all occasions, are too conspicuous, not to excite our doubts respecting the accuracy of some of his conclusions. How far we are justified in this, the following critical and illustrative remarks will shew.

I am, Sir, yours, *&c. &c.*,

J. F. D.

Observations upon two Works of A. de Humboldt, concerning the Geography of Plants. In a Letter to A. H. Schrader.

At a period, when particular observations upon the distribution of plants have become so abundant, that the geography of plants, from an insignificant number of scattered remarks, has raised itself to an independent science, every new contribution certainly merits the greatest attention. Permit me, therefore, to communicate to you a few remarks upon two treatises which have lately appeared upon this subject, by M. de Humboldt. Should the interesting nature of the subject, and the desire which I have frankly to lay before you my opinion thereon, give too great an extent to this letter, I earnestly crave your indulgence.

The treatises alluded to are the following:

1. *Alexander de Humboldt de distributione geographicâ Plantarum secundum cæli temperiem et altitudinem montium.* As the Prolegomena to the work: *Nova genera et species plantarum* quas in peregrinatione collegerunt, descripserunt et partim adumbraverunt Bonpland et Humboldt; e schedis Bonplandi in ordinem digessit Kunth. Tomus 1. Lutetiæ, 1815.

2. *Ejusdem sur les lois que l'on observe dans la distribution des formes Végétales.* Paris, 1816.

Read in the Institute of France, 29th January 1816.

In the treatise which I have marked No. 1., the Author considers chiefly the following objects:—1. The whole number of hitherto known plants, and their distribution in the different parts of the world. 2. The distribution, in regard to climate, of some of the most important families. 3. The distinction between the social and solitary occurrence of plants. 4. Whether the same plants are found in both great continents, and to what extent. 5. The comparison of temperature in the old and new worlds in different latitudes. 6. The influence of altitude upon vegetation in the different zones; and lastly (7), he gives us an essay on the determination of the climate that is best adapted to any of the most important cultivated plants; and in the work itself, to which No. 1. forms the introduction, the families are generally followed by a geographical view of the same. The treatise which I have marked No. 2. cannot properly be considered as any thing more than an abridgment of No. 1. But, before I enter upon the proposed examination, I wish to offer a few observations upon what the author says, in a note p. xii., upon the science of the geography of plants.

If he, by the following: "*Geographia plantarum vincula et cognationem tradit, quibus omnia vegetabilia inter se connexa sint, terræ tractus quos teneant, in aërem atmosphæricum quæ sit eorum vis ostendit, saxa atque rupes quibus potissimum algarum primordiis radicibusque destruantur docet, et quo pacto in telluris superficie humus nascatur, commemorat* *," intends to give a definition of the geography of plants, one cannot by any means approve of this view of the subject; because, being merely an enumeration of the chief points which constitute the science, no advantage is gained by it. The examination of the natural affinities between plants, or, in other words, the natural ar-

* This is taken literally from his earlier work, entitled *Specimen Floræ Fribergensis.* Berolini, 1793. p. ix. Note.

rangement of plants, belongs to the philosophy of botany; and cannot be treated of in the geography of plants, unless a most arbitrary extension be given to the latter phrase, entirely at variance with the meaning of the words, and every idea hitherto associated with them. So little, likewise, can the influence of vegetables upon atmospheric air be an object of this science, that it, on the contrary, belongs to the province of physics; or, if it must be treated of in the science of botany, it is certainly a part of the physiology of vegetables. The inquiry also respecting those plants to which the disintegration of different species of rocks is owing, belongs principally to mineralogy. The same may be remarked of the question as to what plants are chiefly concerned in the production of vegetable mould. Of the five points, then, here given by the author, the second only belongs to the geography of plants. But this does not include every thing that belongs to the science. That the author should give such a definition of the geography of plants, in the year 1793, was not very blameable considering the state of the science at that period; but that he should at this time repeat it, when Professor Stromeyer* has so fully and satisfactorily established the objects of this branch of science, and when so much has been done in it by that gentleman and others, is so much the more surprising, as there is a striking difference between his own *Essay* and the present Treatise, in this respect†.

On the other hand, the distinction which the author has made between the GEOGRAPHY and the HISTORY OF PLANTS, merits entire commendation; and is so natural, that one cannot but justly wonder why it has not been retained by himself, and by another writer subsequent to him. M. Humboldt, in his *Essay*, includes both sciences under the title GEOGRAPHY OF PLANTS; but the subjects enumerated at p. xiv. belong to the geography, and those at p. xix.—xxii., to the history, of plants. Wildenow‡

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* Commentatio inauguralis sistens historiæ vegetabilium geographicæ specimen.—Gottingæ, 1800.

† Essai sur la Géographie des Plantes et tableau physique des régions équatoriales.—Another preceding work of the author. T.

‡ Grundriss der Kraüterkunde. 7. Abtheilung.

comprehends both under the denomination HISTORY OF PLANTS. M. Stromeyer denominates both, GEOGRAPHICAL HISTORY OF PLANTS, by which the confusion is not obviated. However, he has himself felt the necessity of a division, for the objects enumerated at p. xiv., under No. 1. 2. and 3., belong, the first, to the geography, and the two last, to the history, of plants. This perception, also, occasioned him to divide his arrangement into two principal sections.

In my opinion, the GEOGRAPHY OF PLANTS, is that science which teaches us to know the APPEARANCE, DISSEMINATION, AND DISTRIBUTION OF PLANTS, AS THESE EXIST AT PRESENT WITH A DUE CONSIDERATION OF OTHER MATTERS CONNECTED WITH THEM. It considers the different *habitats* of plants, and the distinction between those kinds which are social and those which are solitary, as well as between such as are plentiful and such as are rare; which is perhaps sufficiently expressed by the word (vorkommen) occurrence. It determines the extent of districts over which the plants are spread; and the laws according to which not merely the whole vegetable world, but likewise particular families and genera, are distributed in respect to geographical longitude and latitude, altitude, &c. It borrows from physics and physiology the laws, according to which external circumstances, as soil, temperature, moisture, *&c.*, act upon vegetables, for the purpose of comparison with those by which the geographical distribution, *&c.*, are governed. We may also compose an ŒCONOMICAL GEOGRAPHY OF PLANTS, founded on the results of scientific researches in civil occupations, particularly in agriculture, gardening, and forest-culture. (Forstwesen.)

The HISTORY OF PLANTS, on the other hand, teaches us THE LAWS, THE VARIETIES AND THE DECAY OF THEIR ORGANIZATION. This science, also, resolves the questions, When, where, and how were vegetables first produced? To what extent are we justified in admitting the transportation of plants? Have old species disappeared, and new ones been produced? Is it possible that one species, through the influence of external causes, or through hybrid generation, can be converted into another? *&c. &c.*

This distinction appears to me the more natural and proper, because the geography of plants is founded wholly upon observation; whereas a part of the history of plants rests upon hypothesis. We may then certainly regard, as separate branches of science, the geography, the geognosy, and the œconomical history, of plants.

1. Number of known Plants, and their distribution in the different parts of the World. (p. vii.—xi.)

The author mentions 38,000 as the full number of* phanerogamous plants known in catalogues and herbaria.

It does not appear to me, by any means proper to refer to herbaria in calculations of this sort; since no person can have the opportunity of seeing every collection of plants, and consequently such calculation cannot be accurate. This actually appears to be the case with the reckoning of the author; of the supposed 13,000 plants of South America, he takes 4,500, or if we include those discovered by himself, 7,500 as the number known in catalogues (Schriften), the remaining 8,500 or 5,500, are then to be met with, only in herbaria. To the torrid zone of Asia, he assigns, on the contrary, only 4,500; but since quite as many belonging to this district are already to be found in catalogues, the author scarcely appears to have brought those which are in herbaria, into calculation. But he has enjoyed opportunities of seeing plants collected in this portion of the globe, as well as in America, and ought therefore, to have allotted a much larger number to the torrid zone of Asia. If we were to regard only those plants, which have been made known through catalogues, the highest number we could admit, would be 30,000, for Persoon's *Synopsis* contains only 21,000; but take into consideration all the herbaria, and the number 38,000 is certainly too small.

But in my opinion, much more weighty objections may yet be

* Plants possessing visible stamens and pistils, or visible organs of fructification.—T.

made to the author's calculation. He distributes at p. xi., his 38,000 phanerogamous plants after the following manner:

Europe	7,000
Asia, temperate zone	1,500
Asia, torrid zone	4,500
Africa,	3,000
The two temperate zones of America	4,000
The torrid zone of America	13,000
New Holland and the Islands of the South Sea	5,000
	38,000

Since it is only through the addition of these numbers, that the sum of 38,000 is obtained, a question arises, where the author has placed those plants which are common to several parts of the world; especially a great number which are common to Europe and the northern part of Asia, and also to Europe and the northern part of Africa. From a more exact examination it would appear that only the most common are comprised in the number of European plants; whence it arises, that the temperate zone of Asia has no more than 1,500 plants, although BRIBUSTEIN'S *Flora Taurico Caucasica*, for a small part only of the same, enumerates 2,000 plants; and Africa receives only 3,000, notwithstanding the Flora of the Cape, contains nearly as many*, and that of Algiers, according to the author's estimate, p. x., contains 1,600. It is in any person's power easily to demonstrate the defects of this kind of calculation.

The author asserts, p. ix., that South America possesses only one quarter of the plants belonging to the torrid zone; without, however, adducing the evidence upon which the assertion is founded, for he does not actually prove that South America comprises about one quarter of the area of the whole torrid Zone.

2. GEOGRAPHICAL DISTRIBUTION OF THE FAMILIES OF PLANTS. p. xii.—xx.

The author commences with the remark, that writers upon the

* *Thunberg's Prodomus Floræ Capensis* contains about 2,600 plants.

geography of plants, have hitherto neglected the geographical distribution of families. If he hereby means only the general distribution, upon the whole surface of the earth, we must agree with him; but, concerning the relative proportion of the families of plants in particular countries, Dr. Wahlenberg, in his three principal works*, has presented us with much valuable information.

According to my conviction, there are too few materials at present, to enable us to establish the laws of this distribution with accuracy, certainty and perfection. Since, however, the subject is of so great interest, every attempt to fix them even provisionally, certainly deserves the warmest support; but herein we must proceed with the greatest attention and caution. I propose to go through the chief points, which in my opinion should be taken into consideration, and, with the permission of M. Humboldt, notice at the same time, how far they have been observed by him.

In the present state of the science there are two methods, by which the laws of distribution for different geographical latitudes, or to speak more precisely, different climates, may be investigated. We should either divide the surface of the earth into certain zones, ascertain which of the known plants are found in each zone, and then compare the different zones with each other; or we should first take some Floras of countries in different climates, compare the plants which are found in those countries, and draw conclusions from these with respect to the distribution of plants in general. Of these two methods the Author has chosen the last; we do indeed find inscribed upon the heads of the table (No. 1., p. xviii.) EQUATORIAL ZONE, TEMPERATE ZONE, and POLAR ZONE, but in the calculations prefixed to this table, he remarks that the computed distribution of the equatorial plants is derived only from those found by Humboldt and Bonplandt†, and the distribution in the polar

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* Flora Lapponica. Tentamen de climate et vegetatione Helvetiæ Septentrionalis.—Flora Carpathorum.

† In fact, as we shall see presently, only from a part of the same.

zone only according to the proportion of the families of plants in Lapland. In respect to the temperate zone, it cannot clearly be shewn, how the author has produced the numbers given for the same. As he had before exhibited the families in Germany, France and North America, we could wish that the numbers were the mean of the proportions of these countries; but as he gives in the superscription to the corresponding head of the table a mean temperature of 10—14°, this cannot be the case; for to the northern part of Germany he ascribes a mean temperature of only 8°. 5, to the southern part of France 16°. 7, and in North America, according to him, the northern and southern parts present a mean temperature of 18. (p. x.) It were to be wished that the author had given the particulars of his calculation here; for no one can suppose that the numbers were taken at hazard. In any case the proportion is not estimated from all the plants of a temperate zone.

But this method has two very obvious defects. The first is this: the countries in question not being contiguous to each other, we remain wholly uninformed, as to the proportions of plants in the intermediate countries. I suspect that this circumstance has occasioned the singular and contradictory results in respect to the Ferns. In the treatise, No. 2, the author says that the ferns as well as the Glumaceæ, Amentaceæ, and some other families, increase from the poles toward the equator. But as this conclusion stands in opposition to what has hitherto been admitted in respect to the geographical distribution of these plants, it is surprising that the author does not give in the table the proportion of these families to the whole number of plants found in the equatorial zone, and not only the proportion in the temperate and frigid zones, which of itself is not sufficient to verify his conclusion. In the treatise, No. 1. the ferns are not placed by themselves, the number of which decreases from the equator; but the table is as in No. 2. He remarks, page 33, that the ferns in proportion to the whole number of phanerogamous plants decrease from Lapland to Germany; but he does not know whether they increase towards the equator, because if the proportion be actually smaller in hot climates than in Germany, for example as 1 : 50 in respect to the whole number, then the torrid zone of America must have 23,000, and Jamaica 5,000 known phanerogamous plants. It appears, however, that the author ascribes to South America 460 ferns; but we do not perceive that he has made any use of this number, and he trusts less to his mode of calculation here than in the other families. That the number of the plants of this family, however, increases towards the equator, the author might easily have inferred from his own *data;* for he asserts that among the 1,000 ferns, which are known*, 760, that is $\frac{3}{4}$, are found within the tropics; but that of the whole number of phanerogamous plants which are known, more than $\frac{3}{4}$ are within the tropics, is at variance with his calculation p. xi., according to which at least 16,000 out of 38,000 grow without the tropics; for Jamaica he gives, after Schwartz, 764 phanerogamous plants and 103 ferns, which makes the proportion 1 : 8. He also says, p. 33, that the numbers of known *Filices* in Lapland, Germany and Jamaica, stand in the following relation, 19 : 40 : 103, and thence deduces, p. 33 as well as p. xiii., the proportions of this family in the three zones, 1, 2, 5†, much higher than those of the known phanerogamous plants.

Whilst then the ferns actually appear to have their true climate in tropical regions, (the greatest number being found in the West Indian Islands, the Isles of Bourbon and of France); it is very possible that, if a comparison were made between two countries, one lying near the equator, and the other in the temperate zone, the result would be, that the latter would be found to possess a smaller number of ferns than the former. But that the ferns should increase, not only from the temperate zone towards the tropics, but also towards the pole, as the author attempts to shew through a comparison between Germany and Lapland, would doubtless be a very remarkable phænomenon, for these climates, entirely opposite to each other, would have similar effects on vegetation. But we shall see presently, that

* In fact, this number is too small, for Willdenow has already 1,000; and more lately, particularly by M. M. Brown and Desvaux, many others have been made known.

† Besides, this calculation cannot be admitted. The numbers should be only 1 : 4 : 4.

this likewise proceeds from an erroneous method of calculation.

The second chief defect of the Humboldtian method is this, that in making use of particular Floras we consider only the plants of certain degrees of geographical longitude, and not the whole zone; or, as the author expresses it, all those which appear within the same *limites isothermi*. The author, indeed, supposes that the most exact uniformity is always found within these limits, (No. 1, p. xviii; No. 2, p. 5), so that by means of some families, the whole, and the number of plants belonging to the other families in any given district may be computed; but his own numbers entirely contradict this opinion. I will merely offer a few remarks upon the following variations between the Floras of Germany, France, and North America, as given by the author.

	Germany.	France.	North America.
Cyperoideæ	$\frac{1}{18}$	$\frac{1}{27}$	$\frac{1}{40}$
Junceæ	$\frac{1}{94}$	$\frac{1}{86}$	$\frac{1}{152}$
Labiatæ	$\frac{1}{20}$	$\frac{1}{24}$	$\frac{1}{40}$
Ericinæ et Rhododendra	$\frac{1}{90}$	$\frac{1}{125}$	$\frac{1}{36}$
Compositæ	$\frac{1}{8}$	$\frac{1}{7}$	$\frac{1}{6}$
Umbelliferæ	$\frac{1}{22}$	$\frac{1}{34}$	$\frac{1}{57}$
Cruciferæ	$\frac{1}{18}$	$\frac{2}{19}$	$\frac{1}{62}$
Caryophylleæ....................	$\frac{1}{27}$	$\frac{1}{22}$	$\frac{1}{72}$
Amentaceæ......................	$\frac{1}{39}$	$\frac{1}{32}$	$\frac{2}{25}$

The endeavours to explain this striking difference by the circumstance, that North America extends itself further towards the north, as well as towards the south, than the two cited European countries, and therefore its Flora has a character partly polar and partly tropical. But this explanation is not sufficient, for, in the first place, it appears that these two particulars should, to some extent at least, have reciprocally this effect, *viz.*, that North America should possess a greater number both of polar and of tropical plants;

and, secondly, a similar anomaly is observed on comparing the European Flora with the Flora of a district in North America, which has the same climate; for example, with the Flora of Philadelphia. According to William Bartram's *Prodromus Floræ Philadelphicæ*, the proportions are nearly as follow, the cultivated plants being omitted :—*Cruciferæ* $\frac{1}{60}$, *Umbelliferæ* $\frac{1}{39}$, *Caryophylleæ* $\frac{1}{42}$. Besides, it might be shewn that the geographical longitude occasions a marked difference. Thus the author himself remarks, p. 72, that $\frac{7}{8}$ of the known *Piperaceæ* are found in America. Moreover it is evident, from the immense number of species of *Erica* which grow at the Cape of Good Hope, that the family to which this genus belongs stands in quite different relations to other phanerogamous plants in Africa, and under the same latitude in South America and New Holland. The great number of *Astragali* in Siberia shews the proportion of Leguminosæ there, to be different from that of the temperate parts of Europe or North America. In Gmelin's *Flora Sibirica*, the Leguminosæ make $\frac{1}{14}$, in Germany about $\frac{1}{20}$*. The striking difference between a littoral and continental climate, which has been particularly noticed by WAHLENBERG †, is also very important here.

Since the polar zone is of so little extent in respect to geographical latitude, we may here, perhaps, establish laws from the *Flora Lapponica* only. There is also a great sameness of vegetation in the different countries of the north polar zone, which I myself have had an opportunity of knowing from the inspection of a valuable collection of Greenland plants. In the temperate zone, as has already been remarked, the difference is so great, that no calculation can be made from any single Flora without great errors. But this prevails more in the vast extent of the equatorial zone, where the different countries present the greatest variety; and even here the author's calculation is founded on a small number of the plants of one part of the world.

* Through an error in reckoning the author has $\frac{1}{18}$.

† Magazin der Gesellschaft naturforschender Freunde zu Berlin, 5ter Jahrgang (1811) 3 Stuck.

The second, and indeed the most difficult, method, which considers all the known plants, has also its defects, and amongst them particularly this, that we are not equally well acquainted with all the zones, nor with all the parts of each zone. However, this imperfection is of less importance than might, perhaps, be imagined; for, with the exception of some families, which by botanists, particularly in botanical travels, are neglected, (the *Cryptogamia*, and in a great measure likewise the *Glumaceæ)*, we may, with a tolerable degree of probability, draw conclusions from those which are known respecting such as are not known, since it cannot be admitted as a general law, that a greater proportion of the plants of some families than of others has been discovered. We may, therefore, reasonably suppose, that the proportions of families amongst themselves will be very little altered by new discoveries. In order to illustrate this by example, I have compared Michaux's Flora of North America with the new one by Pursh, and Linné's *Flora Lapponica** with that lately published by Wahlenberg. The following are the results :—

	Michaux.	Pursh.	Linné.	Wahlenberg.
Leguminosæ	$\frac{1}{19}$	$\frac{1}{19}$	$\frac{1}{45}$	$\frac{1}{35}$
Umbelliferæ	$\frac{1}{34}$	$\frac{1}{57}$	$\frac{1}{51}$	$\frac{1}{55}$
Compositæ	$\frac{1}{8}$	$\frac{1}{615}$	$\frac{1}{12}$	$\frac{1}{13}$
Cruciferæ	$\frac{1}{110}$	$\frac{1}{82}$†	$\frac{2}{24}$	$\frac{1}{23}$
Junceæ	$\frac{1}{157}$	$\frac{1}{152}$	$\frac{1}{27}$	$\frac{1}{25}$
Gramineæ	$\frac{1}{14}$	$\frac{1}{105}$	$\frac{1}{11}$	$\frac{1}{10}$

Now since the conformity between the formerly known and lately discovered plants is so striking, even in small numbers, this conformity would be even greater, were we to comprehend in our reckoning all the plants of a certain zone.

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* Edit. Smithii.

† The very small number of North American plants of this family is the reason why some newly-discovered species alter so considerably the proportion.

But although we may in this manner, with seeming accuracy, conclude from the known to the unknown plants, we should nevertheless always, in our investigation of the proportions of families in any country, in order to attain the truth as nearly as possible, consider all the known plants of this country, and not a part only of the same. It is therefore surprising, that M. Humboldt, in fixing the distribution of the families of plants in South America, p. xvi., employs only 3,880 phanerogamous plants, although he himself, p. viii., estimates the number of known plants in this portion of the world at 13,000, of which he gives 4,500 as already known in catalogues; and what is still more astonishing, p. vii., 5,500 as found by himself. In respect to the families formerly neglected, this makes a remarkable difference. So p. xvi. 333 Glumaceæ are given, which number to 3,880, stands nearly in the proportion of $\frac{1}{11}$: whereas p. 239, it is stated that M. M. Humboldt and Bonpland found in their travels 343 Glumaceæ, which number, compared with the whole number discovered, *viz.*, 5,500, presents the very different proportion of $\frac{1}{16}$.

Whilst therefore I cannot approve of the Humboldtian method, I will by no means deny that a comparison of the Floras of particular countries may be very interesting, for the purpose of ascertaining the different proportions of the families in any narrow district. But, in such a comparison, we ought to proceed with all possible circumspection, and fail not to take into consideration the various objects and views, by which the authors were actuated in the construction of their Floras. According to M. Humboldt, the ferns in Germany amount to $\frac{1}{48}$, in France to $\frac{1}{75}$ *, but this difference will, in a great measure, vanish, if we examine the sources of these results. Hoffmann's Flora, made use of by M. Humboldt, is, in respect to the whole number, very inaccurate; but, in the family of the ferns, he has introduced ten species, which more lately have justly been omitted †. If these 10 be deducted from the 42 given, there

* P. xiii.

† *Polypodium dentatum, incisum, trifidum, anthrisifolium, cynapifolium, tenere, fumarioides, pediculari folium, tanacetifolium, molle.* Ofi. WILDENOW's *Species Plantarum.*

remain 32, a number which, with Hoffmann's whole number, affords $\frac{1}{59}$. It may be very well assumed, that the *Flora* of Germany contains at least 2,600 phanerogamous plants, and that the number of German ferns known at present is 36—38; therefore the proportion of this family to the whole number of phanerogamous plants should be $\frac{1}{72}$ or $\frac{1}{70}$. Many *Floras* admit likewise cultivated plants, and since, of some families, a considerable number, but of others only very few are cultivated, there arises in this way another considerable variation.

The author very justly remarks, p. xiv., that in a comparison of different zones, we can only take into consideration those plants which grow in the plains. It is indeed evident that the result must be fallacious; if, for example, in South America, we reckon those plants which grow at any considerable height above the surface of the sea, since they, in respect to their *habitats*, belong to the temperate and frigid zones. From this, however, the author departs, in regard to the respective proportions of natural families given for South America, p. xvi., the plants of which these consist not growing in higher latitudes, for he says himself: "*Observandum tamen est hos numeros veram distributionem non accuratissime exhibere, quia non semper per plana Zonæ torridæ iter fecimus, sed persæpe per clivos Andium, ubi propter decrescentem aëris calorem, diversarum regionum Floræ se excipere videntur.*" Likewise in the proportions given, p. xiv. for Germany and France, and p. xv. for North America; those plants are not excluded which grow at such an altitude, that they cannot be regarded as belonging to the temperate zone. The superscription to the table, p. xviii., runs thus: *ratio plantarum in locis planis provenientium;*" it was therefore to be expected that the plants of the higher regions would be excluded. Whereas, we find, with the exception of the *Umbelliferæ* and *Cruciferæ*, that the proportions which are given here for the equatorial zone, agree pretty exactly with those mentioned, p. xvi.; which is in direct contradiction to what he afterwards advances, *viz.*, that, with some exceptions, the respective proportions of the families of plants vary towards the summits of the mountains, in the same manner as upon the plains towards the pole. If this be true (and it seems not to admit of any doubt,) the *Gramineæ*, *Labiatæ*, *Ericinæ*, and *Rhododendra*, should afford a smaller quotient, when the plants of the higher regions are excluded, than when they are taken into the calculation; the *Leguminosæ* and *Compositæ*, on the contrary, a greater. The *Junceæ*, whose number according to this reasoning, should be smaller in this place, are given as $\frac{1}{400}$, although before as $\frac{1}{430}$; the *Malvaceæ*, the number of which should be greater, are given as $\frac{1}{50}$, but before as $\frac{1}{47}$.

Whether the author, when treating of the second zone, has excluded the plants of the higher districts, I cannot determine, since, as has been already mentioned, I do not know the authorities which he employs. From the proportions, mentioned in the notes to the table, this does not appear to be the case, for the numbers correspond exactly with those given before.

Page 33, the author treats of the distribution of the 1,000 ferns, which are at present known. But here also those which grow in the higher regions are not excluded.

When an extensive family of plants contains many smaller families, or groups, which present a different, or entirely opposite geographical distribution; it seems proper, not merely to consider the distribution of the entire family, but likewise that of each group in particular. This the author has observed in respect to the *Glumaceæ*, three subdivisions of which, *Gramineæ*, *Cyperoideæ*, and *Junceæ*, are treated of separately: but not so in the *Compositæ*. Besides, is it not surprising that Jussieu's *Cichoraceæ* are quite excluded from the equatorial zone *.

This group, however, certainly diminishes from the temperate zone towards the equator. The whole family of the *Compositæ* should, according to M. Humboldt, diminish from the equator towards the poles. Willdenow affirms the contrary †. The numbers given by M. Humboldt for the equatorial and tem-

* About 340 species are enumerated in Persoon's Synopsis, at least 4—5 within the tropics.

† Grundriss der Kraüterkunde, p. 484, und Magazin der Gesellschaft naturf. Freunde zu Berlin. 1ster. Jahrgang, 2tes. Stück, p. 133.

perate zones, differ but little ($\frac{1}{6}$ to $\frac{1}{8}$); indeed, if we take for the temperate zone an equal extent to that used by the author for the equatorial zone, the proportions do not vary. At the Cape of Good Hope, according to Thunberg's *Prodromus*, the *Compositæ* make about $\frac{1}{5}$, a greater proportion than in the equatorial zone, according to M. Humboldt. This example, then, shews how fallacious are the calculations in respect to particular countries.

Besides, which ever method we choose, we shall always meet with plants that are common to several zones or Floras. Therefore the question arises, what are we to do with them. In my opinion, we should (in order to discover the increase of the number of plants in any family, either towards the pole or the equator) exclude them entirely, for, in the first place, it appears, that we learn most accurately the prevalence of this or that family in a climate, from the number of plants which are peculiar to it; and, secondly, we may, by the other method, obtain a result entirely unnatural, for in a small sum-total (totalsumme) a family may contain some plants which are but little affected by variations of climate. This remark will be rendered clear by an example. According to the author's estimate, the *Filices*, in Lapland, make $\frac{1}{26}$, in France only $\frac{1}{73}$; from which he concludes that Lapland, or the polar zone, is more favourable to these plants than France. But, it is quite evident that this conclusion is not just, because the ferns diminish from the tropics towards the temperate zone; and because, in the equatorial zone, the plants of this family are rare in the more elevated regions (see p. li.). This becomes even more manifest from the circumstance, that in Lapland, two only out of nineteen ferns found there are peculiar to it*; whereas, in France, we find a much larger proportion of ferns, which are not met with in polar countries. Those groups and genera, on the contrary, to which the polar zone is justly considered to be favourable, for example, *Carices*, *Saxifragæ*, and *Rhododendra*, possess in

* *Polypodium hyperboreum* and *Aspidium Lonchitis*. These appear likewise without the polar zone, but at such an altitude, that they cannot be admitted among the plants of the temperate zone.

this zone a much greater proportion of species which are peculiar to it. Lastly, only a few of the Lapland ferns appear at any considerable height, and Wahlenberg remarks that even these do not always resist intense cold *.

It has often been remarked already, that by reason of the present imperfect state of our knowledge, we cannot give the geographical distribution of certain families. This prevails chiefly in respect to cryptogamous plants, *Filices* excepted. Notwithstanding the author admits this, p. xiii., yet he brings forward in his tables the following proportions; the number of cryptogamous to the number of phanerogamous plants, in the equatorial zone 1 : 5, in the temperate 1 : 2, in the frigid 1 : 1. As it appears, that in the cryptogamia we should distinguish between the different families; so it is, perhaps, quite evident that the mosses and lichens exist in a much greater proportion in northern countries; for, in the first place, there are a great number of species peculiar to these countries, and, in the second, those two families, in Dr. Wahlenberg's *Flora Lapponica*, make about $\frac{4}{10}$ of the whole vegetation, whereas they comprise only a small part in the Floras of middle Europe. But, in respect to the fungi, the temperate zone is more favourable to them than the frigid; in the *Flora Lapponica* † they make only $\frac{1}{11}$ — $\frac{1}{12}$; whereas in M. Schumacher's *Enumeratio Plantarum Siællandiæ*, they amount to nearly one half of the whole Flora. And if we add, that the difficulties which oppose themselves to the examination and determination of the fungi especially in travelling, may have rendered the proportion too small, and besides that many fungi appear to be omitted in the last-named Flora; the difference then becomes so wonderful, that, at all events, the number in Lapland must be too small ‡.

* "Ex jam relatis constat plerasque Filices Lapponicas e latere Norvegico alpium provenire locis angustis occultis meridiei expositis. Paucissimæ in ipsis alpibus observatæ scrobiculos recessusque maxime absconditos sibi eligunt, ibique brevi vigent; adeo ut neque hæ repugnent cæterarum naturam frigoris impatientissimam." *Flora Lapponica*, p. 289.

† *Flora Lapponica.* Introductio, p. lxiii.

‡ Namely, 939 out of 2,189.

I am of opinion, then, that in the Flora of any small district in middle Europe, if we estimate the fungi at ⅓, we shall be near the truth.

As to the whole number of the *Cryptogamia*, the proportion of these to the phanerogamous plants, in the above-mentioned Flora, is as 2 : 1; exactly the reverse of that given by the author, for the temperate zone. In SCHUMACHER's *Enumeratio* they are as, 3 : 2, although this Flora is very defective, in respect to the aquatic Cryptogamia. These proportions by no means agree with the general proportions of the temperate zone; for plants of diminutive size, are common to a greater number of different districts, than those of a more conspicuous form; and if we examine any considerable tract of country, we shall find that the latter do not extend themselves in the same proportion as the former. In the mean time, we would certainly admit a higher proportion of cryptogamous plants for Germany, and France, than that given by the author.

At the conclusion of this division, I beg leave to offer a few remarks. The author says, p. xv., that the *Labiatæ* and *Umbelliferæ* diminish towards the pole, from which we might naturally conclude that they are most abundant in the equatorial zone, especially, as this is the case with the *Malvaceæ*, *Euphorbiaceæ*, and *Leguminosæ*, with which they are in general associated. But we perceive afterwards that the author reckons these two families, with those that are most abundant in the temperate zone. This is more clearly stated in the Treatise No. 2. Besides, it appears, p. xv., that the *Caryophylleæ* increase towards the pole; but this family is not inserted in the table, and, p. xiv., only a comparative view offered, of the number of species of this family in Germany, France, and Lapland. The *Coniferæ* are likewise omitted in the table. When Lapland is compared with France, or Germany, the number of this family is somewhat greater in the former; but in North America, there is a much greater proportion of species. Several other families, of which the proportion for Germany, France, and Lapland is given, are omitted in the tables, and in the view of South American plants, although we had reason to expect the greatest accuracy, in regard to those last mentioned, in the work before us.

At p. 240, the author says that, the *Glumaceæ* and *Compositæ* excepted, the most numerous families in the polar zone are the *Caryophylleæ*, *Amentaceæ*, and *Ericinæ;* but, p. xiv., there are allotted to Lapland 22 *Cruciferæ*, and only 20 *Ericinæ*. The *Leguminosæ*, *Cruciferæ* and *Labiatæ* are considered to be the most numerous families in the temperate zone, if we except the two first of the abovementioned; but, p. xiv., to France he ascribes 170 *Umbellatæ*, and only 149 *Labiatæ*; to Germany 86 *Umbellatæ*, and 72 *Labiatæ*. In North America only, is the number of *Umbellatæ* smaller than that of the *Labiatæ*. In respect to the equatorial zone, the *Leguminosæ*, *Malvaceæ* and *Rubiaceæ* are considered to be the most numerous families, with the exception of the *Glumaceæ* and *Compositæ;* but, p. xvi., to South America are given 95 *Labiatæ*, and only 80 *Malvaceæ;* and, p. 368, as many as 244 *Orchideæ*.

3. Of the social and solitary Appearance of Plants. p. xxi.

This part, contains very little more than what has been already said upon the subject, by the author in his *Essay*, and by Willdenow in his Elements. When M. Humboldt remarks here, that we find in the torrid zone of the New World, scarcely any social plants besides *Rhizophora Mangle*, *Sesuvium Portulacastrum*, *Croton Argenteum* and *Bambusa Quinduensis*, there is a contradiction of what he asserts, p. 369, *viz.*, that the *Orchideæ* of the torrid zone are distinguished from those of the temperate zone by their social appearance.

4. Whether, and in what degree, the New and Old Worlds possess the same Plants. p. xxii.—xxv.

It is an important and interesting fact, that amongst the less perfect plants we find more that are common to countries widely distant from each other, than amongst the more perfect. This fact is supported partly by evidence derived from *Brown's General Remarks*, viz., that of the Dicotyledonous plants found in

New Holland $\frac{1}{193}$, of the Monocotyledonous $\frac{1}{28}$, and of the Agamous $\frac{1}{3}$, are European, or North-American species, and partly from the author's observations upon the Flora of South America.

In respect to the torrid zone of America, the author has changed his opinion as expressed in the *Essay*. In that work * he thinks that European species undoubtedly appear amongst the cryptogamous plants, but none amongst the phanerogamous. He now admits that monocotyledonous plants, likewise of the Old World appear there; but says, that, with the exception of some plants of the sea-coast (Strandpflanzen), he had not discovered in the torrid zone of the New World one dicotyledonous plant of the Old World; and therefore supposes, that in respect to these plants, the same law is observed as Buffon has established respecting mammiferous animals.

Although we can very well conjecture, through the analogy of that which Brown has made known of New Holland, that in the torrid zone of America likewise, there are not so many dicotyledonous as there are monocotyledonous plants of the Old World; we are by no means authorized to conclude that there are none, because none were found by M. Humboldt. I could present a list of near 100 dicotyledonous plants, which according to different authors are common to the hot districts of America, Asia, and Africa, but particularly to the East and West Indies; and although I will not vouch for the identity of all these species, yet it appears that those brought forward by the author require a more accurate revision; as has already happened with some classes of animals.

In a note, p. 34, the author thinks it probable that Europe and North America have been formerly united, because many plants are equally indigenous in both these parts of the world. To me, however, the connexion between this phænomenon and his conjecture is not very evident, for I do not see why corresponding climates in different countries should not produce the same species, which the author himself seems disposed to admit, p. xxii †. If the conformity of the plants could prove

* Page 20.

† *Zonis frigidis et temperatis amborum continentium quasdam plantas ab initio communes fuisse, statuendum est.*

an ancient union, we might suppose that the same had existed between Europe and New Holland, for these countries, as has been already remarked, have plants that are common to both.

The hypothesis, advanced by the author, p. 54, *viz.*, that the great spread of *Adianthum Capillus Veneris*, may, perhaps, be ascribed to the transport of filtering stones, will scarcely find support.

5. Comparative View of the Temperature of both Great Continents, p. xxvi.—xxxiii.

This part of the treatise presents a most valuable addition to our knowledge of the relations of climate in different countries, and corrects likewise some erroneous conclusions hitherto received. The author particularly points out the unsoundness of the opinion, that every part of North America is colder in the same proportion than parts of Europe, placed under the same latitude, and proves that the difference between the two parts of the world is always greater towards the North Pole *.

Although we must certainly agree with the author here in the main question, yet the progression of the decrement of heat in the two parts of the world, as laid down by him, does not appear to be demonstrated ; in the first place, it is founded upon too few observations ; and, secondly, the comparisons are made in too arbitrary a manner. To render this more clear I will cite a few of these comparisons :—

I. Georgia. Territorium Missisipense. Ægyptus inferior †. Madera.

	Lat.	Calor. Med.	
Natches	31° 28′	18° 2′	Centig.
Funchal	32° 37′	20° 4′	
Orotava	28° 25′	21° 0′	
Roma	41° 53′	15° 8′	
Algerium	36° 48′	21° 1′	
Diff.	7° 0′	Diff. 2° 3′	

* The author here speaks of the difference between the New and Old Continents ; but the points of comparison are taken from the western parts of Europe and Africa. He also shews afterwards that the climate in the eastern part of the Old Continent is colder than in the western.

† No place in Egypt is mentioned in the table.

II. VIRGINIA. KENTUCKY. HISPANIÆ ET GRÆCIÆ PARTES MERIDIONALES *.

	Lat.		Calor. Med.		
Williamsburg	38°	0′	14°	5′	Centig.
Bindigala	44°	50′	13°	6′	
Mons Pessulanum	43°	36′	15°	2′	
Roma	41°	53′	15°	8′	
Algerium	36°	48′	21°	1′	
Diff.	70°	0′	Diff. 4°	0′	

III. PENNSYLVANIA. JERSEY. CONNECTICUT. LATIUM. ROMELIA.

	Lat.		Color. Med.		
Philadelphia	59°	56′	12°	7′	Centig.
Nov. Eboracum	40°	40′	12°	1′	
Maclovium	48°	39′	12°	5′	
Nannetes	47°	13′	12°	6′	
Neapolis	40°	50′	17°	8′	
Diff.	7°	0′	Diff. 5°	0′	

It could be wished that the author had explained, by what method, in this comparison, he has obtained the differences stated in the result; for it is merely said, p. xxvii.; "*Quibus ex exemplis, methodo interpolationis collatis patet*, &c." I think the following has been his process. With a certain place in North America, whose mean annual temperature is known, for example, Natchez (31° 28′ lat., 18° 2′ mean temp.) he makes a twofold comparison, *viz.*, he first compares with this place one in the Old World, which lies under the same degree of latitude; the mean temperature of this last, compared with that in North America, gives then the difference of temperature, which is placed at the end of the second column. Secondly, he compares with it a place in the Old World, which has the same mean temperature, and this comparison then gives the difference of latitude between the two places, which is placed at the end of the first column. But, as we cannot

* Among the places mentioned not one belongs either to Spain or Greece.

easily have, for such a comparison, places in the Old Continent, whose mean temperature, or geographical latitude, agrees exactly with the given places of the New Continent, the author has, in each of these comparisons, made use of two places in the Old Continent, from which the geographical latitude, or the mean temperature of the place is determined, that is to be compared with one in North America. He must, then, in order to institute a comparison with Natchez, first fix upon a place in the Old Continent, which lies under 31° 28′ of latitude, and the mean temperature of which should be known; to obtain this, he compares Funchal 32° 37′, and Orotava 28° 25′, and calculates from the difference between the mean temperatures of both places, the probable mean temperature of 31° 28′ in the Old Continent; this is 20° 5′, which number, compared with 18° 2′, gives the difference = 2° 3′. Secondly, he must have a place in the Old Continent, whose mean temperature should be 18° 2′; he obtains this place in the same manner through a comparison between Rome 15° 8′ (41° 33′ of latitude), and Algiers 21° 1′ (36° 43′ of latitude). I must, however, declare that this last comparison, as well as some of the following ones, do not afford the given difference; which I can only explain through the author's having chosen round numbers.

But, if we compare these two methods, we shall perceive that the author, according to the first, gives 7° as the difference of latitude between two places of the same mean temperature, both in the first and third parallels. According to the second method, however, the difference of mean temperature in the first parallel is only 2° 3′, in the third 5° 3′. It is besides evident, that if the difference of temperature between two places near the North Pole, is greater than between two others more to the southward, the difference of latitude between two places of the same temperature cannot, in both cases, be the same. This surprising difference, in the author's results, proceeds from his having employed, in the first method, places of a temperature comparatively too low, and in the second, too high. For instance, in the first comparison, he takes Orotava, which city, though 8° more southward, has a lower mean temperature than

Algiers, which is made use of in the second comparison. Orotava has a lower temperature than other parts of the Old World in the same latitude; partly, because it is exposed to the influence of the sea, and partly, because it is situated 163 toises above its level *. Algiers, on the contrary, has too high a temperature from being exposed to the effects of the sandy deserts of Africa. If, in the first method, we were to employ the temperature of Algiers, instead of that of Oratava; then, the mean temperature of 31° 28′ (5° 20′ more southward) would be 2° higher †: we should thus obtain a mean temperature of 23° 1′, which number, compared with the mean temperature of Natchez, gives a difference of 4° 9′ instead of 2° 3′. It appears quite as improper to estimate the mean temperature of Europe under 68° 30′ of latitude, according to Eirontekis (— 2° 8′); for Mageröa, the latitude of which is 71°, has a mean temperature of + 0.07 ‡.

This example seems sufficient to prove how uncertain and fallacious it is, to institute comparisons between the two parts of the world through the mean temperature of single places.

This part of the treatise presents likewise a new verification of the law, established and illustrated with so great perspicuity by M. Wahlenberg §, *viz.*, that the mean temperature of the whole year affords a very inaccurate measure of vegetation. Thus has Wahlenberg shewn, that Eirontekis with a mean temperature of — 2° 8′ has a much richer vegetation than the North Cape with a mean temperature of + 0.07, or than the hospital of St. Gothard with a mean temperature of — 0.9. The zone also in the equatorial regions in which trees cease to grow (1,800 toises above the surface of the sea) has a mean temperature of nearly 9° *cent.*; the elevation at which the snow-

* Humboldt et Bonpland Reise in die äquatorialgegenden. 1ster. Theil, p. 450.

† Between Rome and Algiers the author has for 5° 5′ of latitude, a difference of 5° 3′ mean temperature.

‡ Wahlenberg's *Flora Lapponica*, p. 47.

§ *Flora Lapponica;* und eine Abhandlung über die Temperatur der Quellen; in den Schriften der Schwedischen Gesellschaft der Wissenschaften, 1811.

line exists, a mean temperature of + 1 : 5 *; whereas the utmost reach of trees in Lapland extends to 500 toises, and the snow-line to 550 toises, above the surface of the sea, although, as already mentioned, the mean temperature at the level of the sea is below 0°. This point cannot easily be proved theoretically; for the mean temperature of the year is partly determined by the temperature of winter, and that has no influence whatever upon vegetation.

Now although M. Humboldt admits that this †, and many other comparisons instituted by him ‡, and likewise various observations made in the following parts of the treatise §, afford proof of the above; he, nevertheless, employs more signs of mean temperature in his comparative views of vegetation in different countries. This is the case, for example, in the above-mentioned parallel between North America and the old continent, from p. xxvi. to p. xxix; likewise in the table, p. xviii.||, p. x. xvi. xix. and 36, and in part p. lv. In other places he uses a different measure (maastabes), for instance p. xi. and xxx. and in part p. lv. and lvi., the mean temperature of the year and the temperature of summer are used; p. xlii., and likewise in the smaller treatise, the difference between the summer and winter temperatures; p. liv., the annual mean temperature, and the sum of the temperature of those months, the mean temperature of which exceeds 0°; p. xliii., the mean temperature of the year, and that of the hottest and coldest months; p. xlix., the annual mean temperature, and the mean temperature of winter,

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* See p. xlix.

† Pag. xliii. and liv.

‡ Pag. xxix. to xxxii.

§ For example, p. xlvi. xlviii. and liv.

|| It is also surprising that in this table for the equatorial zone, only one fixed mean temperature of 27°. is given, and not, as in the two other zones, a maximum and minimum. Now although the mean temperature from the equator to the tropics appears to decrease less than beyond the same, yet we cannot admit that it is the same at either of the tropics as under the equator. However, p. xxxiv., he ascribes to the equator a mean temperature of 30°.; and, p. xxix. and xxxix., to the Havannah (23° 8′ of latitude) 25° 6′; to Vera Cruz; (19° 11′ of latitude) 25° 4′

and likewise of August; p. lvii., the mean temperature of the year, of summer, and of autumn; and lastly, p. xxix., the annual mean temperature, and the temperature of each of the four quarters of the year, with that of the hottest and coldest months.

Exclusive of the wish that the author could always have used the same measure of temperature, I must here remark, that although the last of the methods considered appears to be the best, yet neither of them perfectly answers the purpose. In my opinion, the temperature of any place, inasmuch as relates to vegetation, is best shewn by a curve, which exhibits the state of the same during the cycle of vegetation, as given by Wahlenberg in the *Flora Lapponica*, and in his *Tentamen de Vegetatione et Climate Helvetiæ*. The size of this curve shews, for instance, the quantity of heat which the given place enjoys; and the form of the same shews the manner in which this quantity is distributed during the different parts of the year within the cycle of vegetation. If the curve be very high and narrow, the place has a short but hot summer, as is the case with Eirontekis; if the curve, on the other hand, be low and broad, the summer is long and temperate, as happens generally in the neighbourhood of the sea-coast and in the islands. If the curve have many deviations from a regular form, the temperature is very variable, and so forth. But, as it might be useful to express these proportions by numbers, I would propose the following formula: a number which represents the sum of the quadrate, included within the curve *; then a number for the height, and another for the breadth of the curve, to which must be added one to represent the tortuosities of the same †; and, lastly, a number for the difference between the temperature of night and day, which is likewise very important, but, in general, from the want of observations, extremely difficult to be obtained.

* See Wahlenberg's *Tentamen*, § 83. He proposes this sum as a measure; but the observation made by himself, § 85, *viz.*, that the distribution of heat has an important influence, contradicts it. However, he is not quite satisfied with it.

† This may be shewn by a regular curve-line drawn upon the curve of temperature.

A third error, which the author endeavours to correct, is this, that the southern hemisphere is altogether colder than the northern. He shews that this prevails only in the part beyond the tropic.

6. The Influence of different Altitudes upon Vegetation in the different Zones of the Earth.—(p. xxxiii.—liv.)

The author, in the first place, presents a view of the varieties which regions of different altitude afford, in respect to climate and vegetation in the equatorial zone (in America). These varieties are given first for the zone between 0° and 10°, and then for that between 17° and 21° of latitude, and in both zones, in respect to altitude, three regions are considered; *regio calida, temperata, et frigida.* It might, perhaps, be thought that the hot region, which rises to 200 toises, is too circumscribed, to be compared with the torrid zone; but, if we take into consideration, a remark made by the author, which I shall mention presently, this probably is not the case. On the other hand, we cannot suppose that the temperate region under 17—20° of latitude, commences at the same height as under 0—10°.

After this view of the equatorial zone, the author takes notice of the varieties, which difference of altitude produces in the temperate and frigid zones; for instance, in the Pyrenees, in the Caucasus, in Switzerland and in Lapland, according to Ramond, Engelhardt, Parrot, and Wahlenberg. I have to remark in this place, that the author places the limits of *Betula Alba* upon the Caucasus, at 1,050 toises, although, according to Engelhardt and Parrot *, this tree, in one place only, under very favourable circumstances, advanced to that height. According to them its mean height (which only we are to consider here) is 870 toises.

All the results are represented in a table, p. xlix., by means of which a comparative view is afforded of the relations in

* Reise in die Krimm und den Kaukasus.

different climates. To render this comparison more complete, we may now add to this table the observations of M. Wahlenberg in the Carpathian Alps *. I am also enabled, through the kindness of a friend, to present a similar view of the Norwegian chain, from 60° to 61° of latitude. The following are the relations in these two countries :

	Carpathian Alps. Lat. 46°.	*Norwegian Alps.* Lat. 60° 61′.
Snow-line	1330 T.	850 T.
Distance of the limit of trees from the snow line	570 T.	316 T.
Height of the limit of trees	760 T.	534 T.
Trees which this limit presents	*Pinus abies*	*Betula alba*
Limit of shrubs	930 T.	634 T.
Shrubs which this limit presents†	*Pinus Mughus*	*Betula nana* *Salix glauca*
Distance of the limit of corn from the snow-line	930 T.	520 T.

I propose now to make a few observations upon the table. The annual mean temperature, the temperature of winter, and the temperature of August, assigned to the more elevated regions of Switzerland and Lapland, are most probably calculated, according to the generally-received decrement of temperature. But if this be correct for Switzerland, it is nevertheless doubtful, whether, while observations are wanting, the same laws prevail in Lapland. Besides, when the limit of ***Betula alba*** is placed at 250 toises above the sea, and the distance of this limit from the snow-line at 300 toises, there is evidently in this place an error of the press; for, in the *Flora*

* *Flora Carpathorum.*

† The author employs the Rhododendra in his comparison; without specifying any either in the Carpathian Alps or in the above-mentioned part of Norway. But since the limit of *Betula nana* and *Salix Glauca*, is not much lower in Lapland than that of *Rhododrendron Lapponicum*, and according to Wahlenberg's view, *Pinus Mughus* in the Carpathian Alps, stands in comparison with *Salix glauca* in Lapland, and *Alnus viridis* in Switzerland, I have chosen the above plants. I will not deny, however, that the parallel is not perfectly accurate.

Lapponica, the contrary proposition is given; the absolute height 300 toises, the distance 250 toises. In determining the limit of trees in the equatorial regions, the author employs *Escallonia* and *Alstonia;* but, as he says in his *Essay* *, when speaking of his *Region* of *Wintera* and *Escallonia*, that it contains shrubs with short stems, and branches trailing on the ground, it appears that we cannot well compare this vegetation with the pine forests of Switzerland, or the birch woods of the north, but rather with *Alnus viridis*, and the *Rhododendra* in Switzerland, and *Betula nana* in the north.

It appears, moreover, that from such a comparison between the vegetation of mountains in different climates (if we abstract from it all anomalies arising out of particular local relations) we may deduce the law, that a line of vegetation, *viz.*, that of the limit of trees, drawn from the equator towards the north pole, does not run parallel to the snow-line, but converges towards it. It is not difficult to make this apparent. It has been already proved that it is not the annual mean temperature which determines the vegetation, but the temperature which prevails within the cycle of vegetation; hence it follows, that towards the north pole, the vegetation is richer in proportion to the difference between the different periods of the year, as might be inferred from the phænomena which present themselves in the equatorial mountains, where the annual mean temperature is nearly the same as that of each period of the year. Since, on the other hand, the snow-line, if it be not entirely governed by the mean temperature, stands nevertheless in a more exact relation to it, because the summer temperature has less, and the winter temperature more, effect upon it than upon the plants; it follows, therefore, that the limit of trees towards the pole must approach nearer the snow-line. In connexion with this stands the circumstance, that during the cycle of vegetation the day towards the pole is always longer, and consequently the plants receive a greater quantity of heat, and the difference between the temperature of day and night is less. The author

* *Essai sur la Géographie des Plantes*, p. 69.

has likewise mentioned these two facts, p. l. Perhaps a third may yet be adduced, *viz.*, that the pressure of the atmosphere, at the same distance from the snow-line is different in different latitudes. The author indeed admits this, as well in this treatise, p. liv., as in his *Essay*, and ENGELHARDT and PARROT hold the same opinion *. On the contrary, DECANDOLLE, SPRENGEL, and WAHLENBERG †, assert that the pressure of the atmosphere has not any influence upon vegetation. At any rate the difference of altitude must have this effect, *viz.*, that the degree of humidity will vary according to the same. It is stated in M. Humboldt's *Essay*, that the mean of Saussure's hygrometer, 300 toises above the sea (250 toises lower than the snow-line in Lapland,) is about 83°., but, at the height of 2,210 toises (250 toises lower than the snow-line in the equatorial mountains), it reaches only to about 54°.

Another phænomenon, closely connected with this, and which is to be regarded as a continuation of the same subject, is that the difference in the distribution of plants, which the difference of altitude occasions, does not stand in the same relation with that produced by difference of latitude ‡. If we thus first compare the distance of the limit of trees from the surface of the sea at the equator, with the distance of the snow-line from the same; and secondly, the distance of the limit of trees above the table-land at the equator, with the distance of those degrees of latitude where the snow-line is contiguous to the sea, which is the case in the northern hemisphere, particularly at 80° of latitude; we shall find that the limit of trees, in respect to latitude, is proportionally much more extended towards the pole, than upon the equatorial mountains towards the snow-line. The proportion is accordingly for the comparison of altitude, 2,460 toises : 1800

* Reise in die Krimm und den Kaukasus.

† *Flore Française.* Tom. II., p. xi. Bau der Gewächse, p. 622. *Flora Carpathorum*, p. cxvi.

‡ We might, perhaps, call the former the VERTICAL, the latter the HORIZONTAL distribution.

toises * = 4 : 3; for the distribution of latitude 80° : 71° † = 10 : 9. If the proportions were the same, all vegetation of trees must cease at 60° of latitude.

Corn ascends, according to M. Humboldt, at the equator to the height of 1,600 toises, which gives, with the height of the snow-line, the proportion 2 : 3. If then, in respect to latitude, a similar proportion should take place, corn would not be seen beyond 54° of latitude. But in Lapland it is cultivated as far as 68—70°. This appears already from the remark of the author, p. liii., *viz.*, that it is very erroneous to suppose that Quito has the same climate as France or Italy; because, although the mean temperature of both places is the same, the distribution of heat is nevertheless very different.

The author shews, p. li. lii., what families occupy the highest regions in the different zones. In the polar zone, we may very well suppose the *Cyperaceæ* to prevail there, on account of the great numbers of Alpine *Carices*. But when he thinks that the *Saxifragæ* are rare in the same region, we cannot agree with him. Wahlenberg's *Flora Lapponica* contains Alpine species of the genus *Saxifraga*, and in the above-mentioned Greenland collection, this genus was the most numerous after *Carex*.

VII. Determination of those relations of Climate, which are most favourable to any of the more common cultivated Plants. (P. lv. lvii.)

Concerning this part I have only to remark, that such views are of the greatest practical advantage; but that they would probably be much more useful, if the temperature was more accurately given, which might be done in part by means of the above-mentioned curve of temperature.

As to the result of this examination, to which I have been led by the labours of M. Humboldt, I am unquestionably of opinion, that these treatises, exclusive of many interesting ideas

* It has already been observed, that this estimate appears too high.

† This is the northern limit of birch given in the *Flora Lapponica*, p. xiv.

and views, contain the most important observations upon the geography of plants. On the other hand, however, it is very evident, that this subject, perhaps through the numerous avocations of the author, has received less attention, than from the interesting nature of the inquiry could be wished. It is scarcely necessary for me to add, that I entertain the greatest respect for the worthy author, and fully acknowledge his distinguished services to science in general, and to the geography of plants in particular. Nor have I any reason to think, that if you, Sir, should deem these observations not unworthy a place in your Journal, they will be displeasing to him. On the contrary, I am persuaded, that even the smallest contribution will be well received by one, who is so great an admirer of science; especially when it relates to a science, of which he is regarded as the principal founder.

I am, &c. &c.

Art. IV. *Hints on the Manufacture of Catgut Strings.*

[In a Letter to the Editor.]

Dear Sir, *Killin, Sept.* 15, 1820.

As no object connected with the arts is beneath your notice, you will perhaps give room to the following hint, on the subject of musical strings, particularly as it is founded on physiological considerations.

It has long been a subject of complaint, as well as a serious inconvenience to musicians, that catgut strings cannot be made in England of the same goodness and strength as those imported from Italy; an inconvenience which was experienced in a great degree during the late war. These, I need scarcely say, are made of the peritoneal covering of the intestines of the sheep; and, in this country, they are manufactured at Whitechapel, and probably elsewhere, in considerable quantity; the consumption of them for harps, as well as for the instruments of the violin family, being very great. Their chief fault is weakness; whence it is difficult to bring the smaller ones, required for the higher notes, to concert pitch; maintaining at the same time, in their form and construction, that tenuity, or smallness of diameter, which is required to produce a brilliant and clear tone. The inconvenience arising from their breaking when in use, and the expense in the case of harps, where so many are required, are such as to render it highly desirable to improve a manufacture, which, to many of your readers, may however appear sufficiently contemptible.

It is well known to physiologists, that the membranes of lean animals are far more tough than those of animals that are fat or in high condition; and there is no reason to doubt that the superiority of the Italian strings arises from the state of the sheep in that country. In London, where no lean animals are slaughtered, and where, indeed, an extravagant and useless degree of fattening, at least for the purpose of food, is induced on sheep in particular, it is easy to comprehend why their membranes can never afford a material of the requisite tenacity. It is less easy to suggest an adequate remedy; but a knowledge of the general principle, should this notice meet the eyes of those interested in the subject, may at least serve the purpose of diminishing the evil and improving the manufacture, by inducing them to choose in the market the offal of such carcases as appear least overwhelmed with exuberant fat. It is probable that such a manufacture might be advantageously established in those parts of the country where the fashion has not, as in London, led to the use of meat so far over fed; and it is equally likely, that in the choice of sheep for this purpose, advantage would arise from using the Welch, the Highland, or the South-down breeds, in preference to those which, like the Lincoln, are prone to excessive accumulations of fat. It is equally probable, that sheep dying of some of the diseases accompanied by emaciation, would be peculiarly adapted to this purpose.

That these suggestions are not merely speculative is proved by comparing the strength of the membranes in question, or that of the other membranous parts, in the unfattened Highland sheep, with that of those found in the London markets; and although a project for putting them to a practical trial, which was suggested some years ago at this place, has not succeeded, the failure must be attributed to the want of mercantile energy in the person to whom it was recommended. Sufficient proof has been afforded that the general principle is correct; but it would be too much to expect from a Highlander the activity or perseverance, required for the establishment of such a manufacture in his own country.

I am, Sir, your obedient servant,

J. Mac Culloch.

Art. V. *Observations on the Theory which ascribes Secretion and Animal Heat to the Agency of Nerves.* By W. P. Alison, M.D., F.R.S.E., *&c.*

As Dr. Wilson Philip has done me the honour to reply to some of the objections I stated to that part of his physiological doctrines which asserts the dependence of secretion on nervous influence, I must beg permission to say a few words in defence of my former paper, before proceeding to the proper object of this.

1. Dr. Philip speaks of the disadvantage under which I labour in this inquiry, in consequence of not being an experimenter. This consideration might have been urged with perfect justice, if I had taken upon me to put my readers in possession of any *new facts* in regard to the physiology of the nervous system, or of secretion; but as I have assigned myself, in these papers, the humbler office of correcting what appear to me to be unwarrantable inferences, that have been deduced from *facts already recorded*, I thought the fairest way of proceeding was to take the facts exactly as I find them stated by those who have observed them. If I have in any place misunderstood him, or any other author, or omitted any particulars which may be thought important, I shall most willingly take his corrected statements of the facts, as the basis of my reasonings; and I admit, without hesitation, that if these reasonings shall not hold, in reference to those *corrected statements*, they are of no value. My object is merely to determine what inferences in regard to

the connexion of the nervous system with the *organic functions** of the body are warranted by the facts that we possess; and, in particular, to state the doubts that have occurred to me in regard to the doctrine, that a constant agency of nerves is concerned in the performance of these functions. If it shall appear, that opinions have been prevalent on this subject, which are not only not proved, but are rendered exceedingly improbable, by knowledge already in our possession, it must be admitted that it is important, *as a preliminary to farther investigation of the facts,* to have these opinions corrected. And on the other hand, if the objections which I state are founded on erroneous or imperfect notions of the facts that have been observed, these facts may be easily stated in such a way as to obviate the objections, and to shew, beyond doubt, that the conclusions have been fairly deduced from them.

2. Dr. Philip thinks, that in asserting "that in the muscles of involuntary motion, the nervous influence produces an alteration in the vital power or tendency to contraction," I have advanced an opinion altogether new, and untenable.—Now I object to the term *nervous influence* on this as on other occasions, on account of its vagueness, and of its apparently implying a theory, the truth of which I very much doubt. But the statement, which I made, that the vital power, or tendency to contraction of muscular fibres, and particularly of the involuntary muscles, may be altered or even destroyed, by *impressions made on the nervous system,* is one, the correctness of which I apprehend Dr. Philip could not have disputed, if he had not misapprehended my meaning in the above sentence; because my intention was, not to advance a speculative opinion, but to express a general fact, the truth of which is no where better illustrated than in his own works, although his mode of expressing it seems to me objectionable.

Dr. Philip (pp. 243, 244) refers to twelve experiments of his

* By the term organic functions I mean those which take place in the natural state, without the intervention or consciousness of the mind.

own, as proving that the nervous influence is capable of acting as a sedative to the heart and vessels of circulation, even to such a degree as to *destroy their power* ;—and in another place he states it as the result of many experiments, that " suddenly crushing any considerable portion of the brain or spinal marrow *instantly destroys the power* of the heart."

In various passages of his work, he speaks of the power of the heart being *impaired* by various causes acting on the nervous system ; for example, at p. 88. It is obvious, therefore, that the opinion of the vital power of the heart being liable to *diminution* from causes acting on the nervous system, is one which I share with Dr. Wilson Philip himself; and the only difference of opinion is in regard to the question, whether its power may be *increased* by such causes, which is really of less consequence, because if the vital power of the heart and of other muscles may be increased in this manner, as I think it may, it is but seldom that this kind of effect on it is produced.

Dr. Philip found in different experiments, that the action of the heart might be much affected by various agents, applied to the nervous system, to which he gave the name of *stimuli,* for example, by spirits of wine applied to the brain or spinal marrow. The account which he gave of the operation of these agents was, that they *increased the ordinary action* of the heart, which was going on at the time when they were applied, (Experiments 14, 15, 34, 36, 38, 41, 42, 43, and p. 245.) The effect of these appeared to me to be an *increase of the contractile power* of the heart, rather than the application of a direct stimulus to it, such as the prick of a pin on its muscular substance ; and to be directly opposed to the effect of other agents (for example, of tobacco,) which manifestly, and by Dr. Philip's own admission, *diminish* the contractile power of the heart. Dr. Philip objects strongly to this way of expressing the result of these experiments, and in opposition to it states a fact of which a confess I was not aware, that the effect is produced by the spirits of wine, although the heart is completely emptied of blood, If by this he means, that when the heart is emptied of blood, and *completely quiescent,* the application of spirits of wine to the brain uniformly *throws it into action*, I shall most willingly admit, that it is a fair example (and the only unequivocal one, as far as I know, on record) of the heart being *directly stimulated* through the medium of the nervous system. But if his meaning is, that the movements of the heart, continuing after it has been emptied of blood, or renewed by other causes, are *increased* by the application of spirits of wine to the brain, I think the correct expression for the fact will be, that its *contractile power is increased*, not that it is directly stimulated*.

But whether in these circumstances, so different from any that ever occur in the natural state, a stimulus, strictly speaking, may act upon the heart through the nervous system or not, I think we have sufficient evidence, independently of the experiments in question, that the contractile power of this and other muscles is liable to increase, as well as to diminution, from causes acting on the nervous system. For example, when the mind is under the influence of any strong exciting passion, the action of the heart becomes quicker and stronger. This appears to me to be just the counterpart of the effect produced on its action by depressing passions of the mind, which cause more or

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* Many physiologists, and Dr. Philip amongst the rest, use the words stimulus and sedative, as if these were directly opposed to each other. But I believe the word stimulus is most correctly applied to what merely *calls into action* the power inherent in muscular fibres during life, without necessarily either increasing or diminishing it; the term sedative to that which *diminishes* or *destroys* that power, and *prevents its being called into action in future* in the same manner that it used to be. According to this use of the term sedative, (which is, I think, the same as that to which it is applied in Dr. Philip's writings,) the term which is strictly opposed to it is *stimulant*, by which I understand, that which gives a temporary increase to the contractile power of a living muscle, and so enables it to *perform its ordinary function in obedience to its ordinary stimuli more vigorously* than it used to do. The effect of salt, sprinkled on a bare muscle, I would express by the term stimulus; that of wine taken into the stomach on the action of the heart upon the blood, by the term stimulant. Whether these terms are correctly applied or not, I think it must be allowed, that there is a real and essential difference between the modes, in which the muscles are affected in these two cases; and it appears from the passages formerly quoted from Haller, that this distinction is justified by his authority.

less of faintness, with a weak and often slow pulse. All must admit that this last change on the action of the heart is the consequence of an alteration (*viz.*, a diminution,) of its contractile power; and is it not almost self-evident, that the former change must be the consequence of an increase of that power? It is certain, that the heart of a person under the influence of exciting passions, contracts more frequently and strongly on the blood which it receives, than usual. The only measure that we have, or can have, of the contractile power of the heart, is the frequency and strength of these contractions. It is allowed, I believe, on all hands, that this change of its action is produced through the medium of the nervous system; and this being so, is it a very new or untenable opinion, or rather is it not a tolerably correct expression for this acknowledged fact, to say, that the heart is liable to increase, as well as to diminution, of that vital power, by which it contracts on the blood, in consequence of impressions made on the nervous system*?

I should not have dwelt so long on this part of the subject if it had not appeared to me of considerable importance, in various departments, both of physiology and pathology, to keep in view the difference between the cases where muscular fibres are directly stimulated to contraction by excitation of their nerves, or of the brain, and those where causes acting on the

* Dr. Philip speaks of the difficulty of understanding, how a power, not derived from the nervous system, can be liable to increase from what he calls nervous influence. This difficulty seems to me to lie more in words than reality. If the contractile power were a tangible or appreciable *substance*, as some physiologists have supposed, we should be much at a loss to understand, how any change taking place in the nerves should increase what the nerves did not originally bestow; but when it is distinctly understood, that by the term irritability or contractile power, we mean merely to express *the fact*, that those parts which are possessed of this property are thrown into contraction under certain circumstances, and do not attempt any explanation of this fact, it appears to me not at all more difficult to understand, that this property should be liable to increase, from causes acting on the nervous system, than that it should be liable to diminution, as Dr. Philip allows that it is; or than that it should be liable to increase from elevation, and to diminution from reduction of temperature; as we know that it is, although no one supposes that irritability is *derived* from heat.

nervous system alter the irritability or contractile power of these fibres. I believe we shall not commit any material error in asserting, that for all useful purposes at least in the animal economy, it is in the first of these ways that the *voluntary* muscles are affected by changes in the nervous system; and in the second, that the *involuntary* muscles are affected by such changes; and it would appear, from the passages quoted from Haller, that this was his opinion in regard to the uses of the nerves of the two sets of muscles. But the farther prosecution of this curious subject would be foreign to the purpose of this paper.

3. The main objects of my former paper were, *first*, to shew that Dr. Philip's conclusion, in regard to the dependence of secretion on an influence derived from the nerves, did not necessarily follow from his premises; and, *secondly*, to state various considerations, which I had not seen fully stated by others, which seem to me to render that conclusion very improbable. The argument on the first head may be recapitulated in a very few words.

Dr. Philip distinctly proved, that although the actions of the heart may be totally suspended by crushing, either the spinal marrow, as in Le Gallois's experiments, or the brain, yet these actions are perfectly independent both of the brain and spinal marrow, and may go on, although both are removed from the body. Hence, those lesions of the nervous system are no proof that the actions of the heart depend on, or are derived from, the brain or spinal marrow. Several experimenters have found, that another lesion of the nervous system, cutting the eighth pair of nerves, materially alters the secretion of the stomach. When Dr. Philip inferred from this, that the secretion of the stomach is *necessarily dependent* on the nervous system, he seemed to me to have fallen into the very same error that he had pointed out in Le Gallois; because, as we are perfectly ignorant how any lesion of the nervous system affects either muscular action or secretion, it is just as possible, for any thing we know to the contrary, that secretion, although independent of nerves, may be altered or suppressed by cutting nerves, as that the action of the heart, though independent of the spinal marrow, may be altered or stopped by crushing that organ.

Dr. Philip, however, maintains, in answer to this, that Le Gallois's conclusion could not have been objected to, unless it had been found that the heart is not affected by dividing the nerves issuing from the spinal marrow. If the action of the heart had been equally destroyed, both by crushing the spinal marrow and by cutting the nerves, he says, "Le Gallois's inference would have been unavoidable."

This is the precise point at which it appears to me that Dr. Philip has gone wroug. Le Gallois's opinion was *disproved* by Dr. Philip's experiments, in which the action of the heart continued, notwithstanding the division of the nerves, and removal of the brain and spinal marrow from the body; but I maintain that *it would not have been proved,* although that operation had stopped its action as effectually as crushing the spinal marrow did. The heart's action we now know is independent of the nervous system. Yet a certain injury of the nervous system stops that action. Why might not another? What reason can be given for supposing that a conclusion might have been fairly deduced from the effect of one lesion of the nervous system, which we have seen was unfairly deduced from that of another? If I understand the subject rightly, the effect which crushing the spinal marrow had on the action of the heart, in Le Gallois's experiments, was a fact; but the conclusion, that the living power of the heart depends upon, or is derived from, the nervous system, *involved a theory,* which would not have been confirmed, although fifty other injuries of the nervous system had done the same thing; and the very same theory is involved in the answer which Dr. Philip has given to the argument which I had advanced, in regard to secretion.

When Dr. Philip calls on me, therefore, to point out some way of *withdrawing the nervous influence* from secreting surfaces without destroying their power, I answer, 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. The simple fact is, that the secretion is changed when the nerves of the secreting part are cut. It may be, that this is because something *continually* passing from the brain or spinal marrow to the secreting organ, which is all that I can understand by the term nervous influence, as here used, is intercepted by the division of the nerves; but I have never seen any proof of this;—to take it for granted, as Dr. Philip has done in the present argument, is an assumption very little short of a *petitio principii*;—the rule of logic is, *affirmantibus incumbit probatio;* and those who advance any theoretical explanation of a fact are not entitled to shift this *onus probandi* off themselves, and lay it on their opponents, until they have made it manifest that the fact admits of no other explanation.

In the mean time, the doctrine of nervous energy, as above explained, being, as far as I know, a *mere hypothesis,* we are entitled to state on the other side,—*first*, that there is *no lesion whatever* of the nervous system, from the effect of which, in stopping any function of the animal body, it *necessarily* follows, that that function is dependent on nerves,—because there is no lesion, the effect of which may not be equally well explained by supposing it to communicate a noxious influence, as by supposing it to cut off a salutary one*;—and, *secondly*, that since it has been found, by Dr. Philip himself, that the action of the heart, though very readily influenced by injuries of the nervous system, is truly independent of it, we have the evidence of analogy for thinking, that the action of secretion may be equally *independent* of the nervous system, notwithstanding the facts which shew, that it may be readily *influenced by injuries of nerves.*

But *secondly*, granting *(for the sake of argument only)* that there is such an existence in nature as a nervous influence continually passing from the larger masses of the nervous system to the small branches,—it appears to me, that some of the facts which I formerly noticed, exactly answer Dr. Philip's demand;

* In using the term noxious influence, I would not be understood to express any opinion as to the *mode,* in which an injury of the nerves affects secretion; but merely to indicate, that I conceive the fact demonstrates only the effect of a certain *injury* of nerves, not the effect of any previously existing *influence* of nerves.

inasmuch as they afford examples of secretion going on, where this nervous influence either must have been cut off, or could never have been applied.

It appears, even from some of Dr. Philip's statements (to which I confess I did not sufficiently advert in my former paper), that in his experiments, the alteration made on the secretion of the stomach, by cutting the eighth pair of nerves, was *in quality only*, not in quantity. The secretion goes on, but its chemical composition appears to be so altered, that it does not act, as formerly, on the food. The secretion on the membrane lining the bronchia, instead of being diminished, is very considerably *increased* by this operation. Whether its composition is altered or not, does not appear. In like manner, Bichat found*, on dividing the nerves supplying the testis of a dog (the only gland, he affirms, on which this experiment can be fairly tried), that the gland inflamed and *suppurated*,—a process allowed by the best pathologists to be strictly analogous to secretion.

The state of the fact, therefore, is, that a material change is produced on the composition of the secretion of the stomach by the division of the eighth pair of nerves; but that in this, and in other instances, secretion (that is, the formation out of the blood of a substance not previously contained in it) goes on, notwithstanding that division. From this fact Dr. Philip concludes, *first*, that secretion in general depends on an influence transmitted by the nerves to the secreting organ; and, *secondly*, that, after the division of the nerves of that organ, a part of the nervous influence, previously transmitted from the brain, or spinal marrow, remains in the nerves below the division, and supports the degree of secretion that continues in these circumstances.

All this he appears to consider not merely as a plausible explanation, but as a *necessary consequence* of the fact just stated; and certainly, if it be not proved by this fact, I know of no other by which it is proved. But to me it appears, not only that he has not adduced, as he supposes, an *experimentum crucis* in favour of the doctrine of the dependence of secretion on

* *Anat. Gener.* T. 4, p. 604.

nerves, but that he has not been able to reconcile the phenomena even of his own experiments with that doctrine, without the aid of a second, and rather strained, hypothesis.

The other fact on which I would chiefly rely, and which seems to me to furnish nearly as complete an *instantia crucis* against Dr. Philip's notion of secretion, as his experiments did against Le Gallois's notion of the heart's action, is the simple fact of secretion, or the analogous function of nutrition, taking place in the animal body, notwithstanding the absence of the most material parts of the nervous system.

In answer to the argument drawn from this source, Dr. Philip observes, that if it proves any thing it proves too much, inasmuch as it would lead to the conclusion, that these parts of the nervous system are not necessary to the existence of the *sensorial power*, that is, of the mind. Now by proving too much, I can only understand, proving that to be true, which other facts prove to be false, or *vice versâ*; and I know of no facts to prove, that those faculties of the mind which have been found to exist where the nervous system has been *defective in its original formation*, are nevertheless necessarily connected with the very parts that have been wanting in these cases;—nor can I conceive any fact, which would set aside the evidence of this short piece of reasoning, that any function, which is performed in a single case where a particular organ does not exist, is not, in any case, *dependent* on (however much it may be influenced by) that particular organ. In fact, on this very ground, the author of the paper in the *Edinburgh Review*, which Dr. Philip has quoted, infers from the cases of diseased brain, which he relates, that the brain is probably not at all concerned in the changes which precede sensation; and adds: "We hesitate about drawing this conclusion, not from an opinion that more evidence on the subject is necessary, for we conceive that *one instance*, such as those last quoted, if it be admitted to be true, is as conclusive as a thousand;—but because we wish to see cases more minute in all their details, and observed with a view specially to this physiological inquiry, substituted for those which we already possess *."

* Vol. xxiv. p. 418.

Dr. Philip objects to the conclusions drawn from cases of *diseased* nervous system, on a different ground, and one which seems to me much stronger;—that the brain and nervous system in general, like other organs, may probably admit of a great degree of distention and compression,—(and I would add, may undergo great change of form and size by interstitial, and even by ulcerative absorption), without its functions being materially deranged. But the argument on which I would be understood chiefly to rely, is that drawn from cases, not of disease, but of *original defective formation*, in which we have no reason whatever to suppose that the portions of the nervous system, which are wanting, ever had existed. In the common case of the acephalous fœtus, (for example, in Mr. Lawrence's case, here referred to), the bones which form the top and sides of the skull are wanting as well as the brain, and the basis of the skull is covered with the common integuments; so that it can hardly be supposed, that any brain has ever existed; and the small knob, which terminated the spinal marrow in Mr. Lawrence's case, instead of being the substance into which the brain had contracted, was, in all probability, the only portion of brain that ever had existed. 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 medulla oblongata; perhaps not even with that; and I would extend the same conclusion to whatever other mental acts that child could be shewn to have performed, precisely on the same ground that I continue to regard that case as one of many which prove that secretion and nutrition may go on, independently of, very nearly, the whole of the brain.

But if the argument drawn from the case of this fœtus be objected to, on account of the knob at the end of the spinal marrow, the same objection cannot be urged against the following cases.

The case referred to by Le Gallois in the *Histoire de l'Académie Royale des Sciences* for the year 1711, is of itself, I conceive, sufficient to justify the strong language in which the conclusion drawn from it is there expressed. " M. Fauvel, Chirurgien, *a fait voir à l'Académie*, un fétus sans cervelle, ni cervelet, ni moëlle épinière, quoique très-bien conformé d'ailleurs. Il étoit venu à terme, avoit vécu deux heures, et donné quelques signes de sentiment, quand on lui a versé l'eau du Baptême sur la tête. Ce n'est pas la première fois que l'on a vû ce fait, dont on tire *une terrible objection* contre les esprits animaux, qui doivent s'engendrer dans le cerveau, ou tout au moins dans la moëlle de l'épine et que l'on croit communément si nécessaires à toute l'économie animale."—*Hist. de l'Acad., &c.* 1711. *p.* 33.

The other case referred to by Le Gallois is also stated, although briefly, in such a manner as to leave no room for doubt that it had been carefully examined.

" M. Mery a vû un fétus mâle, venu à terme, qui n'avoit ni cerveau, ni moëlle de l'épine, et qui a vécu vingt-une heures, et a pris quelque nourriture. *La dure et pie mère faisoient canal dans les vertèbres.*"—*Hist. de l'Acad., &c.* 1712. *p.* 51.

I shall only refer to three observations by our own contemporaries, which appear to me strongly to confirm the conclusion naturally drawn from the above.

The first is a case of which we have a short account by Mr. Lawrence, although he did not see the mal-formed child till it had been kept so long that several particulars could not be ascertained. He states, however, what his well-known accuracy would have prevented his stating, unless he had satisfied himself of the fact, that " it had neither brain nor spinal marrow; the whole of the spinal processes were deficient, and the place of the medulla spinalis supplied by a vascular membrane, like that which covers the basis cranii in acephalous children, united in the same way to the surrounding skin. The heart, lungs, and liver, were deficient; the ribs, short and imperfect, lay close to each other, and did not form a thoracic cavity; the face was mal-formed in many respects; the fingers and toes were under the usual number; *with these exceptions, the formation of the body, and the size of the limbs, were tolerably natural**."

* *Medico-Chirurgical Transactions*, vol. v. p. 168.

Next, in the case of the fœtus found in the abdomen of a boy, described by Mr. Young in the same work, " one of the most singular circumstances was the total absence of brain, of spinal marrow, and of the nerves of sense and voluntary motion; although a distinct plexus of nerves was seen just within the umbilicus, about the commencement of the intestines, to which numerous branches were distributed*." Yet it appears, from the very circumstantial statements there given, that in this fœtus, bone, cartilage, ligament, muscle, fat, membrane, synovia, and meconium, had been formed from the blood.

If any one should say, that in these instances, a nervous influence was generated in the nerves, which answered the purpose of that which used to be generated in the brain and spinal marrow, it will be observed first, that Dr. Philip, at least, must deny all force to this argument, because he has expressly stated, " We may as well, I conceive, suppose a bone, as a nerve, capable of preparing this influence†." His supposition, therefore, is, that galvanism is supplied from some source, hitherto unknown, which supplies the place of the nervous influence. To this I would answer, that our object just now is not to ascertain how the formation of these different products out of the blood is accomplished, but merely whether it can be accomplished without the help of an influence coming from the brain and spinal marrow; and, if this point be made out, I am sure that the evidence hitherto adduced for galvanism being at all concerned in the matter, must be allowed to be exceedingly defective.

But, *secondly*, there is one instance on record of a fœtus, in which the most material parts of the animal organization were found, and in which " there was neither brain, spinal marrow, nor nerves." This case was carefully examined, and is minutely described by the late Dr. Clarke in the *Philosophical Transactions* for 1793, (p. 154); and I beg leave particularly to refer to the observations he has there made upon it. At present I need

* *Medico-Chirurgical Transactions*, vol. v. p. 258.

† Page 262.

only subjoin a single sentence from these. "The whole of the actions of this monster must have been of the vascular system entirely; and these seem to have been capable of forming bone, skin, cellular substance, ligament, cartilage, intestines, *&c.*" p. 161.

I shall only say farther, in regard to the connexion of the nervous system with organs of secretion, that as we know perfectly, that injuries of the nervous system are very apt to affect the circulation in the small vessels of different parts of the body in which secretion takes place, and know likewise that secretion is very much under the command of sensations and emotions of the mind, we should have good reason to be surprised, if the division of the nerves supplying secreting organs were *not* to modify their action very considerably. Dr. Philip's explanation of the *suppression* of the mucous and watery secretions of the alimentary canal in Mr. Brodie's experiments with arsenic is, that this morbid secretion was prevented, because the irritation, which occasions it, was prevented by the division of the nerves. Now, if the morbid irritation of arsenic is prevented by division of the nerves, may not the natural and healthy irritation of food in the stomach be prevented by the same operation; and if the absence of irritation is a sufficient cause for the suppression of the secretion in the one case, may it not be considered a sufficient cause for the alteration of the secretion in the other?

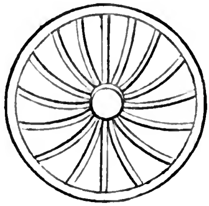
Art VI. *Account of an Optical Deception.*

[In a Letter to the Editor.]

Sir,

The optical phenomenon which I have here attempted to represent, may amuse those of your readers to whom it is new, and will probably serve to exercise the ingenuity of some in attempting its solution.

When a spoked wheel, such as that of a carriage, or the fly of an engine, is viewed in motion, through a series of vertical bars, the spokes assume the peculiar curvatures, respectively, which are represented in the annexed wood-cut



in a general manner. The upper and lower spokes, as they pass the vertical, are seen in their natural form, or perpendicular; but in all the rest a curvature will be observed, directed upwards on each side of the vertical, evanescent towards the upper one in particular, and attaining a maximum, as it appears to me, somewhere about that one on each side which occupies the middle of the two lowest quadrants.

It is indifferent for the production of this effect whether the wheel is running along a plane as it revolves, as in the case of a carriage driving through a street, and viewed through the ordinary iron railing, or whether, as in a fly wheel seen through a similar railing, generally erected in the stair-case of a steam engine, it merely revolves on its own axis. But I must observe that a certain relative distance between the two objects and the eye respectively is convenient; and that the effect is not very perceptible unless the velocity of the wheel exceeds a certain limit. With that velocity, the curvature of the spokes increases to a maximum, which is only limited by the total disappearance of the spokes in consequence of their rapidity.

The general principles on which this deception is to be explained will immediately occur to your mathematical readers, but a perfect demonstration will probably prove less easy than it appears at first sight.

I am, Sir, your obedient servant,

J. M.

London, Dec. 1, 1820.

ART. VII. *A Letter to the Editor of the Quarterly Journal, respecting certain Inaccuracies and Omissions in the* Rev. Mr. TODD's *Edition of* Johnson's *Dictionary.*

Sir, *London, Nov.* 9, 1820.

BEING attached to science, though not a scientific man, I am frequently obliged during the perusal of your Journal to revert to my dictionary for the explanation of certain terms with which I am not sufficiently familiar, though perhaps acquainted with their general import. Finding the definitions in the old editions of *Johnson* often meagre, and oftener incorrect, I purchased the new edition lately published by the Rev. H. Todd, in which, however, I was much dismayed by discovering very little improvement in the definition of scientific words, and more especially in those relating to chemistry; in too many instances erroneous definitions and meanings have been retained, merely, as it would seem, for want of a little trouble in referring to modern authorities; and on other occasions, the absurdities and jargon of old philosophers, are suffered to pollute and darken those pages which should have been adorned and enlightened by reference to modern discoveries. In the following pages I have in one column put down the word which I looked out, with its definition, as given in *Todd's Johnson;* and in another column the true meaning is given, with which I have been furnished by a scientific friend. From a numerous list I have selected a few specimens only, which, if you approve, and consider them correct and of any use to the possessor of the above Dictionary, I shall be happy to add to on another occasion.

I am, Sir,

Your constant Reader.

C.

ARSENIC, a ponderous mineral substance, volatile and uninflammable, *&c.*	ARSENIC, a volatile and highly inflammable metal; called *white arsenic* when in the state of oxide, and highly poisonous in all its combinations.
CHEMISTRY. An art whereby sensible bodies contained in vessels, or capable of being contained therein,	CHEMISTRY is the study of the effects of heat and mixture, with a view of discovering their general and

are so changed by means of certain instruments, and principally fire, that their several powers and virtues are thereby discovered with a view to philosophy or medicine.

Boerhaave.

subordinate laws, and of improving the useful arts. *Black.*

Most of the substances belonging to our globe are constantly undergoing alterations in sensible qualities, and one variety of matter becomes, as it were, transmuted into another. The object of chemical philosophy is to ascertain the causes of all such phenomena, whether natural or artificial, and to discover the laws by which they are governed. *Davy.*

COBALT, a marcasite, frequent in Saxony.

COBALT, a brittle metal; much used in the state of oxide, to give a blue colour to glass and porcelain.

FERMENTATION, a slow motion of the intestine particles of a mixt body arising usually from the operation of some active acid matter, which rarefies, exalts, and subtilizes the soft and sulphureous particles; as when leaven or yest rarefies, lightens, and ferments bread or wort. And this motion differs much from that usually called ebullition or effervescence, which is a violent boiling and struggling between an acid and an alcali when mixt together.

FERMENTATION is a term employed to signify the spontaneous changes which certain vegetable solutions undergo, placed under certain circumstances, and which terminate either in the production of an intoxicating liquor or of vinegar: the former termination constitutes *vinous*, the latter, *acetous* fermentation.

GALVANISM; the action of metallic substances.

GALVANISM, the electricity excited by the contact of certain dissimilar metallic substances.

GLASS. An artificial substance, made by infusing fixed salts and flint, or sand, together, with a vehement fire.

GLASS is any substance or mixture, earthy, saline, or metallic, brought by igneous fusion to the state of a hard, brittle, uniform mass, breaking with a conchoidal fracture passing into splintery, and with a high degree of lustre. Transparency is also a character of most glasses.

GOLD is the heaviest, the most dense, the most simple, the most ductile, and most fixed of all bodies: not to be injured either by air or fire, and seeming incorruptible. It

GOLD, a metal of a pure yellow colour, of a specific gravity exceeding that of all known substances, except platinum; very ductile and malleable; fusible at a bright red

is soluble by means of sea-salt, but is injured by no other salt, &c.	heat; volatile at a very high temperature; soluble in nitro-muriatic acid and in solution of chlorine, but not in the other acids.
GRANITE, a stone composed of separate and very large concretions rudely compacted together; and of great hardness, &c.	GRANITE, a rock essentially composed of quartz, feldspar, and mica, in grains or crystals of various magnitude. Its hardness and compactness vary extremely.
GYPSUM. The name of a class of fossils; the plaster stone; white lime; a kind of plaster.	GYPSUM, a compound of sulphuric acid and lime, or *native sulphate of lime*. When heated red hot it loses water of crystallization, and falls into a white powder called *plaster of Paris*.
IRON, a metal common to all parts of the world: though the lightest of all metals, except tin, it is considerably the hardest, and, when pure, naturally malleable, &c. It is the only known substance that is attracted by the loadstone. Iron has greater medicinal virtues than any of the other metals.	IRON, a metal found in most parts of the world: its specific gravity is 778 water being 100, so that it is not a heavy metal, though there are many lighter. It is one of the few metals which are magnetic. It is employed in medicine, though of much less importance than several other metals.
LIME, matter of which mortar is made.	LIME, one of the alcaline earths, lately shewn to be a metallic oxide: it is an essential ingredient in mortar and some other cements.
MANGANESE, is a name the glassmen use for many different substances that have the same effect in clearing the foul colour of their glass; it is properly an iron ore of a poorer sort.	MANGANESE, one of the metals. The term *Manganese* is often applied to the native black oxide of this metal, which is a commonly-occurring ore.
METAL, a firm, heavy, and hard substance, opaque, fusible, and malleable. The metals are six in number: some have added mercury, or quicksilver, to the number of the metals, but as it wants malleability, the criterion of the metals, it is more properly ranked among the semimetals.	METAL. The metals are characterized as a class, by a peculiar degree of brilliancy and opacity; they are conductors of electricity and of heat: they include the heaviest and lightest solids, and differ extremely in fusibility: some are brittle; others malleable and ductile. All the metals unite to oxygen, producing *metallic oxides*, which, combined with acids, form *metallic salts*. The metals at present known are forty-two in number.

MICA, in natural history, a genus of talcs.	MICA, a mineral composed of silica, alumina, potash, and oxide of iron, which occurs massive and crystallized: it is easily divisible into thin flexible and elastic laminæ, by which it is distinguished from talc, which is not elastic.
NITRE. [The dictionary here gives a very long and erroneous account of this salt, taken from HILL on *Fossils*.]	NITRE, a salt composed of nitric acid and potassa: *nitrate of potassa.*
OPAL. [Again an absurd definition from HILL'S *Materia Medica.*]	OPAL, a gem remarkable for the brilliant display of colours which it exhibits by reflected light. Its analysis affords 90 silex 10 water.
ORPIMENT, a foliaceous fossil, &c. Orpiment has been supposed to contain gold, and is found in mines of gold, &c.	ORPIMENT, a compound of sulphur and arsenic. It is lamellar in one direction, and of a yellow colour.
PHOSPHORUS, a chymical substance, which, exposed to the air, takes fire.	PHOSPHORUS, an undecompounded substance, when pure of the consistency and appearance of white wax; highly inflammable, and exhaling acid fumes of an alliaceous smell when exposed to air.
PORPHYRY, marble of a particular kind.	PORPHYRY, a very hard rock composed chiefly of feldspar: it takes a fine polish, and is sometimes used for ornamental purposes.
QUICKSILVER. [The dictionary gives a long, and for the most part erroneous detail, from HILL'S *Materia Medica.*]	QUICKSILVER, or Mercury, a metal liquid at common temperatures: at about 600° it boils, and at — 39° it freezes, becoming a ductile and malleable solid.
RESIN, the fat sulphureous parts of some vegetable, which is natural or procured by art, and will incorporate with oil or spirit, not an aqueous menstruum.	RESIN, a fusible and inflammable vegetable product, soluble in alcohol and ether, but insoluble in water.
SELENITE, a sort of fossil.	SELENITE, a variety of crystallized sulphate of lime, having a silky lustre: from σελήνη, the moon.

[The Editor has taken the liberty to abridge the above article, and has only retained a few of the "specimens" transmitted by his correspondent; he has also omitted the list of words not inserted in the dictionary, though he agrees with his correspondent respecting the carelessness shown in the omission of numerous words, in classes of which other individuals have been admitted. This is especially the case among minerals and metals.]

ART. VIII. *An Analysis of the Root of the Rheum Palmatum, or Rhubarb. By* W. T. BRANDE, S.R.S., &c.

1. THERE appears to have been no chemical investigation into the nature of rhubarb, if we except a few experiments upon it given in *Neumann's Chemistry*, where it is stated that a great portion of it is soluble in water, and that alcohol scarcely acts upon the residue. Neumann got from 480 grains 180 of alcoholic, and afterwards 170 of watery extract; and inversely 350 watery, and only 5 of alcoholic extract.

2. In the following experiments the finest Russian Rhubarb was used, free from decay, and distinctly streaked with white and red veins upon its cut surface: the former are chiefly gum, the latter contain the extractive and astringent principle, as may be shown by washing the surface with a dilute solution of iron, when the red streaks only are discoloured.

3. 100 grains of rhubarb, digested in eight ounces of boiling water, till cold, gave a yellow-brown infusion, which was tested by the following re-agents, and gave the annexed results:—

Acetate of lead	a copious yellow precipitate,
Sub-acetate of lead	a red precipitate,
Proto-muriate of tin	a copious yellow precipitate,
Proto-sulphate of iron....	an olive-green precipitate,
Nitric acid	a brown precipitate,
Oxalic acid	no effect,
Infusion of galls	no effect,
Solution of gelatine......	a copious brown precipitate.

4. It may be remarked, in respect to the above precipitates, that nearly the whole of the colouring matter was carried down

by the acetate of lead, so as to leave the supernatant liquor almost colourless. The precipitate by nitric acid was most copious when a few drops of acid were added to the concentrated infusion; it had the character of resinous matter, and was probably altered extractive. It was again dissolved by nitric acid added in excess. The precipitate by gelatine was most copious in the cold infusion; it scarcely formed in the hot infusion, and was re-dissolved by adding excess of the solution of isinglass.

5. One hundred grains of the bruised root were digested repeatedly in fresh portions of alcohol, (specific gravity 8.15,) till it exerted no further action, and came off perfectly colourless. The residue weighed, when dried at 212°, 55.8 grains, it was insipid, and when put into water it softened and gave by long digestion a viscid solution. When subsequently dried, it was found to have lost 31 grains, which, obtained by evaporation, had all the characters of gum; it was insoluble in alcohol and did not affect solution of iodine.

6. The alcoholic solution was of a deep yellow colour, and had a peculiarly nauseous taste; it was concentrated by distillation, and carefully evaporated to dryness: it left a brown residue weighing 36 grains, which being triturated with cold distilled water and poured upon a filter was resolved into 10 grains of insoluble resin and 26 grains of soluble matter.

7. The resin amounting to 10 grains was of a brown colour, gave out an aromatic odour when burned, and entirely dissolved in sulphuric ether.

8. The 26 grains of matter soluble in water, were obtained by evaporation, and afforded, upon being re-dissolved, a clear brown aqueous solution, which rendered solution of isinglass turbid, blackened solution of iron, and formed a copious precipitate with acetate of lead: this residue, therefore, was chiefly extract and tan.

9. From the above experiments it appears that 100 parts of rhubarb contain

Gum	31.	55.8 grains (5.)
Wood and insoluble residue	24.8	

Resin	10.	36.	(6.)
Extractive and tan	26.		
	91.8		
Loss	8.2		
	100.		

10. The loss in the above experiments may be ascribed to water, for upon drying rhubarb by long exposure to heat a little above 212°, the average loss of several samples was 10 *per cent.*

11. One hundred grains of rhubarb were put into a small retort and distilled by a heat gradually raised to redness. Water at first passed off, succeeded by a yellow vapour which condensed in the neck of the retort into a thick oil, and an acid liquor passed into the receiver, which blackened permuriate of iron. 41 grains of charcoal remained in the retort, which were reduced to powder, digested in dilute muriatic acid, washed, and dried at a red heat in a close vessel: they lost in this process 6.5 grains.

12. The muriatic solution on being saturated with pure ammonia let fall 2 grains of a substance having the characters of phosphate of lime; this was separated by filtration, and carbonate of ammonia added to the filtered liquor gave a further precipitate, which, collected and dried, was found to be 4.2 grains of carbonate of lime.

13. The results then of the destructive distillation of rhubarb may be stated as follows :—

Water	10.
Empyreumatic oil, gallic acid, and water formed	49.
Charcoal	34.5
Phosphate of lime	2.
Carbonate of lime	4.2
Loss	3
	100.0

14. To ascertain in what state the 4.2 grains of carbonate of

lime had existed in the root, before its destruction by fire, 100 grains of rhubarb were deprived of all soluble matter by the action of alcohol and water; these solutions were evaporated, and the residue submitted to a red heat in an open platinum crucible burned away, leaving no appretiable portion of earthy alcaline or saline matter, a small trace of common salt and of lime excepted. The insoluble woody fibre was digested in muriatic acid, and the solution saturated by ammonia: it gave a precipitate weighing 8.5 grains, from which, by the action of sulphuric acid, a portion of malic acid was separated. If this precipitate, therefore, be regarded as composed of phosphate and malate of lime, it would consist of

	grs.	
Phosphate of lime	2.	(12)
Malate of lime	6.5	
	8.5	

15. The component parts of rhubarb, therefore, would appear from the whole of the preceding data, to be as follow :—

Water	8.2	(10)
Gum	31.0	(5)
Resin	10.0	(7)
Extract tan and gallic acid	26.0	(8)
Phosphate of lime	2.	(12)
Malate of lime	6.5	(14)
Woody fibre	16.3	(5)
	100.0	

16. The very copious precipitate obtained by adding solution of acetate of lead to infusion of rhubarb, induced me to hope that some peculiar principle might be found in it, combined with the metallic oxide; I therefore collected a quantity of the compound, diffused it in water, and passed sulphuretted hydrogen through the mixture, which was afterwards boiled, filtered, and evaporated to dryness: a brown viscid substance, of a peculiar smell, and somewhat acid flavour, remained, which I was at

first inclined to regard as some distinct principle, but a few experiments soon taught me that it was merely a mixture of extractive matter, with a little sulphuric acid.

17. The activity of rhubarb, as a medicine, appears to reside entirely in those principles which are soluble in alcohol; the alcoholic extract was found a drastic purge, and the resin in its pure form also proved aperient; while the gum obtained from the residue insoluble in alcohol (5) was perfectly inert, and did not possess any peculiar medicinal qualities.

Art. IX. *Experiments and Remarks, illustrating the Influence of the Eighth Pair of Nervesover the Organs of Respiration and Digestion. By* S. D. Broughton, *Member of the Royal College of Surgeons, one of the Surgeons to the St. George's and St. James's Dispensary, and to His Majesty's Second Regiment of Life Guards.*

An attempt having been made to show that galvanism is equal to the influence of the nervous system over the important functions of respiration and digestion, in restoring these functions when they become interrupted or entirely suspended, by dividing the eighth pair of nerves in the neck of an animal; the Royal Society appointed some of its members, conversant with physiological pursuits, to investigate the grounds upon which this opinion is built. The report made relative to the point in question was, that no such power had been observed as that attributed to galvanism by the theory above mentioned. In order to satisfy myself as to the cause of such opposite conclusions, and, if possible, to ascertain the truth, I instituted a series of experiments, the result of which I am about to detail. In order, however, to bring this subject fairly into view, a short abstract of the experience and opinions of our ancestors and of our cotemporaries appears requisite.

So early as the time of Rufus and Galen, the attention of physicians was directed to a large nerve on each side of the windpipe, in a great proportion of animals, passing from the brain down the neck, and distributing branches to the thoracic and abdominal viscera; and experiments were occasionally made by fastening ligatures upon this nerve, to which the ancients gave the name of *par vagum* from its general distribution; and, it was also at times divided on each side of the neck, for the purpose of observing the effects of these nerves upon the organs they supply.

Similar experiments have been subsequently repeated to the present day, and varied agreeably to the views of those engaged in the pursuit. The ultimate fatality of the operation of dividing the eighth pair of nerves is generally noticed. All else that can be collected from Rufus and Galen is, that the animal loses its voice. The cause of death in animals submitted to this experiment was by many attributed to the disturbance and cessation of the heart's motions, directly produced by the division of the nerve. Willis and others maintained this notion, whilst some, who also repeated the experiment, were of a contrary opinion, observing, that if this explanation were true, animals could not live so long as they are known to do after the operation. While others attributed the death of animals to inanition, from their being unable to eat. Valsalva remarked frequent efforts to vomit, and subsequent derangement of the digestive functions, and that food filled the œsophagus, and the mouth was covered with foam tinged with blood. Hence he concluded that blood-vessels were ruptured by the efforts to vomit, and that the animals died of hæmorrhage. Some noticed similar appearances, but attributed them to congestion of the lungs, which stopping the circulation killed the animal. Haller also divided the par vagum, and noticed the dyspnœa consequent to the operation. He adds, that the digestive powers fail, and the contents of the stomach become putrid. Cruickshanks and others observed the congestion of the lungs, and supposed it to be the cause of death. Bichat frequently performed the experiment in order to discover the cause of its fatal tendency, with the view of illustrating the influence of the nerves on the thoracic and abdominal viscera. The respiration, he observes, becomes laborious, and continues so incessantly till the animal dies, and he refers the cause of death to the difficulty of breathing. Subsequent French physiologists referred the cause of death to a state of asphyxia following the division of the par vagum.

Dupuytren says, that the asphyxia is produced by the atmospheric air not being capable of uniting with the blood of the lungs, a phænomenon which he states to belong to life exclusively, and to depend on the influence of the brain. We know, however, that blood will mingle with atmospheric air out of its vessels, and become converted to a bright crimson. Another objection to his doctrine is, that, were asphyxia the direct cause of death, the animal ought to die as quickly as if drowned or strangled. Blanville agrees with Haller in attributing death to the impediment to the functions of digestion. Provençal attributed it to asphyxia, brought on by a diminution of oxygen gas, and a consequent deficient developement of carbonic acid, from the interrupted breathing, by which the temperature of the animal is reduced. In dogs he found the lungs red and gorged with blood, but in pigs and rabbits no such appearance was noticed. The asphyxia, therefore, he concludes, does not take place immediately, but at a certain point, and then increases till death.

Such has been the vague and unsatisfactory information upon this subject till Le Gallois engaged in these inquiries, to guide him in his treatise on the principles of life. He performed the experiment of dividing the eighth pair of nerves in numerous animals, varying in age and species, so as to ascertain the influence which such variations held over the different symptoms and phænomena that follow the division of the par vagum.

The result of his experiments is recorded with care and accuracy, and the following circumstances form the leading features of his experience which bear upon the present subject. His chief care was to ascertain the immediate cause of the death of animals after the division of the nerves; and finding with his predecessors that the *heart*, the *lungs*, and the *stomach*, were all disordered by the operation, he endeavoured to make out which was the seat of those symptoms that induced death; and he found it to exist in the *lungs*. His next object was to ascertain in what manner the lungs became affected, as they are found to be after the operation. From several experiments on young rabbits, pigs, cats, and dogs, Le Gallois was induced to come to this conclusion, that, in dividing the eighth pair of nerves, the *recurrents* being also cut off, the muscles moving the larynx become paralyzed, the *glottis* is closed, and the access of the air to the lungs is impeded. He divided the recurrents alone, and the same phænomena presented themselves as when the par vagum was divided. It is added, on the authority of the same author, that by dividing the recurrent on one side only he paralyzed one side of the larynx, and also that the aperture of the windpipe became entirely immoveable after dividing both the recurrents. In order to prove this point he cut a piece out of the windpipe; and immediately, he says, the breathing became free, and the dark colour of the arterial blood was converted to a bright crimson; and animals, on which this opening was practised, lived longer than those on which no opening of the trachea was made; and, he likewise observed, that the division of the nerves affected at the same time the *larynx*, the *heart*, the *lungs*, and the *alimentary canal*.

The combination of phænomena he attributes to the division of the par vagum, from its supplying such important viscera, and which may, therefore, be supposed to aggravate the symptoms accordingly.

He refers the loss of voice, in those animals of which Galen and others speak, to the same principle as that of the dyspnœa, *i.e.*, the cutting off of the communication between the brain and the organs of voice, by dividing the recurrents. He noticed, in performing these experiments upon different species of animals and of different ages, that the comparative severity of the dyspnœa differed one from the other. In very young animals it was more severe than in older, and one species seemed to be more violently affected than another; which he explains by remarking, that the aperture of the larynx is narrower in young animals than in adults, and that this opening varies in its dimensions in different kinds of animals compared with each other. The affection of the heart he found more difficult to determine, but he seems to think that the dyspnœa and the want of fullness in the arteries, from the imperfect oxygenation of the blood checking the circulation, is altogether sufficient to account for the heart's action after the division of the eighth pair of nerves. The affection of the lungs he refers to the severe dyspnœa occasioned by the paralysis of the larynx. He found them always in a state of greater or less congestion, and the bronchiæ full of fluid.

The state of the stomach, he observed, varied in its appearance in different animals, and even in the same species of animals; but, *he did not generally notice any indication of the digestive process being arrested.* Whatever might be the state of the stomach, he attributes it altogether to the disturbance of the functions of the respiratory organs. Le Gallois appears satisfied as to the *immediate cause* of death being entirely referable to the *lungs*, through which the circulation becomes stopped in three ways—1. By the diminution in the opening of the glottis.—2. By the congestion in the lungs.—3. By the extravasation of fluid into the bronchiæ; and that these effects vary according to the age, size, and species of the animal. Of thirty-one rabbits operated upon from one to forty days old, they died between six and eighteen hours and an half.

Majendie, in his elements of physiology, treating of respiration, remarks, that as the eighth pair are the only cerebral nerves which supply the substance of the lungs, many physiologists have been induced to divide them, and that this operation was frequent amongst the ancient physicians, but much less so with the moderns. And, in all cases, he says, the animals have not survived more than three or four days, and that this death has been attributed by different authors to a cessation of the heart's motion, a deficient digestion, inflammation of the lungs, and so on, as already noticed. He cites the experiments of his countrymen, and especially notices the loss of voice by the division of the recurvent nerve, and the same consequence from that of the par vagum; and also the observations of Le Gallois relative to the effect upon the glottis of dividing either or both of these nerves, which is that already quoted, of diminishing its aperture, and thereby obstructing the free entrance and exit of the air into the lungs, so as to produce difficulty of breathing, congestion, the general disturbance of the functions of respiration and digestion, and death. Majendie adopts the opinions of Le Gallois, and difficult as the explanation of the phænomena may appear at first sight, he thinks them readily explained, considering the manner in which the recurrent nerves are distributed to the muscles of the larynx. If, he says, the division of the nerves be made low down in the neck, the muscles which dilate the glottis become paralyzed, while the constrictor muscles which are supplied from the superior laryngial retain their action, and close the glottis more or less completely. In such cases as these, in which life is more protracted than usual, he supposes the division to fail in bringing on the closure of the glottis, and inducing the other phænomena gradually, instead of speedily, putting an end to life by the train of symptoms before noticed, creating difficulty of breathing, and a consequent failure of the proper oxygenation of the blood. Majendie says nothing of his own immediate experience as to the effects upon the digestive organs; but, as a collateral circumstance, the assumed fact of the diminution of the glottis, by dividing the par vagum, seems worthy of observation, especially as it does not appear to be very satisfactorily demonstrated.

Amongst our own countrymen the experiments of Dr. Haighton, though not conducted with any reference to my present objects, afford an interesting view of the effects upon animal life resulting from a division of the eighth pair of nerves; especially as his experiments were varied in the mode of performing them from any hitherto noticed. He observed considerable uneasiness about the organs of respiration and the stomach, with trembling of the whole body, that lasted from the time of dividing the nerves in the neck of a dog till its death, which in one instance occurred in eight hours, in another in two days, and in a third in three days. In all his experiments the voice of the animal was lost. No account of the appearance of the food in the stomach after death is given, nor whether the dogs which survived longer than usual had any interval of health. Dr. H. found that the division of one nerve only produced no symptoms whatever, and that the animal fed as usual and thrived. But when he, at subsequent periods, divided the other nerve, the usual symptoms came on in different degrees of severity; and the animal survived the operation longer than when both nerves were divided at once. Thus, after dividing one nerve alone, and its fellow on the third day, the dog died on the fourth day. After waiting nine days between the divisions, another dog lived thirteen days. Also, after an interval of six weeks between the divisions a dog recovered entirely. Nineteen months afterwards both nerves were again divided at once, and the animal died on the second day of the usual symptoms. Dr. H. remarked the stomach to be more or less affected in all cases; and that dog, which survived after the operation had been performed, allowing an interval of six weeks between the divisions of the nerve singly, was six months in recovering his condition, although he fed as usual after the expiration of one month.

Dr. H. accounts for a total loss of vital functions not directly following the division of the eighth pair of nerves, by observing that the stomach is supplied with branches also from the great sympathetic nerve, by which the functions of this viscus are sustained though imperfectly; while the recovery of his dog, which was allowed to live six weeks between the separation of each nerve, must be attributed, he says, either to an anastomosis of nervous filaments (similar to that of the arterial system), or to a re-production of nervous matter itself in the divided nerve; thus gradually restoring the perfect performance of the stomach's functions, previously impeded by cutting asunder the par vagum on each side at a certain interval between the division of each branch, so as to allow time for the requisite reparation in one before the other is divided. The foregoing statements serve to convey a general notion of the discoveries and different opinions relative to the use and influence of the par vagum in the animal œconomy; and, however varied the explanations may appear, they all tend to confirm one indisputable fact; that the eighth pair of nerves occupies so important a communication between the viscera, which it supplies, and the brain, that by dividing these nerves a very material derangement of the functions of life ensues, altogether sufficient to put a stop to their existence.

But, since the object of this inquiry is principally to ascertain the influence of the par vagum over digestion, and to settle (if possible) the point in dispute relative to the galvanic power with respect to the functions of the lungs and the stomach, it will now be necessary to advert to the experiments of Dr. Wilson Philip, who has pursued a physiological course, somewhat similar to that of Le Gallois, but has indeed gone far beyond him in speculative points in his theory of the analogy between the nervous influence and galvanism. It is needless to follow this author through all the minutiæ of his experiments, their end and object being to this effect, that, *according to his invariable experience*, after having divided the eighth pair of nerves on both sides of the neck of an animal, *the process of digestion ceases to be carried on;* and consequently, any food at the time in the stomach remains *unaltered* after the division of the nerves. It is also unequivocally stated that *the respiration* soon becomes disturbed, and *continues unceasingly so till the animal's death.* These facts being proved to his entire satisfaction, the author next asserts it to be also his *invariable experience*, that, by forming a proper galvanic circle, including the abdomen and chest, he succeeds in supplying the functions of digestion and respiration with the galvanic power, so as to effect *the restoration of these functions;* and accordingly, he says, *the animal will continue to digest his food, and to breathe freely, while the galvanic trough is kept in play; but that on its being stopped, digestion ceases again, and respiration becomes disturbed,* and *either may be restored at pleasure, till the animal is at length killed by galvanism always occurring in a few hours.* To the truth of these assertions several individuals have borne witness.

A doctrine, so novel and so strongly insisted on, excited in the Royal Society an opinion that the grounds upon which it rests ought to be carefully investigated; and accordingly, Mr. Brodie, and two others of its members, were deputed to practise that experiment which Dr. Wilson Philip states to have so uniformly afforded him the results which induced him and others to arrive at the conclusions above mentioned. At each trial two rabbits were fed with parsley after a long fast, and the eighth pair of nerves was divided in both instances in the neck; one of them continued unmolested, and the other was subjected to the galvanic influence. The conclusions to which these gentlemen came were, 1. That *the respiration did not appear in any case to be at all improved by the galvanic power;* and, 2. That *no sensible change was wrought upon the parsley in the stomach, so as to render it in any respect different in appearance to that of the other rabbit, which had not been galvanized*. This was accordingly the report which the Royal Society received.

At a subsequent period Mr. Brodie divided the eighth pair of nerves in a cat, close upon the stomach, below the branches which supply the lungs, so as to observe the effects upon digestion alone. No symptoms whatever were observed, and the functions of life appeared to continue naturally for a week and three days, at the end of which period the animal was killed. The nervous filaments were found to be completely divided, and digestion seemed to have continued. A repetition of this experiment produced the same result.

Considerable attention having been directed to this point in question, I was induced to institute a series of experiments, with the view of satisfying myself of the accuracy of the assertions of Dr. Wilson Philip and his supporters; in the first instance, by endeavouring to ascertain how far the process of digestion was affected by dividing the par vagum, and afterwards of observing the effects of galvanism, if it should appear that digestion was put a stop to by the division of the nerves. It must be premised that I found some embarrassment at first, in making myself acquainted with the peculiar appearances of the contents of the stomach, so as to discriminate nicely the different indications of digestion; but, repeated observations and comparisons soon led me to comprehend the several states of the food, and the stages of the digestive process. My experiments were conducted with careful observation, and witnessed by gentlemen, from time to time, whose professional talents and acquirements render them fully competent judges of the results.

Experiment 1.

The par vagum was divided in the neck of an healthy full grown rabbit, at half-past three P. M. No symptoms had been observed so late as eleven o'clock, but the animal was found dead in the morning. It had fed on oats prior, and parsley subsequent, to the operation. Dark spots were observed in the lungs, and the heart was full of coagulum. The urinary and gall bladders were full. The œsophagus was filled with bright-green chopped parsley, and the bronchiæ were full of mucus. The oats were partially digested, and the *parsley* was of a *brownish colour, very moist towards the cardiac portion of the stomach, and covered with a white semi-fluid layer of mucus resembling the usual appearance of chyme. The parsley lying uppermost approached more to the bright green of that in the gullet and was much less moist.* Some slight redness appeared on the surface of the stomach.

Experiment 2.

After fasting sixteen hours, a young rabbit was fed with parsley, and the par vagum was immediately divided as before, at three P.M. At half-past ten the animal was lying on its side, and drawing its breath with difficulty. In the morning it was found dead. The appearances resembled those of the first rabbit, excepting that there was less redness on its surface. *The parsley was very moist and brown.*

Experiment 3.

A young rabbit was fed with parsley after a fast of fourteen hours and an half, and the par vagum was immediately divided at half-past eight A.M. At half-past twelve it was couched upon its hind legs, and drawing its breath with difficulty. About four, the difficulty of breathing having increased gradually, the animal died. *The parsley in the stomach was moist and brown, with a covering of chyme, as before, about the cardiac portion of the stomach.*

The œsophagus was full of bright-green chopped parsley, and the other appearances did not differ from those of the two former rabbits in any material degree.

Experiment 4.

After a fast of sixteen hours, a young rabbit was fed with parsley, and the nerves were divided as before. Early in the day the breathing seemed to be slightly oppressed, but towards the evening it got better. The animal was found dead the next morning. There was no redness of the stomach, and the appearance of the food *resembled that of the last experiment*, and the œsophagus was full of bright-green chopped parsley.

Experiment 5.

The par vagum was divided on each side of the trachea of an old horse, at eight P.M. Before the division a piece of tape was passed loosely round each nerve so as to separate it from its connexions. Instantly the animal seemed very much distressed, and made urgent efforts to draw his breath and vomit. The nerves being divided, he staggered and fell down, rolled about, and continued to breathe with great difficulty, and in an hour he died. The inspirations were distinct and slow, and the expirations sudden and strong. At first the heart's action was increased, and latterly it became slow, feeble, and indistinct. The lungs were greatly turgescent. He died too soon for any remarks on the state of the food in the stomach.

Experiments 6 and 7.

After fasting sixteen hours two young rabbits were fed with parsley, and the nerves immediately divided as usual, at half-past two P.M. At eleven no difficulty of breathing had been apparent, but in the morning both were found dead. The parsley in both stomachs was *moist and brown*, but in one there was *more chyme than in the other.* In other respects no deviation from former appearances was observable.

Experiment 8.

After fasting sixteen hours, a puppy dog was fed with cold meat, and the par vagum was immediately divided on both sides, at three P.M. Touching the nerve with a forceps brought on efforts to vomit and oppressed respiration, and immediately on their being divided, these symptoms became aggravated, and some of the meat was thrown up. In a few minutes he appeared relieved, walked about, and at distant intervals only seemed to draw a long and slow inspiration, followed by a short expiration. On lapping some milk he vomited again, and was again relieved. Afterwards he lapped more milk, and this was followed by slighter efforts to vomit, which soon went off. In the evening he again lapped some milk, and threw it up directly afterwards. Subsequently he took more milk, but did not make any farther efforts to vomit. No difficulty of breathing occurred since the afternoon, and he ran about as well, to all appearance, as he was before the operation, and subsequently to the last vomiting he took a saucer full of milk. At nine the following morning he was observed to draw his breath with long and slow inspirations, at distinct intervals.

About twelve the breathing became still slower and more laborious, he lay gasping on his side, and died before one.

The stomach was entirely free from redness, and contained merely a little fluid resembling *whey.* Hence it appears, that the milk taken subsequently to the last vomiting had been *regularly separated by the digestive process, and the curd dissolved and passed away.* The quantity of fluid was scarcely a quarter of what the puppy had drunk. The lungs were studded with dark spots, and the bronchiæ were full of mucus.

Experiments 9 and 10.

Two young rabbits, having fasted twenty hours, were allowed to feed on parsley, and directly afterwards, at three P.M. the par vagum was divided as usual. Before six the breathing of one of them became affected, and it made efforts to vomit, and died before seven. At twelve at night the other was not apparently affected, but was found dead in the morning. It eat some parsley during the evening, which brought on efforts to vomit, but which went off again. In the first rabbit the œsophagus was full of bright-green parsley, and in the stomach it was of the usual *brownish tint and moist, with some little chyme.* In the second rabbit the parsley in the stomach was *much more moist and brown,* and it had *a far greater proportion of chyme attached to it.* The lungs in both were covered with dark-red spots, and the stomach of the second had more redness than that of the first.

EXPERIMENTS 11 and 12.

The nerves were divided as usual in two young rabbits after fasting twenty-four hours. They were then allowed to eat of parsley, which they did heartily, and ran about afterwards in a lively manner. Both were very soon attacked with efforts to vomit, but one more severely so than the other. The first rabbit breathed laboriously within half an hour after the operation, which was performed at three P.M., and it died before five. The second rabbit was affected a quarter of an hour later, and lived till eight. In the stomach of the first rabbit, which scarcely lived two hours, the parsley was moist, and *approaching* to the usual brown tint, but *much less so than in those which had survived a longer period;* and there was also a *very small proportion only of chyme.* In the pyloric portion of the stomach there was a small ball of dried and perfectly brown parsley, the remnant evidently of a former meal, probably after all its nutritious qualities had been dissolved; an appearance which I understand is usually observed in the stomachs of rabbits after the longest fasts. The contents of the second rabbit's stomach were *considerably more moistened, much browner, and enveloped with more chyme* than in the first rabbit, which scarcely lived two hours.

EXPERIMENT 13.

A young rabbit was kept without food sixteen hours, and then fed with parsley, and at two P.M. the nerves were divided as usual. For the purpose of observing whether the *diaphragm* acted freely, an incision was made into the abdomen, close to the ensiform cartilage, and it was found in full and regular play. At five o'clock the animal was lively, ran about, and seemed unaffected.

The diaphragm still acted freely, and there was no slowness of respiration. At different periods of the evening it had eaten of lettuce and parsley. After seven it was not seen till past eleven at night. It was then found couched on its hinder legs, and gasping for breath. On touching the diaphragm, the lungs were scarcely to be felt in motion. The rabbit was then killed by a blow on the occiput. The stomach contained parsley *converted to a brown colour; it was very moist, and surrounded with chyme*, while the œsophagus, as usual, was filled with *bright-green* chopped parsley.

Experiment 14.

An horse of fourteen years old, in good health, had the par vagum divided on each side of the windpipe, at eight o'clock P.M. No symptoms occurred immediately, as in the experiment of the former horse, from applying a piece of tape round each nerve, nor upon dividing them. Previous to the operation some hay was given. Shortly after the operation the horse appeared slightly oppressed in his breathing. He drank water, but refused to eat. In a few minutes he lay quietly down, and then breathed with long, slow, and distinct inspirations, and sudden and forcible expirations, but not accompanied with the noise and violence which the other horse exhibited. The respirations were no more than twelve in a minute. There was slight perspiration about the head and neck. The pulse rose to seventy-two, and it beat full and strong. Afterwards it rose to eighty, but it was now much weaker. Fœces were voided after the operation naturally. The respirations became slower. Before twelve, he endeavoured to eat, but it seemed to excite uneasiness. By midnight all symptoms had vanished. The whole of the next day the horse continued to all appearance perfectly well, eat his hay, but refused to drink, and walked about. The pulse and the respirations were natural during the day. Early the following morning he exhibited signs of uneasiness, and refused to eat or drink. The pulse became rapid and weak. The respirations were only six in a minute. In this state he died, at ten P.M., without any violent efforts to vomit, or any struggles to breathe, having survived the operation fifty hours, *twenty-four of which he passed entirely free from symptoms.* In the stomach was found some hay in a masticated state, *but considerably less than the horse had eaten. The duodenum was empty.* In the colon there was some hay, the remnant of former meals, and some of that eaten since the operation. There was *no distention of the stomach*, nor was there any redness of its surface.

The foregoing experiments are sufficient for the object which I had principally in view in making them. Since I brought these experiments to a close, Mr. Field junior (son of Mr. Field the veterinary surgeon, to whose liberal zeal in the cause of science I am indebted for the experiments on horses) has favoured me with the following accurate narration of the effects of dividing the eighth pair of nerves in a third horse. I much regret that I had not an opportunity of witnessing this experiment; but, as it is the only one which has come under my notice in which digestion appears to have been entirely arrested, I feel myself bound in candour to add it to those cited, in which the function of digestion was continued after the division of the par vagum.

The subject of this experiment was a bay gelding, twelve years old, and in good health. The nerves were divided as before, at seven P.M., and immediately the slow breathing, as in the last case, came on, with slight perspiration about the head and ears. The respirations shortly fell to six in a minute, and the pulse was at eighty. The animal eat some hay at intervals, and was not apparently much disturbed. The following morning the breathing was as the night before, and continued throughout the day equally slow, but free from violent efforts. In the evening the respirations fell to five in a minute, and the pulse was at ninety with its usual fullness. On the second morning after the operation the respirations were four in a minute, and the pulse at ninety as before. Some fæces had been passed naturally, and he had staled. He had also eaten of hay as usual. On the third morning the respirations were still at four, and the pulse rose to ninety-six, and at seven in the evening he remained breathing quietly, but slowly, as before. At ten minutes past seven he suddenly began to labour violently in drawing his breath, which was accompanied by a noise that attracted the attention of persons at some distance. The mouth was wide open, and the nostrils dilated. He exhibited great anxiety and debility, fell down and struggled violently, and presently rose again, still making urgent efforts to breathe.

In this state he was stuck, and the body immediately examined, having survived the operation about sixty hours and an half. Both nerves were found properly divided. The colon and cæcum were distended with fæces. The stomach contained twenty-six pounds and an half of masticated hay, without fluid, and it emitted a sour and fœtid odour. No inflammation was perceptible. The œsophagus was full up to the pharynx. A small portion of hay had found its way into the trachea, which, as the animal was masticating at the time that his cries brought persons to his assistance, may probably have slipped in at that period.

Experiment 15.

In a full-grown rabbit, after about eight-and-forty hours' fast, the nerves were divided as usual, and it then eat heartily of some parsley placed before it. It very soon appeared to be suffering some uneasiness, and drew its breath rather slowly. The operation was performed soon after eight A.M. In the evening the animal appeared to be quite well, and was moving about as usual, having been quiet during the day, and free from difficulty of breathing. No further symptoms were observed, but in the morning it was found dead. The œsophagus was full of chopped parsley. The stomach containe[d] a considerable quantity of dark brown-coloured parsley *with abundance of fluid, and chyme.* In the duodenum the mixture of bile was perceptible, *and a quantity of chyle was thus formed. Much yellowish-looking fluid also filled the lower intestines. There was not the smallest putrid or sour odour perceptible to any one.* No increased vascularity could be traced in the intestinal canal and stomach. The lungs were of a dark purple, and highly gorged with blood, and the heart was full of coagulum.

Having, by these experiments, distinctly ascertained *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 continued in every case, excepting the last horse, so long as the animal was otherwise in a condition to digest;* and, having also observed that *the approach of the dyspnœa is much varied as to time and degree of severity, and that it will* sometimes *go off entirely and be renewed again,* it is wholly unnecessary for me to pursue the *galvanic part* of the experiments.

With respect to the different periods at which dyspnœa comes on, it has been objected to my observations of their being indefinitely prolonged, that in animals covered with fur it is difficult to detect the movements of the respiratory organs, and thus, in rabbits, they may escape observation. But I am, nevertheless, disinclined to give up the opinion which I have formed on this point; because, although I am aware of the truth of the objection to a certain degree, yet, when I observe so much liveliness generally following the division of the par vagum in rabbits, and continuing for certain periods till the dyspnœa is apparent; and when I compare this appearance with the peculiar dullness and aversion to motion and taking food, together with the position of the head and body on the accession of the symptoms, I am induced to believe that my observations are not unfounded, and that the symptoms do not in all cases come on immediately, but often at differently-protracted periods from the operation. If, indeed, I could have entertained any doubt on the subject, that must have vanished, from the observations I made (in the thirteenth experiment) upon the action of the diaphragm, by means of an opening into the cavity of the belly, which afforded me a distinct view of its motion.

I must now observe, in relation to the appearances of *digestion*, that I have not drawn my conclusions without frequent comparative observations upon the stomachs of rabbits, (these being the animals which I principally employed), after feeding them with parsley, subsequently to some hours' fasting, and killing them at different periods. Such observations on rabbits simply destroyed without dividing the par vagum, compared with those on which division of the nerves had been practised, led me to observe that no difference was at all perceptible in the state of the food amongst the rabbits examined and compared, beyond the greater or less degree of progress which digestion had made in either; whilst in all which were operated on, the parsley eaten at the time of dividing the nerves chiefly occupied *the cardiac portion* of the stomach, was more or less *moist and brown*, and more or less enveloped with *chyme; appearances precisely coinciding with those of the rabbits not operated on.* Moreover, no difference could be detected in the *odour* emitted from the contents of the stomachs; and in an animal fed after a long fast, and then immediately killed, the parsley in the stomach exactly resembled that of the œsophagus in the rabbits operated on, being *free from a mixture of fluid, and of a bright green colour;* being, in fact, nothing else in appearance but simple chopped parsley, unacted upon to the smell as well as to the eye. If any doubts can exist as to the appearances I have described being any other than those which result from *digestion*, I appeal to the experiment on the *dog*, in which a small quantity of *whey* remained after he had drank a saucer full of *milk*, and the *curd had disappeared.* I may appeal also to the experiment of the *second horse*, in which the animal having eaten of hay freely during twenty-four hours of apparent absence of all symptoms, there was found *scarcely any hay* in the stomach. In addition to which it is to be observed that this horse, as well as the dog and the rabbits, passed fæces and urine naturally at different periods, after the division of the nerves.

From a general review of the testimony of former authorities, I cannot perceive that the conclusion which my experiments have brought me to, 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 true, that some authors notice the loss of power in the stomach to digest food after the division of the eighth pair of nerves, but nothing of the kind is mentioned by the majority of authors; many of them deny its occurrence, and none state it to be an immediate consequence. Le Gallois, who pursued the inquiry into the effects of dividing the par vagum to a very considerable extent, met with one instance only (in a pig) in which digestion ceased altogether.

I by no means mean to assert that the division of the par vagum does not affect digestion at all, or that it is continued as perfectly as before the division of the nerves. The symptoms continuing, the disturbance to digestion is eventually very great, sooner or later, till the general injury which the principal vital functions suffer, puts an end to the animal's life.

Though I am at a loss to account for Dr. Wilson Philip's assertion, that the function of digestion ceases immediately after the division of the eighth pair of nerves, and that it is a phænomenon from which he and his supporters know of no deviation whatever; yet I think it is very easy to reconcile all the contradictory statements elsewhere, from the earliest to the present day, when it is considered how much the phænomena, produced by the experiments under consideration, have differed in their time of coming on, their progress, and general character; variations dependent upon the different species, ages, and other circumstances of the animals employed, as well as, very possibly, on the part of the nerve divided.

But that the par vagum exclusively holds such absolute control over the secretions of the stomach, as to render it impossible for them to be produced after its communication with the brain and stomach is cut off, cannot surely be credited, considering the ample testimony which I have brought forward to the contrary; testimony not resting upon my own experience alone, but supported by that of the ablest physiologists of ancient and modern times. Referring, therefore, to this testimony, I am bound to believe, that it is perfectly erroneous to assert that digestion directly and invariably stops as soon as the eighth pair of nerves is completely divided. Consequently, the most rational inference appears to be, that animals are affected in *different degrees*, some, though very rarely, so as to be deprived of all power of digestion, while the generality continue to digest with more or less facility for certain periods.

Having thus put the question relative to the influence of the par vagum over the functions of the stomach in (I hope) a more satisfactory point of view than it has hitherto appeared in, and shown the fallacy of Dr. Wilson Philip's premises to his theory of the analogy between the galvanic and nervous powers, I shall conclude with some few suggestions relating to the manner in which it appears to me to be probable, that the symptoms following the division of the par vagum arise.

The lungs themselves are supplied by the eighth pair of nerves, whilst the stomach receives branches also from the great sympathetic nerve. All animals which have both lungs and a stomach are found to have the par vagum; but in those which are not furnished with lungs, I believe no such nerve is to be found. Hence the phænomena of dividing the par vagum are analogous to the indications of anatomy, which lead to a consideration of this nerve being solely destined to afford sensibility to the lungs, whilst at the same time it assists in preserving the due performance of the functions of the stomach; but in what precise degree it is difficult, if not perhaps impossible, to determine. The functions of the stomach are carried on in some of the lower animals without the par vagum. In two experiments by Mr. Brodie, wherein he divided the stomachic branches of the par vagum below its distribution through the lungs, the animal (a cat) lived and digested, to all appearance, as usual. In the experiments which I have cited, digestion seemed to have gone on as it ordinarily does, till the continuance of the distressing symptoms following the division of the nerves rendered the animals no longer capable of supporting the functions of life; and, in some cases, it appears that the food has remained in the stomach altogether undigested.

Hence, then, does not the opinion of Le Gallois appear to be correct; that the lungs are the primary seat of the striking effects observed after the division of the eighth pair of nerves, and that it is through this organ that the functions of the stomach suffer, and death is finally produced?

Mr. Brodie, in his lectures at the College of Surgeons, put this also in a very clear light, 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 nerve on both 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; it becomes dark, discoloured, and unfit for the proper secretions of the stomach, and by degrees ceases to circulate altogether; the lungs become collapsed and turgid, and the heart loaded with coagulum. Such are the appearances usually observed, and which in their progress and their influence over the functions of the stomach exert themselves in different degrees, varying in time and severity according to the species, age, and other circumstances of the animal; and (as is strongly demonstrated in the cases of the dog and the second horse in experiments 8 and 14), a suspension of all symptoms will sometimes occur, during which no impediment is put to digestion, or any of the functions of life.

It is to the retardation and the interval of a suspension of symptoms that I am induced to ascribe the continuance of digestion after the nerves have been divided. In the case of the rabbits, time was allowed for a certain progress in digestion to be made before the symptoms assumed a severe character; but, in the case of the last horse, (communicated by Mr. Field), wherein the symptoms came on immediately, and continued for about sixty hours, no opportunity seemed to be allowed for digestion to be performed. These circumstances, I think, tend to confirm the opinion of Le Gallois, that it is through the impediment put to respiration and the proper circulation of the blood, that the organs of digestion suffer; and in proportion to the affection of the lungs is the distress which evidently oppresses the stomach.

With regard to the efforts to vomit, in those animals which are incapable of doing so effectually, and the actual vomiting of others, which soon, and often immediately occur, it may be that this is an immediate affection of the stomach, upon the same principle as that of concussion of the brain, or the motion of a ship at sea exciting nausea and vomiting. The parsley found in the œsophagus must be the result of ineffectual efforts to throw it off the stomach, and, being that which lies uppermost, and unaltered by digestion, forms a striking contrast to what remains in the stomach, and which has undergone more or less alteration.

The expedient of Le Gallois for restoring the free access of the air to the lungs, by cutting out a piece of the trachea, may be supposed to give temporary relief to the symptoms, if the obstruction depend, as he thinks, upon contraction of the larynx in consequence of cutting off the recurrent nerve. But, if the par vagum be considered as a nerve of sensation to the lungs, then it cannot be conceived that this expedient is calculated to restore their functions; since, if they be robbed of their sensibility, they cannot act as they do naturally by the stimulus of the atmospheric air, which may be let in by an opening of the trachea, it is true, but in vain; for it cannot circulate when the lungs are unable to continue their action.

Having noticed this explanation of Le Gallois, I determined to put it to the test of experiment. Mr. Field, to whom I have before acknowledged my obligations for his professional assistance, afforded me an opportunity of observing the effects of taking out a piece of the trachea after dividing the par vagum in the centre of the neck on each side. The subject of the experiment was an healthy pony of six years old. The animal was seized with violent efforts to breathe directly following the operation, and the excision of a large portion of the trachea afforded no immediate intermission of this difficulty in respiring. The symptoms went off for a time, but returned during the day at intervals with the same violence, and the pony died in a state of exhaustion about seven hours after the division of the nerves.

I subsequently performed the same experiment on a full grown rabbit, which was attacked with slow respiration very soon afterwards. The symptoms returned after having apparently subsided, and the animal was found dead in the morning after the operation. In neither of these cases was there any demonstration of relief from an excision of the trachea, nor any prolongation of life beyond the usual periods. I have been informed by Mr. Brodie, that he also has tried the effects of making an artificial opening in the trachea with no better success.

I am inclined to think, that the most probable mode of accounting for the retardation and suspension of symptoms which have been noticed, is the various degrees of susceptibility in different animals and at different periods of life; and that when the symptoms are not immediately apparent, or having come on go off again, the nervous influence supplied before the division of the nerves has been sufficient to avert for a time the consequences of its farther supply being cut off, or to overcome the immediate shock which some animals experience, and allow of an interval of natural respiration and a due circulation of blood to be carried on.

Peculiar states of the constitution, and disease, as well as age *&c.*, no doubt, have also their share in modifying the manner in which the division of the par vagum affects different animals; and hence it appears to be presumable that this inquiry into the influence of the eighth pair of nerves over the organs of respiration and digestion, hitherto conducted for objects solely physiological, may, if properly pursued, open to medical practice a field of pathological investigation, calculated to throw considerable light on some affections of the thoracic and abdominal viscera, at present remotely and imperfectly understood.

London, October, 1820.

Art. X. *Account of the Method of preparing a Black Resinous Varnish, used at Silhet, in Bengal.*

Shields made at Silhet are noted throughout India for the lustre and durability of the black varnish with which they are covered; Silhet shields constitute therefore no inconsiderable article of traffic, being in request among natives who carry arms, and retain the ancient predilection for the scimitar and buckler.

The varnish is composed of the expressed juice of the marking nut, *Semecarpus Anacardium,* and that of another kindred fruit, *Holigarna Longifolia.*

The shell of the *Semecarpus Anacardium* contains between its integuments numerous cells, filled with a black, acrid, resinous juice, which likewise is found, though less abundantly, in the wood of the tree. It is commonly employed as an indelible ink, to mark all sorts of cotton cloth. The colour is fixed with quick lime.

The cortical part of the fruit of *Holigarna Longifolia* similarly contains between its laminæ numerous cells, filled with a black, thick, acrid fluid. The natives of Malabar, in which country, as well as in the eastern parts of Bengal, the tree is indigenous, extract by incision its very acrid juice, with which they varnish targets.

Both are combined for the same purpose by the artisans of Silhet.

The juices are resinous, being soluble in alcohol, and not at all so in water. They may be dissolved in fixed oils, and in dilute alcali.

To prepare the varnish according to the method practised in Silhet, the nuts of the *Semecarpus Anacardium* and berries of the *Holigarna Longifolia,* having been steeped for a month in clear water, are cut transversely and pressed in a mill. The expressed juice of each is kept for several months, taking off the scum from time to time. Afterwards the liquor is decanted, and two parts of the one are added to one part of the other, to be used as varnish. Other proportions of ingredients are sometimes employed; but in all, the resinous juice of the *Semecarpus* predominates. The varnish is laid on like paint, and when dry, is polished by rubbing it with an agate or smooth pebble.

A gentleman, now deceased, who resided long in Silhet, and who communicated the process to me, entertained an expectation that this varnish, or a similar combination of the same ingredients, might be usefully employed for marine purposes. He had found a varnished target untouched by the white ant, *(termes)* in circumstances where every thing contiguous had been demolished by that destructive insect. In consequence of that remark, it was tried by him as a sheathing of vessels used in the navigation of the Ganges. His notion was that the resinous varnish would effectually defend the planks of a vessel, keeping the wood dry and water-tight, deterring attacks of the borer or timber-worm, and preventing adhesion of barnacles. The trials made of it were attended with a more satisfactory result in the internal navigation of the river, than in the tide's way. On a sea voyage it was found, that barnacles fixed themselves on the varnished plank.

Though not adapted to marine uses, the resinous coating in question might admit of other useful applications. In any case it may be satisfactory to know the composition of a most durable varnish, which takes so very high a polish, as is seen upon Indian targets. With this impression I take leave to communicate it for publication, if judged proper.

H. T. C.

Art. XI. *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.R.I., M.G.S., *&c.*

Upon the trial of the action brought by Messrs. Severn, King, and Co., against the Imperial Insurance Company, before Lord

Chief Justice Dallas, and a special Jury, in the Court of Common Pleas, at Guildhall, in the month of April last, the Lord Chief Justice, in summing up, made the following observations: "We have been now employed in the examination during two days, of a great number of the most intelligent persons, that this country or Europe can produce. I am myself, more or less, acquainted with all the writings of every one of these gentlemen: from this I know their information, I know their talents; and whether my time has been well or ill employed, I will not say, but I am proud to acknowledge, that from their labours, I have received at times a considerable degree of pleasure; but I must add, that these two days, thus employed, are not days of triumph, but days of humiliation for science; for when I find that their science ends in this degree of uncertainty and doubt, and when I observe they are drawn up in martial and hostile array against each other, how is it possible for me to form, at a moment, an opinion upon such contradictory evidence? You will not, therefore, expect any opinion upon this part of the case from me; I can form none: volumes have been spoken upon it, and I foresee, without being blessed with the spirit of prophecy, that volumes will be written upon it, and so they ought, for the elucidation of science, and the enlightening of mankind."

In consequence of the impression which these observations have made upon the public, and of the active part which I have been called upon to take in the investigation of the question at issue between the parties, I have felt myself bound to attempt to explain to the chemical world, the reasons upon which my own opinions were founded, and also to endeavour to account for the origin of those conclusions, which were adopted by the practical chemists who appeared in behalf of the defendants.

The plaintiffs in these actions, Messrs. Severn, King, and Company, are very considerable sugar-refiners, carrying on their business at the works they possess in the neighbourhood of Whitechapel. These gentlemen who have been proprietors of this concern for many years past, effected at different times several insurances on their buildings and stock in trade, with sundry insurance-offices, for various sums, amounting together to 70,000*l.*, and have paid to these offices nearly 1,000*l.* per annum, to protect themselves from the peril of loss by fire.

For several years the plaintiffs carried on their business by the usual mode of boiling the sugar in large copper pans, fixed in brick-work, and exposed to the action of naked fires. In the month of August, however, in the year 1819, they erected an apparatus for boiling sugar by means of heated oil, and one of their pans was said to be thus worked, with great satisfaction and profit, for nearly three months. In the month of November, 1819, a fire broke out upon the premises, by which the greater part of the buildings was destroyed, and a loss sustained of more than 80,000*l.* The cause of the fire has not yet been ascertained.

There has been no dispute with respect to the extent of the loss—nor has any imputation been attempted to be cast upon the conduct of the plaintiffs; but the defendants conceived, that on certain grounds they were not entitled to make good this loss, and especially on the ground of the plaintiffs having adopted the new mode of boiling their sugar.

The apparatus employed in this process was invented by Mr. Daniel Wilson, civil-engineer. It consisted of a common sugar-pan, a copper worm, a cast-iron forcing pump, and an oblong wrought-iron vessel, similar in form to a steam-engine boiler, fixed in brick-work, and heated by a common close fire-place. Into this latter vessel about one hundred gallons of whale-oil was put, and when the oil became heated to about 360° of Fahrenheit, a part of it was driven by means of the pump into a worm or coil of copper pipe, fixed within the pan containing the sugar intended to be refined; and when it had passed entirely through the coil, it returned into the oil-vessel by a different pipe of communication. Thus, so long as the pump continued working, did the heated oil continue to circulate through the worm-pipe immersed in the sugar. The consequence was, that the oil being heated to 340 or 360 degrees, and the sugar boiling at a temperature of 240 degrees, the latter might be kept in a state of constant ebullition without danger or difficulty.

For the sake of avoiding any misapprehension respecting the construction or use of this apparatus, an engraving of it is attached to these observations, by which the use of each part will appear, and the advantages will be obvious, which must result from the employment of such an apparatus in boiling sugar or other substances, liable to be injured when exposed in metallic vessels to the direct operation of the fire.

It is observable, that in this process there is no fire underneath the sugar-pan, but at a short distance there is a fire-place, and over that a wrought-iron vessel containing the whale-oil, which does not boil at a temperature much below 650° or 700°. In the top of the oil-vessel a thermometer is fixed, as shewn in the drawing, the tube of which is sufficiently long to allow the mercurial bulb to sink beneath the surface of the oil. It is of importance to remark, that this thermometer is graduated only to 450°; consequently, if the oil were to acquire a temperature much beyond 450°, the thermometer would burst, and the circumstance of the increased temperature would in a moment be discovered, before any mischief could possibly arise from the accident.

In using this apparatus, the pump is set to work as soon as the oil attains a temperature of 350°; and thus a portion of the oil is forced from the iron vessel into the tube G, whence it rushes through the coil of pipe lying within the copper pan; and from the copper pan it returns again and again, like the circulation of the blood, into the iron vessel. The heated oil passing in this way through the fluid sugar, boils the syrup, and that at a temperature very far below the boiling point of fixed oil, so that there appears to be no risk in the process.

However, when the conflagration had broken out upon the premises, and there was no clue to the origin of the fire, it was suggested by some of the Insurance Companies, that it might have been occasioned by the heated oil; although they did not at that time venture to say how. Yet as the payment of the sums insured might have been avoided, if it could have been proved, that the actual risk was increased by the introduction of the oil process, much was imagined for the sake of supporting that supposition.

It was said, that the copper pipes, through which the oil circulated, might have burst, and the heated oil have set fire to the sugar, which is well known to be a very combustible substance. That an inflammable vapour might have arisen from the oil when at that high temperature, and been productive of mischief. That a permanently elastic inflammable gas might have been generated by the action of an intense fire upon the oil, and that this might have escaped from the orifice of the safety-pipe*, and spread itself throughout the body of the building—for though its specific gravity would naturally direct it upwards, it might by a sudden gust of wind have been driven downwards, and then coming in contact with a gas-lamp, which was burning in the room below, it might have become ignited, and the buildings thus have been destroyed. And in order to strengthen the opinion, that the fire might have been occasioned by the sudden ignition of inflammable gas, it was said, that the nature of the oil put into the large iron vessel might have been changed by the repeated and long-continued application of heat;—for though it might not give out any considerable quantity of gas at a certain temperature now, it might after a lapse of time be so far changed in its quality and character, that the same effects would be produced at a temperature of 300° or 400°, which on *fresh* oil, would require a heat of 600° or 700°. It was said also, that the oil-vessel might have leaked, and the oil have dropped into the fire, and thus have occasioned this tremendous conflagration.

These various surmises and conjectures were, however, very completely answered by Mr. Serjeant Copley, his Majesty's Solicitor General; and I trust, that in the following pages they will be entirely refuted. To do this I need only examine the

* This pipe rose directly from the top of the oil-vessel; it was 18 feet long, and terminated within a brick flue, called the steam vent; and this latter communicated with a very lofty brick chimney.

evidence of the respective chemists who were employed for the plaintiffs and for the defendants, and such other persons as spoke to chemical facts, and even such parts of their evidence only, as may seem to require explanation or comment.

In pursuance of this plan, and with a copy of Mr. Gurney's printed Report of the Trial before me, I turn first to the evidence for the plaintiffs. The first person called in their behalf, who was competent to give any valuable chemical opinion on the subject, was Mr. Daniel Wilson, Civil Engineer. This gentleman was examined by the Solicitor General, and deposed to the following facts:—"That he had devoted his whole attention to the refining of sugar for the last six years; that he is the inventor and patentee of the new process of boiling by means of heated oil; that he considers this process to be much less dangerous than the common one, in which the sugar pan is exposed to the operation of a direct fire; that sugar is decomposed at a temperature of 344°; and will then give out inflammable gas; that the working point to which the oil was heated for this process never exceeded 360°; and that had it ever been heated to 440°, the thermometer would have burst, and discovered the circumstance to the workmen; that the boiling point of sugar is about 245°, and the boiling point of oil above 600°; that the oil emits no inflammable gas at a lower temperature than 600°; that several hours of hard firing would have been required to heve raised the oil to that temperature; that if such a fire were made under a sugar pan in the ordinary process, the sugar would take fire, and be the means of destroying the building; that if inflammable gas had been generated in the oil vessel, it would have passed off by the safety tube, and gone up the steam vent into the open air; that the utmost quantity of inflammable gas which the whole of the oil could have produced, was only 8,500 cubical feet, whereas the quantity necessary to render the atmosphere of the sugar-house explosive, was at least 68,000 cubic feet; that if gas had been produced, the vent pipe in the oil vessel was of sufficient size to have carried it all off, and ten times the quantity; that he had repeatedly heated a parcel of whale oil in the course of two years without its undergoing any alteration except becoming darker coloured, and in a trifling degree thickened ; that when the rubbish was cleared away after the fire, the oil vessel was found standing in its original position, the four-inch wall which was on one side of it was as perpendicular as at first, and there was no fissure or rent in the oil-vessel; that the copper pipes could not possibly have burst from the action of the pump, as they were larger in diameter than the valves, and therefore there could be no pressure in them." On being cross-examined by Mr. Scarlett, he stated, that "he had taken out three patents for boiling sugar, and had formerly been engaged in chemical manufactures; that whale-oil does not boil under 640°, or thereabouts; that the thermometer was fixed in the oil vessel, with its bulb about three inches beneath the surface of the oil; that when the bulb of a thermometer is broken, a portion of the mercury usually rises to the top of the tube; and although the fire was burning for a week in the ruins, the oil-vessel was dug out in a perfect state, so much so that water was afterwards boiled in it without its undergoing any repair."

On the foregoing evidence, I am desirous of making a few observations. Mr. Wilson stated "that sugar becomes decomposed when heated to a temperature of 344°, and then gives out inflammable gas." In the first experiment which I made upon sugar, heated in a copper pan, the gas arising from it would not ignite by a lighted match when the sugar had acquired the heat of 370°, but at that temperature the sugar itself burnt with a very strong and permanent flame. In a subsequent experiment, made also in a copper pan, I did not find the gas inflammable until the sugar was heated to 386°, and the sugar did not take fire of itself until the thermometer stood at 398°; and in an experiment made in a small glass retort, the gas did not ignite until the sugar was heated to 470°. This is a remarkable fact, but I have found in other cases, that sugar may be decomposed at a lower temperature in copper than in glass.

Mr. Wilson also stated that "when whale oil is heated to 600°, it gives out inflammable gas, but not under that temperature." When Messrs. Severn's first action was tried, my opinion very nearly coincided with that of Mr. Wilson, but subsequent experiments have convinced me that it was oil vapour which we both mistook for oil gas, and that such oil must be heated to a degree which our instruments will not measure, before the oil will be decomposed, or a single bubble of inflammable gas liberated.

When Mr. Wilson was examined respecting the repairing of the oil-vessel a few days before the fire, he was asked if he took out the oil for that purpose; and if, by being so long heated, it had diminished in quantity? To this he answered, "I could observe no decrease but what could be accounted for by the trifling leakage of the vessel." Surely, more decisive evidence of the oil not having been heated improperly could not have been desired; for if it had been heated sufficiently to have produced decomposition, one part would have been converted into charcoal, and another part would have passed off in gas; the consequence of which must have been, a considerable diminution of the quantity of the oil.

When asked, what was the appearance of the inside of the retort, or oil-vessel? he answered, "When we took off the man-hole there was a quantity of carbonaceous matter on the bottom." "What did that indicate?" "That could only result from the gradual *distillation* of the oil." Here I apprehend the answer should have been, from the *decomposition* of the oil, occasioned by the conflagration of the buildings; because I conceive that charcoal cannot be produced from fixed oil without a portion of the oil undergoing decomposition.

On Mr. Wilson's evidence respecting the thermometer, it may be remarked, that when the bulb of a thermometer breaks, the pressure of the atmosphere, acting upon the column of mercury in the stem, drives it up to the top, provided the vacuum has been perfect, which is always the case in well-made thermometers; but, as Mr. Scarlett dwelt much upon this circumstance, and endeavoured to turn it to the advantage of the defendants, it may be necessary to observe that the mercury falling out, or remaining suspended in the tube, when the bulb is broken, depends entirely upon the size of its bore;

for if that is wide the gravity of the mercury will overcome the capillary attraction of the tube, and the fluid metal will oscillate freely, as it does in a barometer.

Mr. James Harris, a sugar-refiner of Liverpool, was the next evidence of any consequence that was called. This gentleman deposed that he had been a sugar-refiner fourteen years; that he had employed two processes;—the common process of boiling with a fire under the sugar-pan, and that of boiling by means of oil heated in an open vessel; that the danger of fire was lessened by the adoption of the oil process, because the sugar might at any time have been raised out of the heated oil in an instant; that there is great danger in boiling sugar by the common process, as it is apt to boil to excess, until the sugar becomes ignited by the heat below; that, in consequence of this danger he erected three sugar-pans, which he boiled by means of melted tallow, and afterwards by heated oil; that in six months he abandoned both these processes in consequence of the disagreeable smell which they occasioned, and reverted to the ancient method of boiling over a naked fire; but that he thought he should not continue that process much longer, but adopt the new one invented by Mr. Wilson.

Observations.—This gentleman, in describing the process which he employed, stated it to be conducted in an open pan, like the one in common use, but immersed in another pan containing melted tallow, or heated oil. Having seen a model of this apparatus, the nature of it will, perhaps, be more generally understood, if I add, that the vessel containing the oil or tallow, was fixed in brick-work, over a common close fire-place, well contrived for heating the said vessel; that the sugar to be refined was put into a copper pan, suspended by three strong chains, so as to allow it to sink sufficiently within the heated oil; that the upper rings of these chains were hung upon the short arm of a powerful lever, which was permanently fixed over the pans above-mentioned; and that, by this arrangement, the pan of sugar, in case of any emergency, might have been instantly drawn out of the heated medium without any difficulty. Mr. Harris having stated, that he was induced to give up the oil process in consequence of the very fetid smell which was occasioned by the repeated heating of the oil or tallow in an open vessel; he told me in a conversation which I afterwards had with him, that the vapour arising from these heated substances, also spread itself through the sugar-house and deposited something upon the fine goods which injured their colour, and did considerable mischief.

Anthony Robinson, Esq. examined. This gentleman stated that he had been a sugar-refiner between twenty and thirty years; that the common mode of refining sugar, is attended with considerable danger, and requires *incessant vigilance;* that Mr. Wilson's process requires infinitely less vigilant attention to keep it right than the old mode; that he had known many instances of sugar-houses being burnt down, but could never yet discover the cause of their being burnt.

Observations.—This gentleman, who is one of the oldest sugar-refiners in the neighbourhood of London, cannot certainly be called a *chemical* witness, but, as his testimony goes to prove the great combustibility of sugar and the excessive danger which there usually is in the process of sugar-refining, I was desirous of giving a short abstract of his evidence, especially as he is a person of acknowledged talent and well known in the literary world. At the close of this gentleman's examination, Lord Chief Justice Dallas observed that fires are sometimes occasioned by spontaneous combustion; and that the loss of the Ajax, many years ago, was supposed to be occasioned by a fire of that kind. His lordship could not easily have found a more remarkable instance of spontaneous burning than this, the particulars of which are worth relating: His Majesty's ship Ajax, which was thus destroyed, was a man-of-war of 74 guns, stationed in the Mediterranean. It had taken on board a large quantity of pyritous coal, and to this circumstance the fire had been attributed. In some districts of the North of England, martial pyrites are found mixed with the coal in such abundance as to render the coal absolutely unsaleable; but persons are employed to pick out these pyrites and remove them to a proper situation for the manufacture of green vitriol. This species of coal, when in contact with water, is apt to be decomposed, or rather to decompose the water, and thus burn spontaneously. An account of several other cases of spontaneous combustion may be seen in my *Chemical Essays*, vol. i., pp. 222—231.

Bryan Donkin, Esq., *Chairman of the Committee of Mechanics at the Society of Arts,* was next called.

This gentleman stated that he considered Mr. Wilson's process to be much safer than the old one; that fixed oil emits neither inflammable vapour nor gas till it has arrived at a temperature far beyond that at which sugar will take fire; that the lead-pipe arising from the oil-vessel would not have been melted by the oil vapour, even if it had been heated to more than 600°, and that the boiling of sugar in the ordinary mode is attended with moredanger than the boiling of it by means of the oil process.

Observations.—In answer to a question from Mr. Stephen, this gentleman replied, "I rather think no permanent gas would be given out under 600°." When Mr. Donkin spoke of permanent gas, I conclude he meant to be understood of permanently *inflammable* gas; because carbonic acid, which is a permanent gas, is given out from fish-oil at a temperature much below 400°. It is necessary, however, to remark, that at the time of the trial neither Mr. Donkin, nor any of the chemists employed by the plaintiffs or by the defendants, seem to have made any experiments to ascertain the means by which a fixed oil might be rendered capable of giving out inflammable gas. Since the trial, I have made an experiment which convinces me, that fish-oil must be submitted to a heat sufficient to decompose it, before a particle of inflammable gas can be procured from it, and that a portion of charcoal is always the result of the operation. It having been asserted that the fire might have been occasioned by the melting of the lead pipe in the oil vessel, Mr. Donkin gave it in evidence, that he had heated oil to more than 600° in a close vessel that had a leaden pipe inserted in it, similar to that in the vessel at the sugar-house; and that when he inflamed the vapour issuing from the end of it, it continued burning for half an hour, and the pipe was not hurt. Here I am desirous of remarking, that since the trial, I heated a quantity of train oil in a Papin's digester to a temperature exceeding 700°; and having inflamed the vapour which issued from the orifice of a piece of half-inch leaden pipe, fixed in the cover of the digester, and which rose only nine inches above it, the vapour continued to burn with an intense flame for a very considerable time, without the lead being melted, or the pipe sustaining any injury, although the vessel stood the whole time in the midst of a vehement fire which was continually urged by the blast of a powerful bellows.

Mr. Samuel Parkes was next called, and examined by Mr. Solicitor-general.

As I shall have occasion to give my opinion of the several circumstances of the case in various parts of this paper, I shall render the account of my own evidence as short as possible.

In answer to a variety of questions put to me by the counsel, I stated that I had examined the model * of the new apparatus with great care, and had no hesitation in saying that I considered its employment in refining sugar to be attended with less danger than the process of boiling over a naked fire; that I was in court when Mr. Wilson gave his evidence, and that I agreed with him in every thing he said, except as to the † tem-

* This model, which was prepared soon after the fire, by the order of Messrs. Severn, King, and Co., was proved on the trial to be an exact representation of the whole apparatus as it existed at the time of the accident.

† Mr. Wilson and myself were both mistaken as to the temperature at which an inflammable gas is given out by heated whale oil. The expe-

perature, at which an inflammable gas is given out from oil that had been heated repeatedly: that having mixed fifteen ounces of sugar with five ounces of water, and heated the mixture in a small copper-vessel, the following appearances were observed:—When the sugar and water had acquired the temperature of 230°, the mixture boiled rapidly; this it continued to do for about an hour, and during that time the thermometer remained stationary. Afterwards, as the water evaporated, the mercury moved slowly upwards until it marked 343°, when the sugar became black. The heat was continued until the temperature arrived at 370°, and then the sugar burnt with a very strong and permanent flame, and at length what remained in the pan became carbonized like a coke. In answer to some questions respecting an experiment on oil, I stated, that having heated five quarts of oil, it was found that when the oil acquired the temperature of 350° a vapour arose from it, but this was aqueous and not inflammable; that this uninflammable vapour continued to be given out until the thermometer rose to nearly 590°, and then an *inflammable* vapour was perceived; but it was a very feeble lambent flame, and the moment the lighted match was withdrawn, the flame went out. Having been asked whether it would be easy by mere negligence in attending the fire under the oil-vessel, to bring the oil up to a temperature of 600°, and the court having inquired of Mr. Wilson the size of the vessel, and he having stated that it was nine feet long, three feet wide and eighteen inches deep, and the size of the fire-place from twenty to twenty-four inches wide; I gave it as my opinion that one fire, made in such a fire-place and under such a vessel, could not have produced that effect, and that I could not conceive how the working of the oil apparatus could possibly have occasioned the fire *.

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riments were commenced too short a time before the trial, to afford an opportunity for investigating every question that might arise upon the subject. See the preceding observations on Mr. Wilson's evidence page 322.

* In saying this, I meant it to be understood *while the oil vessel and its appendages remained perfect*; and there is no evidence that they had sustained any injury previous to the commencement of the fire.

Upon my cross-examination, Mr. Scarlett made some inquiries respecting an experiment on *old* oil, to which I gave the following reply:—

"With *new* oil I did not observe any inflammable vapour until the oil was heated to 586°; but the gas* from the *old* oil was inflammable at 508°." "Suppose," said he, "there is one gallon of oil that has been heated for every day in a week, and allowed to cool again, and another heated for a month every day, and allowed to cool again; are you able to inform the jury, which of the two would come to a certain degree of temperature with the smallest degree of heat?" To this I replied, "that I had made no experiment of that sort, but that I had observed a remarkable thing respecting the evolution of gas; namely, that when the oil was heated to 590° it gave out eight cubic inches of gas only in four minutes, but when heated to 620° it gave out thirty-two cubic inches in one minute †." On being asked, if the sugars were heated to any thing like those temperatures, what would be the effect on the sugar? I replied that "the sugar would be carbonized long before, and if heated only to 400° it must have boiled over, unless the man had ordered the communication with the pump to be cut off." And, it appears to me, that this is the great advantage of Mr. Wilson's apparatus; because, where there is any apprehension of the sugar boiling over, the man has only to order the pump to be stopped, and there is an end of the danger.

Observations.—In some late experiments I have discovered that the vapour of whale oil will burn readily at the surface of

* In this part of my examination, the terms vapour and gas were used indiscriminately, which was certainly improper; but at that time I imagined that the emanation which took fire as it issued from the heated oil at the above temperatures, was a mixture of oil-vapour and carburetted-hydrogen-gas; whereas, subsequent experiments have convinced me that no inflammable gas is produced until a portion of the oil becomes actually decomposed, and charcoal formed.

† 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.

the oil before the oil attains the temperature of 500°; but at a short distance from the surface it will not continue to burn. This accounts for the vapour not burning with an unintermitting flame when issuing from a tube, unless the oil be heated to about 600°. The cooling of the vapour is so rapid that its temperature is diminished more than 100° in passing through a pipe only a few inches in length.

Respecting the construction of the furnace under the oil-vessel at Whitechapel, I am desirous of remarking that the size of a fire-place is not an accurate criterion of the quantity of fuel which may be consumed in it, as that depends almost entirely upon the nature of the draught. In many cases the enlarging of the fire-place has a direct tendency to lessen the consumption, by checking the rapidity of the current of air. Thus, in the Scotch and Irish distilleries, the quantity of fuel which is burned by concentrating the draught within a moderate-sized fire-place, is very great. Under a fifteen hundred gallon still, the bottom of which is nine feet in diameter, twenty-four tons of coals per day are consumed, three-fourths of which, at least, are wasted, and might be saved by a more judicious application of the heat. In this case, however, my object was to find by the size of the fire-place, whether it would hold fuel enough at once to do any harm if neglected, and allowed to burn away under it. Any one who will take the trouble of estimating the heat necessary to raise a hundred gallons of oil from a moderate temperature to that of 600°, will, I think, agree with me in my answer on this subject.

William Thomas Brande, Esq., *Secretary of the Royal Society*, &c. &c., was next examined. This gentleman, who is also Professor of Chemistry at the Royal Institution, stated, that he "had examined the model of the new apparatus with the view of giving evidence in court; that he had made a few experiments on the preceding day to prepare himself on one or two points; and that, as far as his experiments went, he could say that the apparatus in question was less dangerous than the old mode." When asked the grounds upon which he had founded that opinion, he replied, "I placed a vessel of oil over a fire, and a small pan of sugar in it, there was a thermometer in the oil vessel, and when the oil attained 300° or 400°, the sugar blackened, and I applied a candle to it, and it burnt. I applied a taper to the oil, and found that it was giving no gas; from which I conclude that sugar gives out gas at a lower temperature than oil, and consequently that the oil is not so dangerous as the sugar." On being asked at what time the oil gives out inflammable gas, he said, "I think at not less than 600°. I then took the thermometer out for fear of breaking it, and, a little after, I observed it gave out inflammable gas, but in a very small quantity*." Mr. Brande then proceeded to state, that "if gas were generated in the oil-vessel, it would be productive of no danger; that all inflammable gas from oil is lighter than atmospheric air; that the gas which the combustion of oil produces is heavier, but it is not then an inflammable gas†; that taking the situation of things as they really are, and supposing gas to be produced by an intense fire under the oil, no explosion would have taken place, because the gas would have gone up the steam vent; that he agreed with Mr. Wilson in all the parts of his evidence that he was qualified to give an opinion upon; that it was his impression that the fire had broken out at a higher place in the building than that where the oil-vessel stood." When cross-examined, this gentleman gave the following important and decisive opinion. "If," said he, "there had been a possibility of any inflammable matter passing into the steam-bin, I cannot conceive it could have lodged there *with that aperture*‡; if any thing had closed the aperture, then it might have happened."

* It is observable that Professor Brande stated, that a little after the oil had been heated to 600° it gave out a very small quantity of inflammable gas; but I have reason to think that if he had had an instrument capable of measuring the temperature, he would have found it, in that instance, to have exceeded 700°.

† The products from the combustion of oil are generally carbonic acid gas and water.

‡ It should be recollected that a small leaden pipe, sixteen or eighteen feet long, and connected with the inside of the oil-vessel, rose up into the

Frederick Accum, Esq. M.R.I.A., &c., and Lecturer on Chemistry at the Surry Institution, was next called. He was examined by Mr. Serjeant Lens, and gave the following testimony:—That fresh whale oil would emit inflammable gas at 600°, but not at a lower temperature; that in a pan similar to the one used at Messrs. Severns', if it contained 100 gallons of oil, it would take a man at least eight or ten hours to raise it to that temperature; that he had also made experiments upon some oil which had been in constant use, that is, heated and cooled three times a week for nineteen months, and from this oil he could obtain gas at 460°, similar to the common gas used for illumination; that if an explosion of oil gas had taken place in the fill-house, as supposed, the explosion would have been heard all over London, and that the oil-vessel would have been lacerated in pieces. He also said, that new oil contains mucilage and water, which are carried away before it acquires a great heat; consequently, old oil, which has been frequently heated, and from which these matters have been expelled, will become hot sooner than new oil. Mr. Accum's testimony respecting the certainty of the escape up the steam-vent of any gas that might have been emitted from the oil; the impossibility of the fire having been occasioned thereby; and the greater safety of the new process, when compared with the old one of boiling sugar, was similar to that which had been given by several of the preceding witnesses.

Observations.—Upon Mr. Accum's testimony respecting the temperature at which he obtained inflammable gas from the oil which had been heated for nineteen months, I am under the necessity of saying that he must either have been deceived as to the nature of the oil which had been sent to him, or there must have been some mistake in the manner of conducting the experiment; as several of the chemical gentlemen, who, like myself, were engaged for the plaintiffs in this action, have agreed with me in asserting that inflammable gas cannot be procured

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interior of the steam-bin, and that the steam-bin had a direct communication with the common atmosphere.

from whale oil at 100° higher temperature than he has stated; and he positively asserts that it was whale oil on which he operated.

I apprehend Mr. Accum was right in saying, that if the sugar-house had been filled with oil gas, and this had exploded, the explosion would have been heard all over London; but it surely was of little use to examine scientific men upon this point, as there was nothing in the appearance of the premises after the fire, that indicated any thing of that kind. It is true that Samuel Willoughby made oath, that he saw a window blown out during the fire, and with a force that would have knocked down a horse*; but unfortunately for the veracity of this man, the window which he swore to, is still standing in the wall, in as perfect a state as it existed before the conflagration.

William Allen, Esq., F.R.S., *&c. &c.*, was next called. This gentleman, who had delivered chemical lectures at Guy's Hospital for sixteen years, gave a very perspicuous and decisive testimony, of which the following is a brief outline,—"I have," said he, "inspected the model before us with much attention, and I am decidedly of opinion, that the method described in the model is by far the safer plan, beyond all comparison." "I consider," added he, "the apparatus before us, as obviating a great part of the danger; we merely want a heat of 240° or 250° for sugar, and here you heat oil to between three and four hundred degrees; it is made to circulate through a copper tube, and gives out the heat in the safest possible manner." He moreover stated, that "the difference of the temperature at which old and new oil give out inflammable gas, is very small indeed, and amounts to a very few degrees of Fahrenheit's scale;" that if gas of this kind were generated in the oil-vessel, no danger whatever would arise from it; that "if a person were industriously mischievous, it would take him many hours to bring the oil up to a temperature at which it would be decomposed; and in proportion as it was decomposed, it would find its way up the steam vent, and out of the

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* See Mr. Gurney's Report, page 123.

house;" "that fat oils cannot be raised in distillation without becoming totally changed, and therefore they are called *fixed* oils; that it requires particular management, and a great deal of heat, to produce the oil of dippel;" that "he considered the gas from the lamps used in the manufactory, was ten times more dangerous than the oil apparatus;" that "he had examined the oil-vessel after the conflagration," and "not the least fissure, or symptom of explosion, was visible in it."

Observations.—The oil of dippel was introduced by Mr. Allen, in consequence probably of its having been asserted that this inflammable substance might have been produced by the oil at Whitechapel, and have occasioned the conflagration. Among all the hypotheses which have been advanced respecting the fire at Messrs. Severns', this is the least tenable, as it requires a red heat to convert fixed oil into dippel's oil. The common mode of obtaining it for the purposes of medicine is by the distillation of *dry* bones in an intense heat.

At the close of this gentleman's examination, I was called to explain to the jury the distinction between fixed and essential oils; and in answer to several questions, I stated, that "fat oils, such as linseed oil, whale oil, olive oil, rapeseed oil, and several others, are called *fixed* oils, because they will endure a high temperature, and may be boiled in an open vessel with very little danger; that they are also called fixed oils, to distinguish them from essential oils, which cannot be heated without danger *; in short, there are no two substances in nature," said I, "more different than fixed oils and volatile oils; they are alike only in name, and ought not to be confounded together."

Mr. Thomas Barry was the next practical chemist who was called. He stated that "he had made a few experiments,

* I took this opportunity of saying a few words upon essential oils, because I had just been told there was an intention of insisting upon the danger of distilling the essential oil of turpentine, which has nothing to do with the question at issue, but was very likely, without explanation, to have perplexed the jury.

and found that sugar was liable to boil over at a temperature of 250°, the oil at a temperature not less than 650° or 700°; that the sugar took fire at 370° by the application of flame; and the oil took fire at a temperature higher than Fahrenheit's thermometer goes, suppose nearly 700°; and that, with a view to determine whether the leaden pipe might have been melted by the oil process, he put some lead into the oil and heated it up to 600°, and afterwards took out the lead and found it uninjured." The other parts of Mr. Barry's evidence were confirmatory of the testimony which had already been given by other gentlemen.

Observation.—The result of Mr. Barry's experiment on lead is what might have been expected, though it contradicts the most respectable testimony; Sir Isaac Newton and Dr. Lewis having given 540° as the melting point of lead, Morveau 590°, and Irvine 594°. Some years ago I made a series of experiments on the melting points of several of the metals, and found the *pure* lead, which I had prepared by the reduction of litharge, would not melt till it had acquired the temperature of 612°.

Mr. Charles Silvester spoke very decidedly as to the superior safety of boiling sugar by oil instead of by a naked fire; he said "he had attended in court during the examination of Mr. Wilson; that he agreed with him in the evidence he gave, and did not differ from him in any thing that he was acquainted with, equally with himself; that from the experiments which he had witnessed he was sure it must have taken three hours to raise the oil in the large vessel from 300° to 600°, even if the pump had been stopped and the fire constantly attended to; that if gas were given out from that apparatus it would be in very small quantities at first; that the oil would be decomposed so gradually as to let the gas escape without compression; that it would be impossible to set fire to the oil in the vessel, because there could be no oxygen present, and that if the oil had leaked into the fire, no flame could be communicated to the oil in the large iron vessel."

The next person called was Mr. J. T. Cooper, lecturer on chemistry at the Russel Institution. He stated that if gas should be produced in an apparatus that corresponds with the model in court, " it would all go off up the steam-vent, even if it were generated at the rate of 500 cubic feet a minute; that in heating oil, when he had got it to a certain temperature, he found it difficult to add to it; that in one experiment it took forty minutes to raise it from 200° to 350°; whereas, to bring it from 350° up to 600° or 610°, it required nearly two hours and a half more; and, at last, he could hardly elevate it at all, though the fire was regularly supplied the whole time.

Robert Hendrie, Esq., being next examined, related that " he had applied his mind to chemical pursuits; that he had made an experiment to ascertain the difficulty of raising oil above a certain degree, with a particular portion of fuel, and to do this, I took," said he, " a small portion of oil, and heated it in a vessel over an Argand lamp: I found I could easily raise it to 400°, but I could go no further with that degree of heat."

Henry Coxwell, Esq., one of the chairmen of the committee of chemistry at the Society of Arts, was next called. This gentleman gave it as his decided opinion that the operation of boiling sugar by the oil process, is infinitely less hazardous than the common mode, and that he considered it to be so for the reasons already given by Mr. William Allen and others.

Mr. James Deville, manufacturer of gas-light-apparatus, stated, that " he had been much accustomed to the uses of oil and experiments connected with it; that he had been in sugar-houses in great number, and thought, for the reasons already given, that the oil method was safer than the old mode; that in an experiment he made with oil he took the degrees of temperature every two minutes and a half,—that at first he increased it 27° in that time, then only 18°, and afterwards only in the following ratio, 15°—12°—10°—8°—6°, of heat acquired in the same time, though the fire became still stronger and stronger; that he had seen oil boiling in large quantities at Hull; there were at least five or six tons, and that the proprietor put a lighted paper to the surface, for the purpose of shewing the experiment, and it would not take fire."

Mr. Samuel Parkes was then *recalled*, and examined as to the nature of explosive mixtures.

Timothy Bramah, Esq., was the last witness called in behalf of the plaintiffs, in this stage of the trial, who was capable of giving a chemical opinion; and he stated as follows:—" I have," said he, " made the experiment mentioned by Mr. Hendrie of heating oil over an Argand lamp, and I gave it up because I could not bring the oil to the desired heat; for, having got it to a certain degree, the thermometer became perfectly stationary. I tried the experiment again in another way, and the plate underneath the apparatus melted, and I gave it up." He added, " I have seen the operation of boiling sugar on a large scale by means of this apparatus, a year and a half ago; and I think, for the reasons already adduced, it is much less dangerous than the former plan."

On the Second Day, evidence for the Defendants was called, and the first person who spoke on the chemical part of the subject was Samuel Wilkinson, who said he was foreman to Messrs. Taylors and Martineau, chemists and engineers. The following is a brief account of the most material parts of his testimony. " I received orders," said he, " from Mr. John Taylor, to make experiments on oil. The vessel I used was three feet long, fifteen inches wide, and fifteen inches deep, made of wrought-iron united by rivets. Mr. Martineau tried this boiler with a small quantity of common whale-oil, which he *boiled* in it on the Saturday afternoon, the 13th of February. On the 15th, Mr. John Taylor ordered me to add a quantity of oil to the oil Mr. Martineau had boiled; and I added sufficient to make the whole between twenty and thirty gallons, with the design of ascertaining (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 vapour did not appear inflammable until we came to a temperature of 280°. The vapour at 280° took fire in sudden gusts, as an explosion; when it came to a temperature between 280° and 300°, there was a noise like fat frying in the boiler; and when the vapour that was inflammable issued from the tube, there was a sudden concussion in the boiler, which I call an explosion. On the 16th of February, I emptied the boiler and cleaned it out, and then put in about thirty-three gallons of fresh whale-oil. The experiment was continued twelve days, and the oil was heated about eleven hours each day, and the highest to which it was carried was 507°. On the second day the vapour was slightly inflammable at 375°, as it was before at 280°, in the other oil. On the fourth day the vapour was very inflammable at 360°, and at 380° scarcely inflammable. On the sixth day I applied a light to the vapour *in the boiler*, which took fire. I unscrewed the tube, and applied a light to the hole in the boiler, and the vapour burned in the boiler. On the seventh day it was suggested that it was proper to take twelve gallons of the oil out of the boiler, which was done. I received orders at the same time to raise the temperature to 500°. On the eighth day it was carried to 500°; when at the highest it was inflammable at seven inches and a half from the top of the tube, and it burned like lightning; the explosion in the boiler was still the same. When this concussion did not take place in the boiler, there was no vapour or flame, it was only fire occasionally, as the vapour came from the tube. On the ninth day, at 497°, the vapour took fire at the end of the worm: there was a worm attached to the boiler by another pipe; this was fifteen feet long, it passed through a cask of water, and at the end of this it took fire at 497°, and continued burning twenty minutes, and burned six inches in length. On the tenth day it was inflammable at 345°; at 390° it spread itself like lightning. On the eleventh and twelfth days it was slightly inflammable at 310°. Mr. Pastorelli supplied the thermometer, I received it from him myself; but it was proved before it was put into the oil *by hot water.* Mr. Pastorelli makes them; he makes clocks, and so on."

Observations. So contrary is this testimony to every thing which I have found to the conclusions of those gentlemen who were consulted with myself by Messrs. Severn, King, and Co., that I have gone through the whole with great care, and have collected the principal points into one view, that any individuals accustomed to chemical inquiries, may, if they please, repeat the experiments, to see if from common whale oil they can produce the results here related.

I have first to observe, that he says Mr. Martineau *boiled* the oil, which appears to me to have been very improper, as the object ought to have been to assimilate the experiment as much as possible to what took place at the sugar-house; where the oil was always kept at a much lower temperature. The mixture of two kinds of oil was also improper. It is stated by Wilkinson that "the vapour at 280° took fire, and there was a sudden concussion in the boiler like an explosion." To this I have only to say, that I have never been able to procure an inflammable vapour from whale oil that would burn at the end of a short tube fixed in the cover of the retort, until the oil was heated beyond 600°, and that I never witnessed the concussions and explosions which he speaks of. It is also remarkable, if the vapour which issued from the oil was so *explosive,* that he was not on the sixth day blown to atoms, when he unscrewed the tube. On the ninth day he says "he had a worm-tub, with a leaden-pipe fifteen feet long passing through a cask of water, and at the end of this the vapour took fire at 497°; it continued burning twenty minutes, and burnt six inches in length." I do not believe I could have produced such an effect with *pure* whale-oil if I had heated the vessel to 700°. I have seen the vapour burn at 480° for a moment *close to the surface of the oil;* that is, when kept hot by the body of oil, but it would not inflame at the end of a tube twelve inches long when heated to 600°. On the tenth day, he says, "that at 390° it spread itself like lightning, and on the eleventh and twelfth days was inflammable at 310°." These statements convince me that there must either have been some mistake as to the *nature* of the oil that was employed, or a part of the mercury must have escaped from the thermometer by an accident, or by distillation, in the intense heat to which oil may be subjected, as we are told the thermometer was *open at the top*. It is certainly not usual to employ such thermometers in any experiments that require accuracy. Mr. Wilkinson also tells us, "that the thermometer was proved by hot water before it was put into the oil;" but I should be glad to be informed, how the accuracy of a thermometer, which is designed to measure temperatures above 212°, can be determined by hot water.

Mr. Michael Faraday, Assistant in the Laboratory of the Royal Institution, stated, that "he heated oil in an open pan, and found the vapour combustible *on the surface of the oil* at 490°, and that it will burn constantly, the temperature being kept up; that he has distilled whale-oil, and produced naphtha from it; that he had produced it at a lower temperature from oil that had been heated to 360° for twenty-four successive days, than he could from fresh oil; that the vapour of the distilled oil is heavier than the atmosphere, and so is the vapour produced from the naphtha; that the vessel in which the oil was boiled had a tube rising two feet above it; that he caught the vapour issuing from the pipe in a pewter vessel, and when a light was applied to it, it inflamed throughout the capacity of the vessel, and that if the steam-bin at the sugar-house had been filled with such vapour, he should not be surprised if it did inflame." On being asked, if any experiment was made to see what effect was produced on the oil itself, he replied, "they rose the oil to 410°, and the emission of vapour was continued; the heat was then continued to get a stronger vapour, but instead of that we got the oil out." When asked at what degree it had then arrived? he answered, "During the boiling, and when we began to look at things, the temperature was 460°." He went on to say, "it passed out in jerks, it was thrown out by a rapid concussion in the boiler, the formation of the vapour expanded, and threw out the oil; it rose four or five feet from the end of the pipe, and struck the ceiling; it was a sort of irregular fountain; the oil ran into the fire, and the fire was very much increased by it at the time this boiling happened." On being asked to shew the effect of the explosion of the naphtha, he lighted a small piece of taper, and put it into the phial of what he called naphtha, which, after one or two attempts, produced a feeble lambent flame, of a bluish yellow colour, which extinguished the taper and immediately expired—a most offensive smell being at the same time perceived throughout the court. On being asked, if the inflammation that took place when he collected the oil vapour in a vessel was accompanied with noise? he replied, "without noise;" and added, "You may make *explosion* without *noise*, and, generally speaking, it would be a *silent* one." He said he thought the process of heating sugar by oil more hazardous than the ordinary mode, and especially from the production of the naphtha at 410°, and from the danger of the oil boiling over*. On being asked the construction of the boiler in which he made his experiments, he said "the boiler was not enclosed with bricks, and the oil ran into the fire." "Then," said a juryman, "their fire could not have been made on the same principle as that," (pointing to the model of the apparatus used at the sugar-house); and another juryman added, "the experiments of this witness do not apply to such a boiler as this."

Observations. Mr. Faraday stated, that in his own first experiment he could not obtain inflammable vapour, even on the surface of the heated oil at less than a temperature of 490°, which is a striking contradiction to the first witness on the same side of the question, who asserted that he procured it, *at the end of a tube*, at 280°. It was improper, I conceive, to say that "if

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* If Mr. Faraday had attended to the habitudes of boiling oil, he would not have spoken of the danger of its boiling over, as it does not rise in the pan, except as is occasioned by the gradual expansion from change of temperature; but always boils with an horizontal motion.

the steam-bin had been filled with oil vapour he should not be surprised if it did inflame," because he must have known it to be impossible to fill such an area as the steam-bin with oil-vapour, as it condenses on coming into a cooler medium, and is no longer vapour. The statement was likely to mislead and prejudice the jury. What he could mean by saying that "during the boiling the temperature was 460°," I cannot understand, if it was whale-oil that he was operating upon, as such oil does not boil until it be heated to many degrees beyond 600. On the oil being driven through the pipe until it struck the ceiling much might be said, if I were not afraid of exceeding my prescribed limits; but I must observe, that 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; there is, however, reason to believe that the vessel was nearly full when they began the experiment, and the sudden accession of heat occasioned such an expansion in the oil itself, that the vessel was incapable of containing it, and then there can be no wonder that it was expelled with force through the pipe. I have found by direct experiment, that whale-oil expands more than one-fifth in being heated from 58° to 460°. The silent explosion which this witness spoke of, I cannot at all comprehend; for explosions without noise, appear to me to be as absurd as to talk of music without sound, or fragrance without smell.

One word more respecting the construction of the experimental apparatus employed by Mr. Faraday and his friends. The boiler, he says, "was not enclosed; it sides were left bare to the air, and there were crevices between the boiler and the fire." How unlike this to the real apparatus which it was intended to represent! and how can we wonder, when the vessel had been filled without a due regard to the expansion which the oil would experience, and the oil spouted up to the ceiling in consequence thereof, that the persons who were collected to witness the experiment, and saw the fire greatly increased by the oil running into it, down the sides of the vessel, should have been greatly alarmed, and should afterwards have come into court and declared on their oaths, that they believed the boiling of sugar by means of heated oil to be a dangerous process.

Mr. Richard Phillips, F.R.S. E., Professor of Chemistry in the Royal Military College, and Lecturer on Chemistry at the London Institution, was next examined. He stated, that in his first experiment with oil he heated half a pint in a retort for about thirty hours up to 360°; he then observed that the oil had thickened considerably, and become darker. "I then heated it," said he, "pretty suddenly up to about 500°, and there distilled over, at that temperature, a volatile oil: this oil I have a specimen of, this is a portion of it," (producing it). "This oil," continued he, "though it appears extremely fluid now, if taken into a cold atmosphere, becomes solid." He went on to state, that he obtained a vapour from common whale-oil at 500°, but that the oil which had been heated to 360° for twenty-three days, gave out inflammable vapour at 400°; that this was vapour, and not gas, because the temperature, in his estimation, was not sufficient to convert oil into gas; but when heated to 460°, it gave out aqueous vapour and inflammable gas; that water is formed during the distillation of oil, by a portion of the oxygen and hydrogen uniting; that he considered the oil-apparatus for refining sugar dangerous; that the oil-vapour could not get on fire without coming in contact with flame; but, said he, "the oil, as in the public experiment, would be forced out by sudden ebullition, and run down to the fire-place, and take fire;" that "in the large vessel there were 100 gallons of oil, and the force would probably be in some ratio to the quantity." On being asked what time it would require to effect that, he replied, "A few minutes after it was heated to 360°; supposing they were at work at that temperature, and the pump stopped, in twenty minutes, I am certain, by a strong fire, the oil would spout out at the end of the tube." In their experiment vessel, he did not know whether the safety-pipe dipped into the liquid or not.

Observations.—I am aware that a volatile oil may be distilled from whale-oil at 500°; but I was not aware that an oil could be separated from it, that in the month of April would become solid by exposure to the cold of the common atmosphere. From oil that had been increased in its specific gravity by a continual heating for 28 days, from 0.922 to 0.963, I procured a volatile oil by common distillation, that was only of the specific gravity of 0.865; but this seems to have no tendency to become solid, and I have examined it several times since it was distilled. As to this gentleman's procuring inflammable vapour from oil at 400°, I can only say, that I could not procure it at that temperature; but as to the production of inflammable *gas* from whale oil at 460°, that is certainly a mistake, as 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. Mr. Phillips' assertion, that if the oil at the sugar-house, heated to 360°, had been urged by a strong fire, it would in twenty minutes have arisen to 460°, and that the oil would then have spouted out at the end of the tube, and would have run into the fire-place, and taken fire, has very much surprised me; as I know he is a man of great discrimination, and not apt to make assertions, without due consideration. The statement of such an opinion, which might have done the plaintiffs in this action great injury, was made, no doubt, without considering the different capacity of the two vessels, and the quantities of oil put into them. In the one case, the vessel was so nearly filled with oil, that when the usual expansion* took place, the vessel could not contain it; whereas, in the other case, there was a vessel of the capacity of 300 gallons, with only 100 gallons of oil in it, and a tube of safety in the cover, which was sixteen feet long, and terminated in a brick tunnel, communicating with the common atmosphere. In the experiment-

* I have found by experiment, that whale-oil expands in heating from 58° to 430°, more than one-fifth of its original volume.

vessel at Whitecross-street, room was not allowed for the oil to expand sufficiently, and therefore the sudden accession of heat necessarily drove it through the tube, and occasioned it to strike the ceiling; and as the vessel was not properly set, so as to enclose the fire completely, the oil naturally fell from the ceiling into the fire-place; but in the vessel which was used at the sugar-house, such ample room was left for expansion*, that no fire, which could have been made in the fire-place underneath it, could have forced it into the tube of safety; and if that had occurred, the oil could not have come in contact with the fire, as the fire-place was entirely covered by the iron vessel itself, and completely enclosed on every side by compact brick-work.

Doctor John Bostock, F.R.S., F.L.S., and Lecturer on Chemistry at Guy's Hospital, was next called. This gentleman told the Court that he attended some experiments at Mr. Taylor's on the 6th of April last; that he saw half a pint of oil submitted to distillation in a glass retort, and that a volatile oil came over at 410°, that was inflammable; that he then directed his attention to a parcel of oil that had been subjected for 23 days to a heat of 360° in an iron boiler, and was reduced to a state like *pitch* in appearance; that he applied a taper to the top of the tube communicating with the upper part of that boiler, and that at many degrees under 400°, there where small jets of flame, and that at a little more than 400° those jets became more considerable; that they coiled up a sheet of paper in the form of a cap, and put it loosely on the extremity of the tube, and that upon applying a taper this became immediately filled with flame. He then described the spouting of the oil out of the tube, which had been related by former witnesses, and added,

* The oil, which measured 100 gallons in the common temperature of the atmosphere, would probably have measured 122 or 123 gallons when heated to 460°; but as it was heated in a vessel of such large dimensions, and as oil boils, not with a perpendicular, but with a horizontal motion, I cannot conceive that the most violent boiling would have forced it out of the vessel.

that he believed those facts were *quite new* to all the parties present, and to chemists in general; and on being asked if he thought the new process a more hazardous operation than the old mode of boiling sugar, he replied, "I think no person could view the experiment in Whitecross-street, without seeing it was an agent of extreme activity and danger."

Observations.—From the undoubted respectability of this gentleman, and also of the others who were engaged on the same side of the question, one can have no hesitation in believing that they all faithfully reported what they saw; but when we compare the degrees of temperature at which the several appearances took place, with the temperatures at which similar phænomena occurred in the experiments of the Plaintiffs' chemical witnesses, we are naturally led to search for some satisfactory explanation of these contradictory results. 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, and that there must have been some source of error in the manipulations of our opponents which have escaped their notice. It has occurred to me, that as whale-oil often goes through several hands, before it comes to the consumer, and as different dealers have different modes of refining it, that there may have been something in the oil which there ought not to have been; or it may possibly be attributable to the thermometer, which from the circumstance of its being an open one, is not so improbable as it otherwise would have been. I do hope, however, that the gentleman who instituted this public experiment, as it has been called, will investigate the matter thoroughly, for the credit of all of us. Instead of "a state like pitch," Dr. Bostock must have meant, a state like tar. All he intended was, as I conceive, to describe the oil which had been long heated, as a black and viscid substance. I have only one other observation to make respecting Dr. Bostock's evidence, *viz.*, that from the effect which the accident in Whitecross-street had upon the Doctor's mind, I should not have been surprised if his description of the oil as "an agent of extreme activity and danger," had proved fatal to the plaintiffs; at the same time I believe that if he had known the quantity of oil that was put into that vessel, and had been aware of the great expansibility of whale oil, he would have expected just such a result as did actually happen, and would have given a very different testimony in court.

John George Children, Esq., F.R.S., F.L.S., M.R.I., F.A.S., M.G.S., of the British Museum, was next examined. This gentleman, who had never had an opportunity of seeing a sugar-house, or of making any experiments himself on either oil or sugar, was not likely to throw much new light upon the subject; but having seen the public experiment in Whitecross-street, he gave an opinion respecting the comparative safety of the two modes of boiling sugar, similar to several of the witnesses on the same side of the question. Mr. Children, from his general knowledge of chemistry, was very well qualified for investigating the subject, and it is to be regretted that he did not do so.

John Taylor, Esq., M.G.S., Chemist and Engineer, was next called upon to give evidence. This gentleman stated, that his first experiments on making gas from oil, were five years ago; that he had a patent for it; that he was now a partner in a very large manufactory, and from his experience in oil, he did not think it a very safe article to be used to apply heat to any other substance. He then said a great deal on the habitudes of tar oil, but as this does not appear to me to bear at all upon the question, and as I have extended this paper too much already, I omit the whole of it, together with the reasoning respecting currents in mines. On being asked respecting his experiments with whale-oil, he said there was an inflammable vapour that issued from it at various temperatures from 340° to 390°, but that he left Wilkinson to make a minute of every thing. That he had 400 thermometers made by Pastorelli; that the one used in the experiment was one of them; that he had proved some of them, and found them good for *common thermometers*. On being asked if he proved the second thermometer before it was applied by Wilkinson, he said, "I believe not; I left it to him to do it; in fact, I left the experiment in his hands." On his further examination he said that "the vapour which arose in their experiment appeared to him to be the same sort of combustible matter that he had seen from the tar oil." On being asked if he thought the new machine added to the danger of refining sugar, he replied, "I have thought so long ago, and think so now."

Observations.—From the knowledge which this gentleman possesses of chemical science in general, and from his experience in making chemical experiments, it is greatly to be regretted that his avocations did not permit him to superintend and conduct this important experiment himself; for if he had, he would not have depended upon a common thermometer open at the top, and consequently liable to lose part of its mercury, but would have taken the most perfect instrument that he could have procured. I do hope, however, that he will make the necessary experiments himself, as no man is more capable of doing it with accuracy, and that for the credit of science he will give the results to the public as soon as possible. I observe the whole of Mr. Taylor's evidence refers to oil-vapour, and not to oil-gas.

The next chemist called was Mr. Alexander Garden. This gentleman said that he was present at the experiment in Whitecross-street, but he was evidently cautious how he gave any opinion. On being asked if he could form a judgment of the comparative danger of the two methods of boiling sugar, he replied, "I should be inclined to consider the process by oil is, of the two, the most hazardous."

Observation.—This gentleman gave his evidence in so dispassionate a manner, that I think it very probable, if he had depended solely on his own experiments, and had not witnessed the spouting up of the oil at Whitecross-street, which had a natural tendency to alarm and prejudice those who were unacquainted with the real cause, it is very likely he would have come into court with a different impression, and would have given a very different testimony.

Arthur Aikin, Esq., F.L.S., M.G.S., and Secretary to the Society for the Encouragement of Arts, *&c.*, was then examined. He stated that some years ago he made experiments on whale-oil, and ascertained that besides the proper oil, it contained a quantity of animal jelly in solution. " I found," said he, " that when this was *boiled* pretty rapidly, it burnt, in some degree, to the pan, in consequence of which the oil became black; and it is well known that if animal jelly be exposed to such a temperature as to blacken it, it will be decomposed, and a quantity of very volatile inflammable oil will be given out. This oil is known by the name of Dippel's animal oil. He then stated that he thought a thermometer dipping into the surface of the fluid, in a vessel of heated oil, would be a very inadequate test of the temperature of the bottom of that fluid. " On this account it is," said he, " that I think oil is a fluid which it is not advisable to make use of to raise the temperature of other substances."

Much of Mr. Aikin's evidence was similar in tendency to that of other gentlemen who went before, and which I have remarked upon already. I, therefore, pass it over, together with other parts which are so ambiguous that I cannot understand them. When cross-examined by Mr. Solicitor-General, he said, that to produce a vapour from oil that would be inflammable, it would require " a temperature sufficient to char the substance," and that " if the mass of oil is kept in motion, it is less liable to this than if it remains quiescent;" but that southern whale-oil contains less gelatine than the other oils. In answer to a question from Mr. Scarlett, who shewed him a pot of oil, he said, " I should expect this to be oil which had been exposed to a high temperature, in which the jelly is considerably charred, and the oil is fouled; and I think it would transmit heat more slowly than previous to the thickening and blackening of it; the more limpid oil is, the more it will transmit heat. It has been proved by Count Rumford, and other chemists, that the transmission of heat is retarded by its being solid."

Observations.—I conceive that Mr. Aikin ought not to have dilated upon the effects which would take place when whale oil was " boiled pretty rapidly;" because it had been proved in court, that the oil which was employed for heating the sugar at Whitechapel was *never boiled*, but heated only to a temperature of about 340°, or 350°. In regard to Dippel's oil, I am surprised Mr. Aikin did not know that this substance cannot be produced by any means but by what has been called destructive distillation. His observations respecting the thermometer do not exactly apply, because there would be no difficulty in placing a thermometer so that the bulb should touch the bottom of the vessel. I think Mr. Aikin was wrong in saying, that to procure inflammable vapour from oil it would require " a temperature sufficient to char the substance," for several persons have proved that they had produced it at a much lower temperature than this indicates; but if Mr. Aikin had said inflammable gas, I could agree with him entirely.

Mr. Aikin is, I think, perfectly right in the importance which he attaches to the pump in this apparatus; and his opinion of the superior quality of southern whale-oil, in some measure justifies Messrs. Severns in having chosen that particular kind for their purpose of boiling sugar. The opinions of Count Rumford respecting the power which fluids have of conducting heat, are not at this day entirely agreed to, but I believe Mr. Aikin is quite right in his conclusions on this part of the subject.

Several witnesses were now called to give evidence respecting their observations on the first appearance of the fire, and they each declared that the flames were of the usual colour, and that they perceived no peculiar smell whatever.

On the Third Day of the trial Mr. Faraday was recalled, and he delivered the following opinions: That the vapour given out from the oil-vessel at the sugar-house, " if fired, would have exploded, or have burnt more or less quickly, in proportion to the mixture of air with it; that the explosion would not resemble that of gunpowder, for it would not be so violent, but that the explosion, if such took place, would be of a bursting nature, and would rapidly expand;" that "the smell of the vapour is essential to the vapour, but when it is exploded it forms other substances." After several other remarks from Mr. Faraday, I was called and asked if I concurred in this testimony, to which I replied, that I never witnessed the explosion of gases without smell; that I do not believe that an inflammable vapour is produced from whale-oil at the low temperatures which have been named, unless some other oil be mixed with it; that I conceive it impossible that inflammable gases could have remained in the steam-vent, they must have gone into the atmosphere as they were generated; that I never witnessed any explosions of carburetted hydrogen gas without tremendous noise, nor without smell." On Mr. Phillips being recalled, he said, that "with respect to the adulteration of the oil, it was as likely to be so in the other case of this concern, as in the experiments they made." After which one of the jurymen said, "We are not satisfied about the oil being pure."

Observation.—The obvious reply to Mr. Phillips's remark is this: That it is not likely that the oil used by the chemists for the plaintiffs was adulterated, because it had not the characters of volatility which the other oil had; neither do we know of any thing that could have been added to oil to *restrain* its volatile parts, and render it *less* inflammable; whereas, there are many substances with which the whale oil might unintentionally have been contaminated, that would *increase* its inflammability, such as oil of turpentine, or tar oil.

Henry May and Mr. Lockie were recalled to explain some parts of their former evidence; and when Sir John Copley, the Solicitor-General, had addressed the court and jury, his lordship summoned up in a very luminous speech. The jury withdrew, at four o'clock, and returned in three quarters of an hour, finding a verdict for the plaintiffs,—Damages, £7181 2*s*. 6*d*.

Thus have I attempted to give an impartial account of the particulars of this very important trial, with such observations as naturally occurred to me when reviewing the evidence on both sides the question, and, at the same time, have endeavoured to explain the reasons upon which my own opinions were founded, some of which are in direct opposition to the majority of those persons who undertook a series of experiments in behalf of the defendants.

The gentlemen from whom I differ are, however, all well known as persons of considerable scientific attainments, and some of them have long been my intimate acquaintance. I cannot therefore be suspected of entertaining an improper feeling towards any of them. Their evidence has been examined by me merely in the capacity of a chemist, and I have endeavoured to do it without fear or prejudice.

As to the purity of the motives of the gentlemen above alluded to, the known characters of the individuals leave no room for doubt; but it has always been acknowledged that the most scientific men have sometimes been mistaken in the conclusions which they have drawn from their own experiments.

It is not for me, however, to assert that I am less fallible than they; it is for the public to investigate the nature and value of the different experiments, and then to decide between us.

To enable that public to do this on good grounds, the above concise account of the case has been written; and I do flatter myself that it will have the effect of removing from the science which I have espoused, that character of doubt and uncertainty which some circumstances attendant upon this trial, might otherwise have attached to it.

Samuel Parkes.

December 1, 1820.

P. S. The other trials, arising out of the dreadful conflagration which has occasioned this paper, are fixed to come on at

Guildhall, on the 13th of this month; and, as it is expected that one trial will settle the question as to the remaining policies, the whole will, probably, be decided before these animadversions are published. Should any thing, however, occur on the ensuing trial that is new and interesting, I may, probably, deem it necessary to trouble the editor with some farther observations. S. P.

Description of the Plate.

Fig. 1.—Is an elevation of an apparatus for boiling sugar, or evaporating any liquid by means of the circulation of heated oil.

A, is a wrought-iron vessel for heating the oil, similar to the boiler of a steam engine. It is set in brick work, with a fire under it of a moderate size, and without any flues round the sides, so that the whole action of the fire is upon the bottom. It is made of an oblong form, and its length should exceed its breadth as much as the situation it is to be placed in will allow. The size depends upon the quantity of oil to be heated, or the liquor which is to be evaporated; and it is observable that the more the surface presented to the fire exceeds the evaporating surface, the greater will be the economy of fuel. Whale-oil, free from sediment, is found to answer better than any other for this purpose, and the quantity necessary to be employed, is merely sufficient to cover the bottom of the vessel to the depth of six or eight inches.

B, is a thermometer for ascertaining the heat of the oil.

C, is a small tube opening at the lower end into the oil vessel, while the upper extremity passes into a long flue, called a steam vent, and communicating with the atmosphere. This pipe serves three different purposes: the first is, that before the pump begins to work in the morning, there is a quantity of air contained in it, and it is necessary there should be a vent for that, when the pump is set to work, in order to prevent any compression in the inside of the vessel. The next is, that with a common suction-pump it is necessary there should be a communication with the atmosphere. Thirdly, it is designed to carry off the aqueous vapour from the fresh oil, which has a very bad smell, and such vapours would injure the sugars, if they got abroad in the sugar-house.

D. Is a cast-iron pump with a spring metallic piston communicating with the oil-vessel A, by means of its suction-pipe E. It is set in motion in the usual manner, by some mechanical power.

F, is a copper vessel, the bottom of which is covered in the inside by a coil of pipe, communicating at one of its ends with the pump at G, and at the other end with the oil-vessel through the pipe H. Through this coil of pipe the heated oil circulates, and being surrounded on all sides by the liquid in the pan F, it gives out about 100° of heat in its passage, and returns to the oil-vessel to obtain a fresh increase of temperature. This pan is surrounded by brick or wood work, to prevent cooling. Of course it has no fire under it.

Fig. 2.—Is a ground plan of the same apparatus in which the coil of pipe in the evaporating vessel F may be seen.

A, is the oil-vessel in which are inserted the thermometer B, and the vent-pipe C.

D, is the pump.

E, G, the pipes forming the communication between the oil-vessel and evaporating pan; which, after circulating in the form of a coil, passes out at the centre of the bottom, and returns to the oil-vessel by the pipe H.

Art. XII. *On the Vapour of Mercury at common Temperatures, by* M. Faraday, *Chemical Assistant at the Royal Institution.*

It has long been admitted, that in the upper part of the barometer and thermometer an atmosphere of mercury exists, even at common temperatures, but having a very small degree of tension. The following experiment renders it easy to shew this atmosphere even when the air has not, as in the instruments above mentioned, been removed. A small portion of mercury was put through a funnel into a clean dry bottle, capable of holding about six ounces, and formed a stratum at the bottom not one-eighth of an inch in thickness; particular care was taken that none of the mercury should adhere to the upper part of the inside of the bottle. A small piece of leaf-gold was then attached to the under part of the stopper of the bottle, so that when the stopper was put into its place, the leaf-gold was enclosed in the bottle. It was then set aside in a safe place, which happened to be both dark and cool, and left for between six weeks and two months. At the end of that time it was examined, and the leaf-gold was found whitened by a quantity of mercury, though every part of the bottle and mercury remained apparently just as before.

This experiment has been repeated several times, and always with success. The utmost care was taken that mercury should not get to the gold, except by passing through the atmosphere of the bottle. I think, therefore, it proves that at common temperatures, and even when the air is present, mercury is always surrounded by an atmosphere of the same substance.

Art. XIII. *Some Account of the late Expedition to the Polar Regions, under the Command of* Captain William Edward Parry.

The circumstances attendant on the failure of the search for a north-west passage, by the expedition under the command of Captain Ross, in the year 1818, induced the Lords of the Admiralty to make another experiment; and accordingly the Hecla and the Griper, properly fitted out for the undertaking, were placed under the command of Lieutenant Parry; a gentleman, who, as the result has amply proved, was highly qualified for an undertaking, perilous in itself, and rendered peculiarly arduous by the failure of his predecessor.

This expedition left the river in the month of May, 1819, and arrived, on the 14th of June, off Cape Farewell, which is the most southern point of Greenland. They then made a fruitless attempt to gain the western coast of Davis' Straits, but were prevented by the ice, and were obliged to proceed midway between the two coasts; having thus ascended to 74° north, the latitude of Lancaster Sound, to which their attention had been directed in the first instance, and finding the ice still between them and the western coast, they entered the ice, and, after eight days of extraordinary effort, they succeeded in getting into the open water to the west. Having landed at Possession Bay, the place where the former expedition had touched after leaving Lancaster Sound, they entered the Strait to which that name has hitherto been attached on the 1st of August. This inlet was now ascertained to lead direct into the long-sought for Polar Sea; it extends about one hundred and fifty miles in a direction due east and west, the shores bounding it to the north and south being nearly parallel, at an average distance apart of from forty to fifty miles. These shores, though in several places indented so as to form considerable bays, or perhaps, in some cases, entrances into channels dividing the adjoining lands into islands, in no one case were found to approach in such a manner as by any possibility to cause them to appear to meet; on the contrary, there is no position in the whole Strait, where, to an observer sailing in a vessel into it, such a deception could have arisen.

The supposition, therefore, of the existence of a high range of land (Croker Mountains,) which was represented as terminating the supposed bay, seems to have been altogether unfounded, and even without a plausible pretext to justify or palliate the mistake. To the now-ascertained Strait the name of Barrow's Strait was given. In this the water was deep, and clear from ice; but, on entering the Polar Sea, the barrier of ice preventing further progress westward, they bent their course in a southerly direction, and entered a large sound or inlet of about twenty-five miles in breadth, and which, upon its eastern coast, was sufficiently free from ice. (See the Map, Plate III). Having sailed one hundred and twenty miles down this inlet, called *Regent's Inlet,* they were obstructed by ice, and as their object was not to proceed to the south, they returned without further investigating its direction; we believe, however, that there is little doubt respecting its communication with the north of Hudson's Bay; in this inlet, and more especially about its entrance, there were seen numerous black whales of a very large size, as well as many seals and narwhals; indeed we should presume that these unexplored regions are a nursery of these animals. Having again reached the western extremity of Barrow's Straits, the ice had broken up to such an extent, that they were enabled to proceed westwards. Here the sea was open, and clear of ice to the north and to the west, in which latter direction, however, land was still discerned, and the ships pursued their course, passing a number of islands, some presenting precipitous cliffs and rocky, others low and apparently sandy; one of these, of a large size, in about 104° west longitude, they named *Byam Martin Island,* and found upon it the relics of some huts which had belonged to the Esquimaux, and also the horns and some of the bones of musk oxen and rein-deer. Proceeding still westward, they attained, on the 6th of September, by longitude 110°, the meridian of the Copper-mine River. Here they discovered a very large island extending from long. 106 W. to 115, and in latitude 74° 30″ to nearly 76°. This island was called, in honour of the first lord of the Admiralty, *Melville's Island.* From long. 96° to 110° W., the sea appeared free from land, though completely frozen up in a southerly direction (off the N. boundary of America); but land, probably an island, was discerned in long. 112° to 114°, being very distant to the southward. On the 8th of September they attained the longitude of 112° west, and finding the ice rapidly increasing, and every appearance of the commencement of the polar winter, with violent and dangerous north-westerly gales, they retreated on the 22d in consequence of the dangers that threatened the vessels, and on the 26th anchored in a small bay on the south coast of Melville's Island in about five fathoms water, and within two hundred yards of the shore. The latitude of this harbour is 74° 45″ N., and its longitude very nearly 111° W. They experienced considerable difficulty in reaching it, from the rapid accumulation of ice, through which they were obliged to cut a passage for about three miles. Here the ships were imprisoned during a period of three hundred and ten days, for it was not till the 31st of July, 1820, that the ice began to break. Having sailed again on the 6th of August, they reached the west extremity of Melville's Island in long. 114°, when, owing to the immense and impermeable barriers of ice, further progress became impossible, and the ships, returning through the Polar Sea, Barrow's Straits, and Lancaster Sound, into Baffin's Bay, sailed directly homewards.

Such is a brief account of the geographical proceedings of Captain Parry, and his brave associates, and of which the annexed map will offer some further illustrations. (Plate III.)

Of the inclemency of the season which our travellers endured during their sojourn in *Winter-harbour*, we may form some idea from the following facts:

When the expedition anchored in Winter-harbour, the thermometer was at 15° below zero, and, in the course of the ensuing month, it occasionally fell as low as 28° below zero and never rose higher than 17½° above it. But this was trifling to the occasional cold which frequently attained a depression equal to 50° below zero; and, in February, the thermometer indicated the excessive cold of 55° below zero, or 87° below the freezing point of water.

The mercury in the thermometer was continually frozen (and we understand that the spirit thermometers were some degrees at variance with each other, when cooled down to the lowest point, so that a mean of several of them was taken in the above statement). A fine opportunity presented itself of ascertaining the characters of solid quicksilver, with which we have hitherto been but very imperfectly acquainted, in consequence of examining it in small masses only, and at a temperature verging upon its point of fusion. We learn that it possesses the characters of a ductile, malleable, and tenacious metal; that in these respects it appears to rank between tin and lead; that it becomes brittle and easily frangible when near its melting point; and that a piece, of the size of a walnut dropped into a tumbler of warm water, occasioned its instant congelation, accompanied by the fracture of the glass.

It is curious that during such intense colds, no inflammatory diseases made their appearance, and that so few instances occurred of what has been termed *frost-burning*, or mortification of the exposed parts of the body. Only two serious cases of this kind, we believe, occurred; one in a servant of Captain Sabine, whose observatory-house having caught fire, the man was incautiously exposed without having covered his hands; he lost four of his fingers. The other case was of a marine, who lost four fingers of his right hand in consequence of carrying his musket in his bare hand.

Our readers will readily perceive the impossibility of keeping such an exposed place as a ship's cabin, or any part of it, of a tolerably comfortable temperature by artificial means, with an exterior cold of the degree we have stated; and accordingly our travellers had recourse to the careful preservation of their own animal heat, by furs and woollen clothing; they slept under skins in a kind of blanket-bag drawn over their heads, and these precautions, with regular exercise, which was enforced throughout the crew, preserved perfect health; one only death occurred, in the case of an individual whose heart was diseased, and who, probably, would have died at least as soon, at home. Towards the spring some of the sailors suffered slightly from scurvy, which, however, was speedily cured by a copious supply of fresh sorrel, growing on the island, and almost the only esculent vegetable met with in these islands.

On the 11th of November the sun entirely disappeared, and they were deprived of its light for a period of eighty-three days, it re-appearing on the 3rd of February. The horrors of a night thus protracted, in a region barren and desolate in the extreme, covered with snow, and surrounded by immeasurable plains of ice, with an atmosphere of excessive cold, it is difficult to conceive, and impossible to describe; yet amidst these, the gallant crew kept up their spirits by various amusements: they acted plays; published newspapers; and in other ways contrived to pass this formidable and dreary season, of the real horrors of which they became more sensible after it was passed, than during their endurance. The confinement to this single spot for such a length of time, was not wholly unprofitable, since it afforded the opportunity of making various observations on points of science, respecting the form of the earth, and on the various phænomena of magnetism. We understand, that Captain Sabine has arrived at some very curious results upon these subjects, which will probably appear in Captain Parry's narrative. The twilight was sufficient at noon to enable them to read small print in the open air, when the sun was in its greatest southern declination. When the atmosphere was not obscured by drifting snow, the day was like a clear winter's evening, but the moon and stars did not appear more brilliant than is usual in our climate. The *Aurora Borealis* was occasionally visible, principally in the south, and at all hours, but this phænomenon was not peculiarly distinct or brilliant; it would seem, indeed, that our travellers were to the *north* of its splendour. That they were *north* of the magnetic pole of the earth was evident from the direction of the compass needles, which pointed to the southward as they passed the longitude of about 100°.

In April the severity of winter began to relent, and a continuous thaw set in about the end of May; yet it was not till the end of July, that the ice around the ships gave way. Towards the end of May, Captain Parry, with Captain Sabine, and a party of officers and men, proceeded to the examination of Melville Island, and crossed it to the sea on the opposite side. On the west of this island is a considerable bay, called *Liddon's Gulf*, bounded by high and precipitous shores, especially upon its north side. Towards its eastern extremity were found the remains of huts belonging to the Esquimaux. The soil in many places appeared soft and rich; abundance of sorrel was found, with saxifrage and other arctic plants, together with several varieties of grass. The remains of a large whale were also found far inland.

The only quadrupeds which remained during the winter in the islands, were wolves, foxes, and mice, which were occasionally seen even in the severest weather; and hares, which remained torpid. In the spring, musk-oxen, rein-deer, together with abundance of sea fowls and ptarmagans, come from the continent, the quadrupeds travelling over the ice.

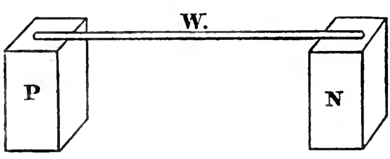
Such are the principal details which we have been able to collect, respecting this highly interesting and successful voyage of discovery, the results of which are in every way highly honourable to Captain Parry and his intrepid associates, and also reflect much credit upon those by whom the expedition was suggested and fitted out.

The public will no doubt look forward with much earnestness to the official publication of Captain Parry's narrative, which, we understand, may be expected early in the spring.

ART. XIV. *On the Connexion of Electric and Magnetic Phænomena.*

No discovery has, for a long time, so strongly excited the attention of the philosophic world, as that of the magnetic phænomena belonging to the Voltaic apparatus; we shall, therefore, endeavour to give our readers a short statement of what has been done in this department of scientific inquiry.

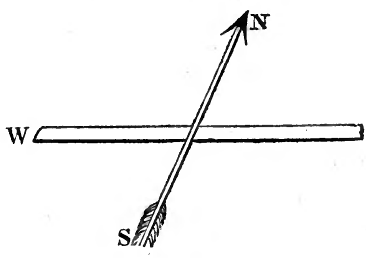
1. If the extremes of a Voltaic battery (we will suppose it to consist of 20 pairs of 8 inch plates,) be connected by a *platinum* wire, it becomes heated, and, if of sufficiently small diameter, it suffers ignition. Let us suppose such a wire, W, lying upon the supports P and N, which represent the positive and negative



conductors of the active Voltaic apparatus, P being connected with the first zinc plate, and N with the last copper plate; upon bringing the north pole of a common magnetic needle *below* and at a right angle to the platinum wire, it will be repelled or driven downwards; if we now remove the needle keeping it in the same position, so that its north pole may be *above* the platinum wire, it will then be attracted towards it. If the electric poles be reversed, these phænomena will also be reversed.

If we suppose the conjunctive platinum wire to be vertical, instead of horizontal, and in that position approach it with either end of the magnetic needle, the needle will *oscillate*, but will not be permanently attracted or repelled by any part of the conjunctive wire.—Professor Oersted.

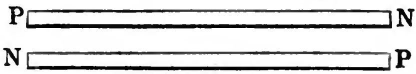
2. If a small steel bar be attached to the conjunctive wire, and parallel to it, it does not become a polar magnet; but if it be attached transversely, it does become polar, and it becomes north and south, or south and north, according to the direction of the supposed electric current traversing the conjunctive wire, according as one or the other end of it is positive or negative. Thus supposing W to represent the platinum conjunctive wire of the Voltaic apparatus, and N S a wire of iron attached



transversely to it, the latter becomes permanently magnetic.—Sir H. Davy.

3. If we suppose a second conjunctive wire parallel to, and similarly situated with, the first, as in this figure, those wires will

attract each other; but if one conjunctive wire be traversed by the electric fluid in one direction, and another in an opposite direction, as in the following wood-cut, those wires will *repel*

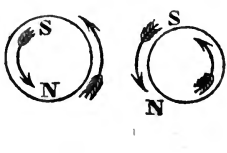


each other. In this circumstance, the dissimilarity of the electro-magnetic and of simple electric phænomena is observed; for bodies similarly electrified repel each other, and, dissimilarly electrified, attract each other; but here the horizontal wires, similarly electro-magnetized, attract; and, dissimilarly electro-magnetized, repel each other.—M. AMPERE.

4. The shock of a Leyden jar, or battery, passed through a wire, confers upon it, at the moment of its passage, properties precisely similar to those of the Voltaic apparatus.

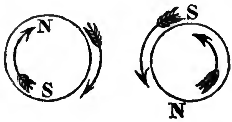
To render a steel bar magnetic, it is not necessary that it should *touch* the conjunctive wire, to which it is attached at right angles, for the electro-magnetic influence is conveyed to some distance, and is not excluded by the interposition of a plate of glass, of metal, or of water.—Sir H. DAVY.

5. The phænomena, exhibited by the electro-magnetic, or conjunctive wire, may be explained upon the supposition of an electro-magnetic current passing round the axis of the conjunctive wire, its direction depending upon that of the electric current, or upon the poles of the battery with which it is connected.Dr. WOLLASTON.



In the above figure, such a current is represented in two sections at right angles to the axis of the wires, when similarly electrified, from which it will be apparent that the north and south powers meeting, will attract each other.

In the following figure, the sections of the wire are shown



dissimilarly electrified, by which similar magnetic powers meet, and consequently occasion a repulsion.

ART. XV.—*Letters of the Cavalier* ENEGILDO FREDIANI, *known amongst the Arabs by the name of* AMIRO, *to the* Marquis of Ischia.

Palmyra, 17 *December*, 1818.

SUBLIME GENIUS!

I COULD not write to Canova with a better title. Before I speak to you of these memorable antiquities, I wish to call to your recollection what I antecedently observed.

Departing with your prosperous omens from the beautiful shores of Italy, I steered along the islands of Elba, Corsica, Sardinia, Ægà, Sicily, and Iperia, with the rock which the carefully-wrought saying of Fenelon rendered more brilliant, and, having discovered Crete, celebrated for its labyrinth and the birth-place of Jove, I finally dropped down upon the long-desired, and to me delightful, land of Egypt.

Turning first towards the Titanian column that bears the name of Pompey, and which rises like a giant more than 100 feet; then to the Watch-tower and Obelisks of Marc Antony, I left the city of Alexandria, and passing by sea the voluptuous Canopus, I arrived at the mouth of the miraculous river, upon advancing into which I felt myself the possessor of a new life. Having viewed the trough of the sea at Bolbitina, the canal of Cleopatra, and the land once made fertile by the Milesians, I arrived in the city of Grand-Cairo, where a well, said to be Joseph's, and an aqueduct, not very modern, detain but little the traveller.

Making but a short stay there, I embarked in the neighbouring Babylonia, and turning away from Rhodes proud of its Nilometer, I found, running upwards, Cimopolis, and the city that calls to remembrance the depraved licentiousness of Adrian, the Lower Abydos, Licopolis, and many other places not mentioned with us.

The picturesque prospect of a thousand cavities called to my mind the anchorites of Thebes.

Following the well-employed journey I observed Abutis, Arroditophopolis, and Tentea, where, in the temple of Isis, I tasted with wonder the Egyptian learning, and turning towards the opposite shore I passed by Coenas, and Little Apollinopolis; reviewing near thereto the city of One Hundred Gates.

Here is Carmak with its boundless walks of sphinxes, the Propylæon, porticos of granite, the courts, the squares, and the temple, with eighteen orders of columns hieroglyphically sculptured, the circumference of which seven men hardly span with their arms.

Luxor with its obelisks and innumerable colonnades.

Behold *Medinet-Abu* covered with endless ruins, and with the monstrous colossus that saluted the appearance of the king of the stars, and still shadows the Theban plain.

Follow and behold Gowm, where the seat of Memnon makes a rich display; and the bright image of the great Sesostris.

But of the tombs of these subterranean abodes, that which an Italian, Giovanni Belzoni, opened last year, under the auspices of Mr. Salt, consul-general of England in Egypt, feeds the doubt, whether it is the production of a mortal hand.

The interior is entered through an ample gate, when a path with walls beautifully sculptured, leads to galleries still more beauteous, by the side of which are the royal rooms, which preserve in diffuse painting the Egyptian mysteries, and the different nations first known. The sanctuary of Isis captivates both the eye and the mind.

Then a superb catacomb of pure alabaster hieroglyfied both externally and internally, rises in the centre of the greater wing, which alone might enrich, and give reputation to a museum. Why were not you with me in that hour when I found in the great Thebes the whole world?

Having so opportune a motive, I directed to you from thence a letter. Tearing myself away as it were by force from the divine Hecatompylos, I passed Armuntis, Crocodilopolis, Latopolis, and Apollinopolis the Great, saluting afterwards amongst its pleasing hills the remote Syene.

Having visited the temples of that frontier, and the well that was the looking-glass of the sun, and the island Elephantina (or Elephantine Island,) the abode of Emefet, I joined the illustrious party of my lord Belmore, intent upon visiting Nubia, and having passed the last cataract, improperly called the first, the caves of granite, and the sumptuous edifices of *Philoë*, &c., reached *Sieg Ibsambal* the antient Aboceis, abandoned to Petronius by the unfortunate Candace, and where is still the best monument of Ethiopia, re-opened by the order of the aforesaid Mr. Salt, by our Belzoni, and by us another time when the Nisis had covered it with sand. The name of Mr. Salt is dear to the republic of the literati, and to amateurs of travels, by calling to their remembrance the interesting accounts of Abyssinia.

From Ibsambal passing over to Ischiet, we met Daud Kaschef, one of the seventy children of Hassan, who received us with an agreeable politeness, under a throne of palms in a field. Oh, if you had seen how different from our own are the customs of the people of Nubia!

Here captain Correy, brother of lord Belmore, and myself, were seized with the desire of passing the penultimate cataract, in order to arrive by the way of Sennar at the pleasant island of Meroë, which is the Saba conquered by Moses before the high mission, when under the name of Sontifanti he enjoyed high credit at the court of Pharaoh.

We were immersed in the new project, when some people of the provinces subject to the Grand Negus told us, that the Mamelukes confined in Dongola by the brave Mahomet Ali, notably suspected all those who came from Egypt; wherefore we retroceded, and the 26th December, 1817, I cut out the name of *Ilias* and my own, upon the highest top of the cataracts of Nubia.

That river which fertilizes so many kingdoms and makes them blessed, is here divided into millions of various streams which, gushing out from amongst the stones, and folding into heaps of flowers, form to the eye a spectacle not elsewhere known in nature.

Having met with, under the torrid zone, the points of the antient Phthuris, Assciga, Yicroseia, Corthes, Pselchas, Thutzis, Talmis, Taphis, and Thitzi, and having returned to Syene, I soon directed my steps towards Ombos Sacra, to Crocodille, to Stilithia, Enubis, to Koptos, the friend of the maritime Berenice, and which experienced all the rigour of Dioclesian, to Diospolis the Little, Abydos the Great, which preserves considerable remains of the temple of Osirides, to Panopolis, Antinopolis, Emopolis Magna, Tanis Superiore, and to Oririneus in Siut, where I met with the count Forbin, French traveller.

Spending some time in *Radamore*, where is the distillery of rum and a sugar bakery, under the direction of the hospitable Mr. Brine, I went down to the pyramids of Saccara, and by the plain of Memphis to those of Ghizeh, where I found Mr. Belzoni anxious to penetrate into the second of those heaps, thought to be of Cephrenus; knowing his intelligence, I endeavoured only to animate him still more to the undertaking, and after *bivouacking* some days, we traversed a place inaccessible for many generations; and, I know not how to express my feelings at wandering amongst those shades.

A very long inclined gallery entirely of fine and massy granite; a passage at the end so narrow that a man bending horizontally can hardly enter: then a horizontal gallery which looks into the hall where is the tomb worn away; a perpendicular gallery somewhat inclined with a room on the left side of the passage; various collections of saline productions figured upon the walls; various inscriptions; and, finally, crosses designed upon these same walls: this is what we saw.

Emerging from this delirium to the light, I wished to ascend the highest pyramid, and arrived at the top, I appeared to touch the stars: I remained there the whole night, which was the best of my life. Forty centuries had been silent under my feet, whilst I was ponderating the cause and effects of the creation.

The following morning the rising sun illumined me, which shone around the horizon with a pomp never dreamt of, either by painter or by poet.

From this place I wrote to you, to Dionigi, Morghen, Bartolomei, Pindemonte, Morichini, Ferroni, Vacea, Scarpellini, Camellieri, Delfico, to the cardinal Consalvi, to the chevalier Fossombroni, and to other lights and souls of my country.

I have scarcely mentioned to you the celebrated woman of Mizraim; she has been a prey to all the scourges of time, so that we can only write upon her remains, " Here was Memphis !"

Turning from the pyramids I entered into Grand-Cairo, and thence down to Alexandria, in order to expedite to you the plan of my researches: for you and the Regent of England were the first to second my efforts.

During my above-mentioned sojourn, I went to pay homage to the man who governs Egypt, worthy of being inserted in the pages of history by the side of Mœris and Menes, or with Evergetes and Ptolomy, son of Lagos.

Returning to Grand-Cairo I repaired to Asia: and plunging into the deserts of Etam and those of Kedar, to see on one side Pharan, and on the other Casinus, which includes in its bosom the bones of the great Roman yet unrevenged.

As I left, and Egypt was deserting me, I was reminded what Amru wrote to the great Omar, desirous of a picture of that country: figure to yourself, O Prince of the Faithful, a vast and arid desert, with a river in the middle which is attended in its course by two opposite hills, the borders of the ground rendered fertile by that flood so blessed by Heaven. Most just is the picture, and in that too which afterwards follows.

Continuing by single stations I passed the isthmus of Suez, and the fragments of Rinocerure, Raffia, and Agrippiades, and leaving behind me Besor, I comforted my weary eye with the olives of Gerar, the happy land of the Philistines.

Departing from Gaza I went to Beer-sheba, to Sorek, upon the borders of which grew Dalilah, to Timnath and Gabaton, known already by the feats of Samson, and getting out of the way of the tribe of Simeon, I advanced into the mounts of Judah and Benjamin, arriving by the plain of Booz at Jerusalem, in the very time of the Greeks demanding from Heaven their sacred fire.

At the view of the hills of Sion and the Olive, at the appearance of the holy city, I felt myself, both as Christian and philosopher, touched by an hitherto unfelt emotion, which, somewhat retarding my steps, covered my heart with pleasing melancholy, and my mind with incessant meditation. Oh! what a difference between the figurative and the true.

Having reverenced those places which record the beginning of the greatest religion in the world, I contemplated with indescribable transport, the Tower of David, the Temple of Solomon, the Palace of Herod, the Fountain and the Pool of Siloah, the Proving Bath, and that of Beer-sheba, the Kedron, the Golden Gate, the Well of Nehemiah, which concealed the true fiery element, the Mount of Offence, and that of Scandal, with the Valley of Tophet, where the priests of Israel sacrificed human victims to Moloch; the Sepulchre of Manasseh in the Garden of Uzza, the Sepulchres of the Kings, and those of Absalom, of Jehoshaphat, of Zachariah, son of Barachiah; the only architectural objects I thought worthy of you amongst the modern antiquities of the Hebrews.

You are never satiated with delight over the ruins of Jerusalem, and taking the advantage of a company of pilgrims, I went with them to Bahurim, whence Shimeis threw the stones at the Psalmist, in Adummim, or Place of Blood, to the Fountain of Elijah, to Jericho, which no longer gives odour to the chaste flower, down to Gilgal; I cleansed myself in the Jordan at Bethabara, where John baptized.

Before me were Reuben and Gad, with the Plains of Moab, and the Land of the Amorites.

Amongst the crowd of pilgrims were distinguished the Britons, Bengs, Mangles, Irby and Legh, and the exemplary companion of the Italian Belzoni.

Returning to Jerusalem, I was present at the tragic quarrel which occurred between the Greeks and Latins, near the Tomb of Jesus Christ. I wrote to the hero of the pontificate, exhorting him to interfere, in order that, in future, such scandalous occurrences might not happen.

I then undertook another journey, and the places I saw were the Valley of the Giants, the Lands of Jacob, the Sepulchre of Rachel, near Ramath, the Cistern of David, Bethlehem, a smiling town of Judea, the Villa of the closed Garden, the sealed Fountain, and the vessels of Solomon; the Hills of En-gaddi, Tema, the country of Almos; and Giloh, country of Ahithophel; the Grottos of Adullam, and the Wood of Ziph, where the successor of Saul, David, often hid himself; the Valley of Mamre, the Field of Damascus; whence re-proceeding, the Vale of Terebinthus, fatal to Goliath, and the surrounding places renowned by the nativity and abstinence of the Precursor. I, lastly, saw Bethany.

Having drawn from the library and the archives of the friars what I thought of service to my purpose, I bid adieu to the Daughter of Sion, and by the Pool of Gibeon, Beth-horon, Succoth, the Valley of Rephaim, Azekah, Emmaus, Anathoth, the country of Jeremiah placed against Modin, the Glory of the Maccabees, and by Aramathia, passing Sharon, I stopped at Joppa, which still boasts of its rocks warm with the tears of Andromeda. Here approached the Tyrian ships, bearing the precious stores and purple which the son of Abibal sent to the sapient king, and here, too, daily approaches the pilgrim led from afar to pay the vow.

From Joppe I went by the left bank to Ekron, Ashdod, which kept the ark a prisoner, to Ashkelon destroyed, and having returned to Joppa, I ascended the inheritance of Ephraim to the Sepulchres of Benjamin and Simeon; to Sichem, whence we mounted Ebal and Gerizim, to the Well of Jacob, and the sepulchre of Joseph; and meeting with the Abbé de Mazure, a warm panegyrist of France, and measurer of Judea, I went with him to Silos, upon the road that leads from Jerusalem to Napolis.

Napolis, or Napolosa, lies upon the ruins of Sichem, and here, returning from Silos, I found the ancient Samaritans, or Cuteans, who were praying, from error, to a well, believed to be Jacob's. I taught them the truth, which doctrine excited against me no small disturbance; so far, that the said Samaritans, thinking me one of their brethren, wished by all means to retain me in the country; and what is more singular, exacted that I should promise marriage to a woman of their sect.

The Christians of Napolosa took up my defence; whence, getting off at my own hazard, foreseeing the favour of the former, I took shelter in Samaria, where there is no vestige of the importunate Samaritans. I wrote to you, that, with the exception of some columns there is nothing interesting in Sebaste.

On leaving Samaria the tribe of Issachar presented themselves to me in Galilee, with the fountain of Israel, and plain of Esdraelon, over which the eye cannot reach; Endor, at the foot of the second Hermon, known by the victory of Deborah and Barak. Sophos, the native place of James and of the friend of his master; Cana, the country of Simon and Nathanael; Tabor, terminating with Heaven; beautiful parts of Zabulon; Bethsaida, the country of Peter and Andrew on the shores of that water, abundant in the deeds of the Divine Instructor of virtue.

Returned to Tiberias I undertook the analysis of those mineral waters; and in the city where lives, in retired delight, that deserving man of society, the noble gentleman (cavaliere) Raphael de Piciotto, consul-general of Austria in Syria, whose roof and whose fortune never denied to any one a constant sacred hospitality.

And you must know, *à-propos*, that amongst the Hebrews dispersed in the various regions of the globe, and amongst those of Asia and of Africa particularly, there exists an ancient custom of coming to finish their days upon the spot, bathed by the sweat of their ancestors. Such a sentiment gladdens their heart from the most tender years of youth, and hence it is moving to see arrive in the ports of Palestine, the aged Israelite, who leaning upon the shoulder of his old consort, approaches with her amidst the cheers of hope, to deposit his ragged spoils in the sepulchre of their forefathers.

The heat suffered upon the lake of Gennesareth having moderated, I revisited the tribe of Issachar, and having ascended the Carmel I dropped down to Caifas, to Dora, to Cesareth, to Manasseh; and passing in the Tribe of Asher over the space of Semeron and the Waters of Cenderia, I continued afterwards the Delo to Ptolemais, still dyed with that blood which the cruel Djezar caused to flow in torrents.

Thus following the course of the Phœnician shore, every moment appeared to me an age which interfered with that which should show me in a miserable rock, surrounded with water and with sand, that so powerful mistress of the seas.

The Greek Archbishop, D. Cirillo Debbas, receiv ed me cordially in his house, and causing to be prepared a frugal repast, placed on the ground after the fashion of the East, and setting himself down beside me, spoke as follows :—" Eat with good-will that God may preserve it to thee. I receive thee negligently after the manner of the apostles, and this scanty food I consume with thee in good-will, as I do daily with the other guests. If I had more I would give thee more, but my only income, which is that of the Archbishoprick of Tyre, does not produce me annually above 200 crowns (scudi) of thy country, the half of which I employ to nourish the poor of my diocese. Besides being their spiritual, I am also their temporal, physician, and lend gratuitously my remedies wherever they are necessary. The other prelates live more secure under cover of the mountains, but I am more fortunate than they are, who divide with my flock the days of sorrow and of joy." May those be blessed who speak and reason with so much truth.

Leaving Tyre with the benedictions and sincere embraces of my host, I passed the Well of Living Waters, the Pseudo Eleutherius and Sarepta, when the smiling plain of that Sidon opened itself before me which struggled hard with its approaching fall. Monsieur Ruffin, French Consul, politely offered me reception, and I deplore the loss he has since sustained in a companion who was the model of the tender sex.

My Lady Esther Stanhope, who, for so many years, has attracted the attention of Asia and of Europe by the singular manner of life she has adopted, is encamped one hour's distance from Sidon, in a small habitation called Ceruba ; and, in order to render herself still more remarkable, insists upon her will being obeyed, that no European shall approach her, even for a moment. To blame her for it, would it not be an act of intolerance?

Traversing that mountain which includes so many mountains, and may properly be called a kingdom, and which I shall call Libania, I hastened forward to Cilicia, and thence to Damascus, the name of which imposes more than is due to it.

In all the circuit of the Libanus, as well as in the Carmel, I collected a thousand fruits and petrified testaceous substances, the proof of a tremendous deluge.

My intention of going from Damascus to Palmyra not succeeding at that time, I came to Balbek, where it appeared to me as if Thebes were revived in the midst of Siria.

An entire volume would be insufficient for the description of the Temple of the Sun.

Six columns arise amidst the marshes, each in height seventy-one feet, and twenty-one feet eight inches circumference. Three stones of granite occupy the space of one hundred and seventy-five feet and a half, and another has sixty-nine feet of length, twelve of breadth, and thirteen of thickness. You alone, Sublime Genius! can solve the problem whether it is the work of common men, or of a race of beings superior to our own.

Re-ascending the Libanus I wished to smell its boasted cedars, see Eden, the grottos of Canobin, and the horrible cave of the great Egyptian Asceta. Oh, how the pure and sweet life of the patriarchs flourishes here! Here is that simplicity and peace that man in vain seeks amongst mankind.

Again returning to Phœnicia I went to Tripoli, to Tortosa, witness of the great congress in the first crusade; to Eleutherius, Sober; to the city of Gabale, which preserves one of its amphitheatres; to Laodicea, where the Signor Agostino Lazzari entertained me with more than social treatment; and penetrating amongst the mountains of the Arsarites, worshippers of dogs and of the base senses, I arrived at the Milky Waters of Orontes and Antioch, an object worthy of contest.

From Theopolis, by a road covered with abusive inhabitants, I came to the more flourishing Aleppo, thence to the Euphrates, and hardly touching Mesopotamia, the sound of Nineveh and Babylon already struck my fancy, and drew it away more rapidly than the steed of Elimaides the chariot of Cyrus.

Passing again through Aleppo, I kept the other road of Damascus by Apamea, Cima, and Emesa, where the delicately blond-haired, white-complexioned nymphs, display themselves, with their black eyes, more beautiful than whom were never produced by the native of Urbino or by Titian.

Whilst I was enjoying the presence of Emesa, the catastrophe of the Palmyrenes came to my memory and the blood of the just Longinus almost drew from me a tear.

Warmly recommended to the governor of Damascus by the excellent Piciotto, consul-general of Austria in Aleppo, a son worthy of his father, I advanced towards Palmyra in company with an only guide, and after five days of most troublesome journey, reposed in the court of Odenatus and Zenobia.

But what can I tell you of this memorable spot which so much electrifies the intellects, unless that about thirty towers, the Temple of the Sun, and three hundred columns scattered here and there, over a soil covered with sand, are still standing to eternize to the world the great Palmyra? What I pass over in silence shall blossom in my future little work.

Affectionate to the glory of your name, I was spurred on to accomplish my enterprise, and having cut out *Ilias* upon a marble, added the following:—*Frediani stima degne le Rovine di Palmira del genio del divin Canova!*

But why do not you come also to admire the genius of the nations who figured from time to time on this planet?

In fifteen months, and about 7,000 miles, I have passed through the Mediterranean, Misraim, Nubia, Kedar, Idumea, Philistia, Judea, Samaria, Galilee, Phœnicia, Cilicia, Syria, and Mesopotamia, having seen the sea of Pentapolis, have drank that of Tiberias, and the Nile, the Jordan, Orontes, and Euphrates; have ascended the Pyramids, Sion, Gerizim, Tabor, Libanus, and Carmel, and have reposed in the tombs of Thebes, amongst the Cataracts of Nubia, and upon the dust of Memphis, Heliopolis, Ashkelon, Tyre, Sidon, Balbek, Palmyra, Samaria, and Jerusalem.

Would it not be a precious thing for posterity one day to read, *Canova in Egypt*, *Canova in Syria*, *Canova in Palmyra!*

Oh no! pardon the flights of a mind for a moment from the sacred centre of its country, but which returned to itself, sends forth a vow that posterity may read alone; *Canova in Italy*, *Canova upon the Tiber*, *Canova in Rome!!!*

AMIRO.

Second Letter of AMIRO *to* CANOVA.

Upon Mount Sinah, May 8, 1819.

SUBLIME GENIUS!

I WRITE to you from the most memorable heights in the universe, but hear how I came here.

Having closed the letter, I directed to you from the ruins of Palmyra, I followed the silent contemplation of those remarkable remains, and under the protection of the hospitality of the modern Palmyrenes, who are the best Arabs I know of, I passed hours joyful and tranquil.

Their questions turned upon *Boneborte* (Bonaparte) and my Lady Stanhope; the former they remembered from his expedition into Soria, for the fame of him resounded greatly amongst them, and the latter for the prodigality displayed in the journey she undertook in the desert.

Their curiosity, and my own being satisfied, I continued my journey with my guide, and arrived at Damascus. Thence, through Cilicia, I ascended Libanus once more, which I was delighted to contemplate amidst the horrors of the winter, and, descending to the Beritus by Phœnicia, the pleasant Philistia, and the wearisome Etam, I returned to the Nile.

After one day's repose, I went to offer my personal tribute to the Pyramids, and *à-propos* of these heaps, whilst I was writing my name upon the third called Phrine, I perceived that *Frediani* was the anagram of *Dia Frine.*

I then returned to Cairo, and as the pestilential scourge was beginning to mow down human victims, instead of remaining there I thought better to continue my journey, and three days of sand made me ejaculate *Dulce videre Suez.*

Having admired the progress and decrease of the waters, I put myself on board an India ship, commanded by the excellent Captain Laudale; and embarking afterwards in a small boat, I sailed as far as *Der Essafran,* where it is believed that Israel passed over, and traversing almost in right line the famous sea, I approached *Del el Hamman.*

Departing by the waters of Suez, I had ordered my Arabs to wait for me at a place indicated, and judge of my surprise upon my arrival to find no one there!

The solitude of the place, the inefficacy of the bark to continue as far as Tor, the wind contrary for my return to Suez, the want of provisions and water particularly, were the mournful thoughts that sat heavy at my heart.

But that immutable eternal Providence, ever present where he least appears so, but where most necessary, caused in an instant my guides to approach; whence by the path of the people elect, I trod upon Paran and Sin, and sighing, arrived at the sides of these mountains, which are Sinai and Horeb.

The first idea I conceived when for the first time I heard of Mount Libanus, was that of an isolated mountain, and in such respect all the ideas of men are alike, whence I shall call it the Country of Libany instead of Mount Libanus; that country as large almost as our Abruzzo, and larger than our Tyrol, which comprises luxuriant valleys, fertile meadows, flowing rivers, beautiful hills, very high mountains, populous towns, ten bishoprics, seventy principalities, and which can produce 50,000 champions for the protection of its precious liberty.

Third Letter of Amiro *to* Canova.

Cairo, December 1, 1820.

When I write to Canova I think I am writing to my native country, for you are its first-born.

Leaving Horeb and Sinai, from the summits of which I gazed at lands which form lucid points in the blaze of human intellect, I descended into the country of Elim, where still are to be seen the wells and the palms that quenched the thirst of the Jews.

Having cooled myself in Tor, where I tried its waters, I returned by the road of Suez to Cairo, and going down to Alexandria, I turned towards the Lake Mareotis, thence to that of *Madiar* and Edeo, and making an excursion in merry company to the beautiful Rosetta, I traversed the branch Bolbitina, the Delta, and arrived at the ruins of Batis, and the mouth of Sebene upon the branch of Fammeticus in modern Damiatus.

Embarking thence upon the Lake of Memale, and arrived at the islands of *Mataria*, I advanced into the canal of Moez, whence I might view the scattered remains of Tanis, and returning to the lake, recognised the mouths Taniticus and Pelusiacus, with the *Rogasus of Rahi.*

Disembarked upon the shore, I arrived through the desert at the sides of the mountain Casius, and the day following ascended that celebrated eminence, whence I came to Pelusium, that famous key of Egypt, and trusting myself once more to the waves, I visited the islands of Tennis and Thuna, and passing over the mouth of Mendesius, I returned to Damietta.

Reposing a little, I took diversion upon the lake, and penetrated by the canal of Moez into that of Salahie, and descending into the desert, I found endless fields of soda, both vegetable and mineral.

Whence approaching towards the Nile, I arrived by the canal of *Asmun*, at the city of Benhi, the antient Mendes; thence upon the branch Fammeticus to the bed fatal to Louis IX.; and finally returned to Grand Cairo.

Now that, thanks to the magnanimous Viceroy of Egypt, the brave Mahomet Ali, and his faithful minister Burgoss Jusuff, I am furnished with ample and generous means of penetrating into spaces shut up by the seal of ages, I am preparing to approach the torrid zone, where I hope to shew to Italy that I am not entirely unworthy of belonging to her.

Art. XVI. *Proceedings of the Royal Society.*

The meetings of the Royal Society were resumed after the long vacation, on Thursday, November 9, at which meeting a letter was read from M. Ampère, of Paris, to the President, containing an account of some electro-magnetic experiments, which our readers will find adverted to in a preceding article.

On the same evening Sir Everard Home communicated some observations on the influence of the black substance in the skin of the negro, in preventing the scorching operation of the sun's rays. As black surfaces become much warmer by exposure to the sun's rays than those which are white or of paler tints, the cause of the black colour in the negro has long appeared problematical to the physiologist. In this paper, Sir Everard shows, that by exposing the back of the hand and other parts of the body, covered with thin white linen, to the direct influence of the sun's rays, they became irritated and inflamed; small specks or freckles first appear, and these, on continued exposure, are followed by a vesicular separation of the cuticle; the same happens when the bare surface is exposed, which, in common language, becomes *sunburnt*; when, however, the part of the body thus exposed is covered with a piece of black crape, though the temperature of such part, when exposed to the bright sunshine, exceeds that produced upon the bare skin, the scorching and blistering influence of the rays is entirely prevented; hence it appears, that the deleterious effects of the sun's rays are prevented by an artificial blackening of the surface of the skin; that perspiration becomes more copious, as is especially remarked in the negro; and, in short, that the conversion of the radiant matter of the sun into sensible heat, which conversion is effected by the black surface, tends to prevent the scorching effects, and to promote the cuticular secretion.

Thursday, Nov. 16.—A letter was read from Sir H. Davy to the President, on the magnetic effects produced by electricity; to part of this letter we have elsewhere adverted.

Thursday, Nov. 23.—The Earl of Morton communicated, in a letter to the President, a singular fact in Natural History. Some years ago his lordship imported a male Quagga, or Quacha, from the Cape of Good Hope; he bred from this animal and a young chestnut mare of Arabian blood, and the result was a female hybrid, bearing both in form and colour, decided indications of her mixed origin. The same mare passed into other hands, and was afterwards bred from by an Arabian horse; and this progeny, though possessed of the leading characters of the Arabian breed, retains in certain stripes and marks of the body and in the hair of the mane, certain characters decidedly belonging to the quagga, characters which appear to be transmitted to the progeny by the exclusive influence of the mare.

Thursday, Nov. 30.—This being St. Andrew's day, the Society proceeded, according to annual custom, to the election of a President and officers for the year ensuing. Dr. Wollaston, who in consequence of the death of Sir Joseph Banks, was appointed by the Council to succeed to the President's chair until the anniversary, signified at that time his decided intention of withdrawing from the office upon St. Andrew's-day. The Scrutators having duly examined the ballotting lists, announced the result of the election as follows:—

PRESIDENT.

Sir Humphry Davy, Bart.

TREASURER.

Davies Gilbert, Esq. M. P.

SECRETARIES.

William Thomas Brande, Esq. Taylor Combe, Esq.

COUNCIL.

Dr. Ash
W. Blake, Esq.
Earl Brownlow
Bishop of Carlisle
J. G. Children, Esq.
Sir G. Clerk, Bart.
H. T. Colebrooke, Esq.
J. W. Croker, Esq.
C. Hatchett, Esq.
J. F. W. Herschel, Esq.
Sir E. Home, Bart.
Captain Henry Kater
J. Planta, Esq
J. Pond, Esq.
Earl Spencer
Dr. W. H. Wollaston
Dr. T. Young, *For. Secretary.*

At this meeting the President announced that the Council had determined to present the Copley medal to Professor John Christian Oerstedt, of Copenhagen, as a testimony of their approbation of his electro-magnetic discoveries. Dr. Wollaston then, in a luminous and eloquent discourse, stated to the Society the nature of Professor Oerstedt's investigations, and pointed out their probable influence upon the future progress of some of the most difficult branches of physical science.

Thursday, Dec. 7.—On taking the chair, the President, Sir H. Davy, proceeded in a short discourse, to point out the objects of the Royal Society in particular, and its relation to various other scientific institutions, assembled for the purpose of pursuing individual branches of inquiry. He then adverted to the present state of the Sciences, and to the important part taken by the Fellows of the Royal Society in their improvement and extension; and enumerated the different subjects of natural knowledge that stood most in need of accurate research, in nearly the following terms :

" In pure mathematics, though their nature, as a work of intellectual combination, framed by the highest efforts of human intelligence, renders them incapable of receiving aids from the observation of external phænomena, or the invention of new instruments; yet, they are, at this moment, abundant in the promise of new applications; and many of the departments of philosophical inquiry which appeared formerly to have no relation to quantity, weight, figure or number, as I shall more particularly mention hereafter, are now brought under the dominion of that sublime science, which is, as it were, the animating principle of all the other sciences.

" When the boundary of the Solar System was, as it were, enlarged by the discovery of the Georgium Sidus, and the remote parts of space accurately examined by more powerful instruments than had ever before been constructed, there seemed little probability that new planetary bodies should be discovered nearer to our earth than any of those already known; yet this supposition, like most others, in which our limited conceptions are applied to nature, has been found erroneous. The discoveries of Piazzi, and those astronomers who have followed him, by proving the existence of Ceres, Pallas, Vesta, and Juno, bodies smaller than satellites, but, having the motions of primary planets, have opened to us new views of the arrangements of the Solar System. Astronomy is the most antient and the nearest approaching to perfection of the sciences; yet, relating to the immensity of the universe, how unbounded are the objects of inquiry it presents, and amongst them, how many grand subjects of investigation; such for instance, as the nature of the systems of the fixed stars, their changes, the relations of cometary bodies to the sun, and the motions of those meteors, which in passing through our atmosphere, throw down showers of stones: for, it cannot be doubted, that these bodies belong to the heavens, and that they are not fortuitous or atmospheric formations; and in a system, which is all harmony, they must be governed by fixed laws, and intended for definite purposes.

" The grand question of universal gravitation, and its connexion with the figure of the earth, has been long solved; but the mechanical refinements of one of our Fellows have afforded means of estimating with more perfect exactness the force of gravity;—and that pendulum which is so well fitted as a standard of measure, may be admirably applied to acquaint us with the physical constitution of the surface of the earth. I trust we shall have some interesting new experiments on this subject. Our brethren of the Royal Academy of Sciences of Paris, who have laboured with so much zeal and activity towards the measurement of a great arc of the meridian in France and Spain, are, I know, extremely desirous their measures should be connected with those carried on by the command of the Board of Ordnance in Britain; that the work should be completed by the philosophers of both countries. Should this be done, there will be established, on the highest authority, an admeasurement of nearly twenty degrees, or $\frac{1}{18}$ of the whole circumference of the earth, from the Shetland Islands to Formentera, which will be a great record for posterity, and an honour for our own times.

" I cannot pass over the subject of the figure of the earth," continued Sir Humphry, " without referring to the late voyage to the Arctic Regions, which has shewn that there is an accessible sea to the west of Baffin's Bay, presenting hopes of greater discoveries, and which has terminated in a way equally honourable to those by whom the expedition was planned, and to the brave, enterprising, and scientific navigators by whom it was executed. Such expeditions are worthy the greatest maritime nation of the world; shewing, that her resources are not merely employed for gaining power or empire, but likewise, for what men of science must consider as nobler purposes, in attempting discoveries which have the common benefit of mankind for their object, and the extension of the boundaries of science.

" In the theory of light and vision, the discoveries of Huygens, Newton, and Wollaston, have been followed by those of Malus; and the new phænomena of polarization, which we owe to the genius of that excellent and much-to-be-lamented philosopher, are constantly leading to new discoveries: and notwithstanding the important labours of Arago, Biot, Brewster, and Herschel, the inquiry is not yet exhausted; and it is extremely probable that these beautiful results will lead to a more profound knowledge than has hitherto been obtained concerning the intimate constitution of bodies, and establish a new connexion between mechanical and chemical philosophy.

"The subject of heat, so nearly allied to that of light, has lately afforded a rich harvest of discovery, yet it is fertile in unexplored phænomena. The question of the materiality of heat will probably be solved at the same time as that of the undulatory hypothesis of light, should the human mind ever be capable of understanding the causes of these mysterious phænomena. The applications of the doctrines of heat to the atomic or corpuscular philosophy of chemistry, abound in new views; and probably at no very distant period these views will attain a precise mathematical form. There are many remarkable circumstances which seem to point to some general law on the subject. First,—the apparent equable motion of radiant matter, or light and heat, through space:—2, The equable expansion of all elastic fluids by equal increments of temperature:—3, The contraction or expansion of gases by chemical changes, in some direct ratio to their original volume; for instance, $\frac{1}{2}$ or $\frac{1}{4}$:—4, The circumstance that the elementary particles of all bodies appear to possess the same quantity of heat.

"In electricity the wonderful instrument of Volta has done more for the obscure parts of physics and chemistry, than the microscope ever effected for natural history, or even the telescope for astronomy. After presenting to us the most extraordinary and unexpected results in chemical analysis, it is now throwing a new light upon magnetism:

"Magnos accinctus in usus."

But upon this question I shall enter no farther, as it has been discussed in the discourse given in the award of the Copleian medal to M. Oersted, by my predecessor in office, with all his peculiar sagacity and happy talent of illustration.

" To point out all the objects worthy of inquiry in chemistry, would occupy the time appropriated to many sittings of the Society. I cannot, however, avoid mentioning amongst important desiderata, the knowledge of the nature of the combinations of that principle existing in fluor or Derbyshire spar, and which has not yet been obtained pure; the relations of that extraordinary fact, the metallization of ammonia; and the connexion between mechanical and chemical phænomena in the action of voltaic electricity. I must congratulate the Society on the rapid advances made in the theory of definite proportions, since it was first advanced in a distinct form by the ingenuity of Mr. Dalton. I congratulate the Society on its progress, and on the promise it affords of solving the recondite changes owing to motions of the particles of matter, by laws depending upon their weight, number, and figure, and which will be probably found as simple in their origin, and as harmonious in their relations, as those which direct the motions of the heavenly bodies, and produce the beauty and order of the universe.

" The crystallizations or regular forms of inorganic matter are intimately connected with definite proportions, and depend upon the motion of the combinations of the elementary particles: and both the laws of electrical polarity, and of the polarization of light, seem related to these phænomena. As to the origin of the primary arrangements of the crystalline matter of the globe, various hypotheses have been applied, and the question is still agitated, and is perhaps above the present state of our knowledge; but there are two principal facts which present analogies on the subject: One, that the form of the earth is that which would result, supposing it to have been originally fluid; and the other, that in lavas, masses decidedly of igneous origin, crystalline substances similar to those belonging to the primary rocks, are found in abundance.

" In following the sensible phænomena of nature from the motions of the great masses of the heavenly bodies, which first impress the senses and affect the imagination, to the changes individually imperceptible, which produce the phænomena of crystallization, there is a regular gradation, and a series conformable to analogy; and, where crystallization ends, another series, that of animated nature, begins, governed by a distinct set of laws, but obedient to a principle, the properties of which, independent of matter, can never be submitted to human observation. The functions and operations of organized beings, however, offer an infinite variety of beautiful and important objects of investigation. For instance, in those refined chemical processes, by which the death and decay of one species afford nourishment for another and higher order; by which the water and inert matter of the soil and the atmosphere are converted into delicately organized structures, filled with life and beauty.

"In vegetable physiology, how many phænomena still remain for investigation; the motion of the sap, the functions of the leaves, for instance, and the nature of the organs of assimilation.

"In animal physiology the subjects are still more varied, more obscure, and of a higher character. May we not hope that those philosophers of the schools of Grew and of Hunter, who have already done so much for us, will not cease their efforts for the improvement of those branches of science, which are not merely important in their philosophical relations, but of great utility, the one to agriculture, and the other to medicine."

The President concluded by expressing his confidence, that the Fellows of the Royal Society, in all their future researches, would be guided "by that spirit of philosophy, awakened by our great masters, Bacon and Newton; that sober and cautious method of inductive reasoning, which is the germ of truth, and of permanency in all the sciences. I trust," he said, "that those amongst us who are so fortunate as to kindle the light of new discoveries, will use them not for the purpose of dazzling the organs of our intellectual vision, but rather to enlighten us by shewing objects in their true forms and colours.

"That our philosophers will attach no importance to hypotheses, except as leading to the research after facts, so as to be able to discard or adopt them at pleasure; treating them rather as parts of the scaffolding of the building of science, than as belonging either to its foundations, materials, or ornaments:—That, they will look, where it be possible, to practical applications in science; not, however, forgetting the dignity of their pursuit, the noblest end of which is to exalt the powers of the human mind, and to increase the sphere of intellectual enjoyment by enlarging our views of nature, and of the power, wisdom, and goodness of the Author of nature."

Thursday, Dec. 14.—A paper was communicated by the Secretary, detailing the composition and properties of some new compounds of chlorine and carbon, discovered by Mr. Faraday. He has ascertained that, by exposing carburetted hydrogen, mixed with great excess of chlorine, to the action of light, a white crystalline substance is formed, which, when purified by washing with water, is a perchloride of carbon. This substance is nearly tasteless; its odour resembles camphor; its specific gravity is about 2; it is a nonconductor of electricity. It is volatile, and in close vessels fuses at 320°, and boils at 360°. It is not very combustible, but burns when held in the flame of a spirit lamp, with the emission of much smoke and acid fumes. It is insoluble in water, but readily soluble in alcohol and ether; these solutions deposit arborescent and quadrangular crystals. It also dissolves in volatile and fixed oils. It is scarcely acted upon by alcaline and acid solutions; but most of the metals decompose this substance at a red heat. Potassium burns brilliantly in its vapour, causing the deposition of carbon, and the production of chloride of potassium. The metallic oxides also decompose it at high temperatures, producing metallic chlorides, and carbonic acid or oxide, according to the proportion of oxygen present; no water is produced, showing the absence of hydrogen in the compound. It appears, from various analytical experiments upon this compound, among which may be mentioned its decomposition, by passing it through red-hot peroxide of copper, that 100 parts afford 10 carbon + 90 chlorine; whence it would appear to consist of

2 Proportionals of Carbon	5.7 × 2 =	11.4
3 Ditto Chlorine	33.5 × 3 =	100.5
		111.9

Thursday, Dec. 21.—The reading of Mr. Faraday's paper was continued and concluded. When the perchloride of carbon is passed through a red-hot tube, containing fragments of rock-crystal to increase the heated surface, it gives off a portion of chlorine, and is converted into a liquid protochloride of carbon. This is a limpid colourless fluid, specific gravity 1.55, and not combustible, except retained in the flame of the spirit-lamp, when it burns with a yellow flame, much smoke, and fumes of muriatic acid. It does not congeal at 0°; it rises in vapour at about 165°. It is insoluble in water, but soluble in alcohol, ether, and the oils. It is not affected by the acids or alcalis, nor, at common temperatures, by solutions of silver. It dissolves chlorine, iodine, sulphur, and phosphorus. It affords, when decomposed, 17 carbon + 83 chlorine; whence it may be inferred to consist of

1 Proportional carbon	=	5.7
1 Ditto chlorine	=	33.5
		39.2

Mr. Faraday has also found, that when iodine and carburetted hydrogen are exposed to the action of light, they combine, and form a hydriodide of carbon; and, reasoning analogically upon the facts already stated, in respect to the chloride of carbon, it is probable that it may lead to the discovery of an iodide of carbon, but that compound has not as yet been formed.

The hydriodide of carbon is a white crystalline solid, volatile without decomposition, and in many respects analogous to the hydrochloride of carbon; its taste is sweet, and its odour aromatic.

Art. XVII. *Proceedings of the Academy of Sciences at Paris.*

The following is an abstract of a paper read before the Academy, entitled *Chemical Researches on Cinchona, by* MM. Pelletier and Caventou.

The existence of morphium, an alkaline body in opium, was announced in 1816, by M. Serturner, and confirmed by M. Robiquet. Until 1818, no further discovery of this kind was made, but then being assisted by M. Caventou, I sought after other vegetable alkalis, and we very soon published an account of a second body of this kind, which was called strychnine ; and we shewed that the energetic action exerted by the strychnines on the animal system were due to this substance. About the same time M. Boullay ascertained the alkaline nature of picrotoxine, the active principle of the cocculus Indicus, and shortly after we made known the existence of another of these bodies in the Brucea Antidysenterica, since called Brucine. We then found an alkaline substance in the eminently poisonous family of the colchicum ; this substance, named veratrine, is remarkable for its sternutatory powers. Finally, MM. Lassaigne and Fernie found an alkaline principle in the *delphinium staphisagria,* which they called Delphine. The substance discovered by M. Vauquelin in 1812, in the Daphne, should also be classed among the vegetable alkalis. Lately M. Brandt has announced the discovery of several alkaline substances in the belladonna, aconite, *&c.*

If we consider the analogy existing between the action of these bodies on the animal economy, and the action of the substances from which they are obtained, we cannot but conclude that it is to these principles that the vegetable preparations owe their power. These, and similar considerations induced us to extend our researches to other active vegetable substances, and the cinchona naturally drew our first attention.

A strong and active effect on the animal system, and medical properties which do not exist in other substances, or at least only in a minute degree, indicate that the cinchona contains a particular principle which, judging from analogy, should possess alkaline properties. This drew our attention, and, passing at present all points except the mere properties of this principle, we hasten to detail what these are, and how the substance exists in the plants which contain it.

From analysis of the grey bark* (*cinchona condaminœa,*) we found it composed of—

1. Cinchonin united to kinic acid.
2. Green fatty matter.
3. Red colouring matter slightly soluble (cinchonic red.)
4. ——————— soluble (tannin.)
5. Yellow colouring matter.
6. Kinate of lime.
7. Gum.
8. Starch.
9. Lignin.

The first of these substances, and the only one on which we shall dwell at present, is the *cinchonine.* In saying that it is combined with the kinic acid, is to say that it is a salifiable base, or an alkali, and this is fully established by our experiments. We should, however, state that cinchonine was discovered by M. Gornis of Lisbon, but he neither ascertained its alkaline nature, nor studied its combination with acids. Its principal properties escaped him, and to this, aided by the circumstance, that in a more recent memoir M. Lauber regarded it as a pure crystalline resin, is owing the slight degree of attention that has been given to it by chemists.

The following are the properties belonging to cinchonine, as we have observed them. It is white, transparent, and crystallizes in needles; it has but little taste, requiring 7000 parts

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* Common Peruvian bark ?

of water for its solution; but when dissolved in alcohol or an acid, it has the bitter taste of bark. When heated, it does not fuse before decomposition. When analyzed, it gives oxygen, hydrogen, and carbon, the latter being predominant. It contains no nitrogen. It dissolves only in small quantity in the fixed or volatile oils, or in sulphuric ether.

Cinchonine combines with acids, and forms neutral salts, of which the solubility and crystalline form varies with the acid employed. The sulphate of cinchonine is easily crystallized, is moderately soluble, and is composed of

Cinchonine.......	100
Sulphuric acid....	13.021

The muriate of cinchonine is more soluble, and is composed of

Cinchonine.......	100
Muriatic acid	7.9

The nitrate is uncrystallizable.

The weight of the atom of cinchonin, calculated from the analysis of the sulphate, is 38.488.

Gallic, oxalic, and tartaric acid, form neutral salts with cinchonine, which are very slightly soluble, but are soluble in an excess of acid. M. Vauquelin ascertained that infusion of nut-galls constantly formed an abundant precipitate, when poured into a decoction of good cinchona. This precipitate is the difficultly soluble *gallate of cinchonine.*

Cinchonine does not combine with the simple combustible bodies. By the medium of water it converts chlorine and iodine into chloric, muriatic, hydriodic, and iodic acids.

After having examined minutely the properties of cinchonine, and the substances which accompany it in the grey cinchona we passed to the examination of the yellow bark, *(cinchona cordifolia).* This substance proved to be extremely analogous to the former in its composition. But the base contained in the latter is not exactly the same with the *cinchonine,* and though it resembles it in many properties, it differs from it in others, and in these differences these two alkalis may be compared to potash and soda.

The alkali of yellow bark may be distinguished from cinchonine by the name of *quinine.* Quinine may be obtained by precipitation as white and pure as cinchonine. It cannot be crystâllized by evaporation of its alcoholic solution, though it may be obtained in transparent plates. It is as insoluble as cinchonine, but is of a much more bitter taste. The salts which it forms are different from those of cinchonine, both in the proportion of their elements and the properties they possess. They are generally more bitter, and are distinguished by a nacrous pearly aspect.

The weight of an atom of *quinine* is 45.9069. This base has less capacity of saturation than *cinchonine.* The sulphate is formed of

Quinine	100
Sulphuric acid	10.9147.

The acetate of quinine is remarkable for the manner in which it crystallizes; its crystals are flat needles, of a nacrous appearance, which are grouped in silky massive bundles, or stars. The acetate of cinchonine, on the contrary, crystallizes in small lamellar crystals, not possessing any of the silky appearance belonging to the former salt. The gallate, oxalate, and tartrate of quinine, are, at least, as insoluble as the similar salts of cinchonine.

Quinine is very soluble in ether, cinchonine is not. Ether, therefore, may be employed both as a test for the two substances, and as an agent to separate them one from the other.

The analysis of the red quinquina *(cinchona oblongifolia)* followed. It was interesting to ascertain whether this febrifuge contained cinchonine or quinine. It was possible that a third variety of alkali might be found in it, but what we did not expect occurred, namely, a combination of the two alkalies, cinchonine and quinine, proper to the species already analyzed. The cinchonine of red bark is exactly similar to that of grey quinquina; the quinine does not differ from that of the yellow bark, except by the slightest shades in its properties. Another remarkable circumstance is, that red bark contains more of cinchonine than the grey bark, independent of the quantity of quinine; and the latter is more considerable than the quantity of quinine given by an equal portion of the yellow bark.

As to the real quantities of these alkalis contained in the cinchonas, all estimations as yet given are merely approximations from the loss of matter, which occurs in the process of separation; and for the same reason it will not be proper to deduce the activity of these alkalis, in a medical point of view, from the quantity given by a certain weight of bark. Neither do we recommend the substitution of these alkalis for the substances from which they are obtained, until some practitioner, joining prudence with wisdom, pursue these medical investigations of the alkalis of cinchona, and thus give to our works a really useful medical character.

After cinchonine and quinine, the substance occurring in bark, which appears to us most remarkable, is the red matter already described by Reuss. A singular property of this substance, not observed by Reuss, is its convertibility into tannin by the successive action of alkalis and acids. It acquires this property by being dissolved in water, acted on by an alkali, and afterwards separated from the alkali by an acid. It thus even precipitates gelatine.

[We have received from our Paris Correspondent several Scientific Communications, which the pressure of other matter has obliged us to omit. Among these are the details of M. Ampère's Electro-magnetic Researches, which, having been in part laid before the Royal Society, and not leading to any important general conclusions, we have not thought necessary to insert in this Number.]

Art. XVIII. ANALYSIS OF SCIENTIFIC BOOKS.

I. *The Philosophical Transactions for* 1820. P. II.

This part of the Philosophical Transactions, besides six anatomical and physiological papers by Sir Everard Home, Bart., illustrated by several finely engraved plates, from the excellent drawings of Messrs. Clift and Bauer; a very elaborate paper giving a sketch of an analysis and notation applicable to the estimation of the value of life contingencies, by Mr. Gompertz; and some astronomical observations by Mr. Groombridge; contains the following communications, which will probably be thought interesting to the general reader.

i. *On a new Principle of constructing Ships in the Mercantile Navy, by* Sir Robert Seppings, F.R.S.

This country is so deeply interested in whatever tends to give additional safety to the persons and property employed in commerce, as to render the improvement of ship-building a matter of first-rate importance. Our readers are aware that Sir R. Seppings has frequently given the results of his inquiries upon this subject to the Royal Society; and that he has introduced many weighty improvements into that department of naval architecture, relating to the construction of ships of war. In the paper now before us he treats of vessels employed in the mercantile service; and after showing the erroneous principles and loose practice that have hitherto prevailed in this department, and adverting to the lives and property that have in consequence been sacrificed, describes the means of obviating such serious defects.

In the present mode of constructing mercantile ships, half the timbers only of the frames or ribs are so united as to form any part of an arch, every alternate couple being unconnected with each other, and resting upon, instead of giving support to, the external planking. To the disgrace of the English merchant-ship-builder this practice is almost peculiar to him, a preferable system having been long followed by other maritime powers.

The present mode of joining together the several pieces of the same rib is also open to much objection: it is done by the introduction of wedge-pieces, between four and five hundred of which are required in an Indiaman of 1,200 tons; these wedges soon become defective, and communicate their own decay to their attached timbers; besides which, the grain of the rib-pieces is so much cut to give them the curvature required, as considerably to weaken the general fabric; and, lastly, they occasion a great consumption of materials, since the ends of the two rib-pieces must first be cut away, and then replaced by the wedge.

Another great defect of the present system is, that the lower timbers are not continued across the keel; so that no support is given in a transverse direction when the ship touches the ground, nor any aid to counteract the constant pressure of the mast; this great *sacrifice of strength and safety* being made for no other purpose than that of giving a passage for the water to the pumps: for the same purpose, the floor-timbers, which alone cross the keel, are also weakened, and after all the conveyance of the water is very uncertain, and there is always a residue of putrid bilge, at once offensive and injurious.

Such are some of the most serious defects in our present mode of ship-building. To obviate them, Sir Robert proposes to connect the ends of the pieces of timber forming the ribs, by circular pieces of wood, as from time immemorial has been practised to unite the fellies of carriage-wheels, the component parts of each rib being of shorter lengths and less curvature, and consequently less grain-cut: that the timbers should uniformly be carried across the keel, leaving water-courses in the joints for the purpose of conveying the water to the pumps, which reaching below it allow of the removal of all stagnant water; and farther, the water-course being a smooth channel, can be easily cleared, whereas at present it is not only inaccessible in places, but forms compartments for the reception of putrid water.

Many further details are given in this paper respecting the methods of closing the openings between the timbers, and of preventing the access of air to the enclosed parts, by the injection of coal-tar; and directions are also laid down, upon a variety of subjects, which require reference to the plates, and which we are therefore obliged to omit.

The great advantages derived from the plan here described are, the attainment of additional strength, decrease in the consumption of materials and difficulties of construction, protection from worms externally and vermin internally, and facility in stopping leaks.

ii. *Upon the different Qualities of the Alburnum of Spring and Winter Felled Oak Trees. By* Thomas Andrew Knight, Esq., F.R.S.

The timber of oak trees, felled in winter, has generally been acknowledged as superior in quality to that felled in spring, yet the practice of winter-felling has been given up on account of the greater value of the spring-bark.

In this paper Mr. Knight proposes to obtain the advantages of both seasons, by taking off the bark in the spring, and suffering the tree to stand till the ensuing winter.

iii. *Some Experiments on the Fungi, which constitute the colouring Matter of the Red Snow discovered in Baffin's-Bay. By* Francis Bauer, Esq., F.L.S.

Our readers will recollect that very considerable difference of opinion prevailed respecting the colouring matter of the red snow, discovered in Baffin's-Bay, during the northern expedition under Captain Ross: we believe that Dr. Wollaston first suggested their vegetable origin, but it was not until the publication of a very interesting paper upon the subject in this journal (Vol. vii., p. 223,) by Mr. Bauer, that the real nature of this curious matter was made out, and that it was shown to be a fungus of the genus *Uredo.* In this paper Mr. Bauer details a series of experiments on the vegetation of this fungus, which show that it grows luxuriantly when embedded in snow at a temperature considerably below 32°; and that although exposure to the sharp air and wind sometimes kills the primitive fungi, their seeds retain sufficient vitality to vegetate and propagate under snow, which seems to be their natural soil. It is to be regretted that the plate annexed to this paper was not coloured after Mr. Bauer's original drawing.

iv. *On the Errors in Longitude, as determined by Chronometers at Sea, arising from the Action of the Iron in the Ships upon the Chronometers. By* GEORGE FISHER, Esq.

The observations contained in this paper are important, as connected with the chronometrical determination of the longitude; and the perspicuity with which they are detailed does much credit to the author.

The sudden alteration in the rates of chronometers when taken on ship-board has generally been ascribed to the motion of the vessel, and frequently noticed by intelligent seamen. Mr. Fisher accompanied Captain Buchan in his voyage to Spitzbergen in the summer of 1818, and soon after the arrival of the ships off that coast, the chronometers on board the Dorothea were found to be rapidly gaining on their original rates in London, hence the land appeared westward of its true position as determined by lunar observation. On the 9th of August the chronometers were landed upon an island, and their acceleration immediately ceased. The loss in the rate on shore amounted in some cases to no less than 13 or 14 seconds daily; in some the change was sudden, in others more slow, but it was invariably found to take place.

This acceleration is not peculiar to high latitndes, for several chronometers put on board in the river with losing rates, were found to having gaining rates when the ships arrived at Shetland. Having adduced these and many other instances in proof of the fact of acceleration, Mr. Fisher proceeds to inquire into its cause. That this was not the motion of the vessel, was proved by its taking place equally under all circumstances, even when the ships were firmly beset with ice; nor was it change of temperature, for not the least correspondence between its changes and those of the rates could be observed; it appeared, therefore, to Mr. Fisher, to result from the magnetic action exerted by the iron in the ship, upon the inner rim of the chronometer's balance, which is composed of steel. That the iron of ships is magnetic is shown by its polarity, the whole forming one large magnet, having its south pole on deck, nearly amid-ships, and its north pole below; now when we consider how easily the presence of any thing magnetical is detected by the alteration of the rate of a chronometer, it is not surprising that the ships' iron should exert considerable influence upon them. Mr. Fisher concludes this paper by showing, that magnets, placed near the steel-balances of watches and chronometers, produce a very rapid acceleration in their rates of going, in every position of the magnets, and with both poles. Upon too near an approach of the magnets the watch is often rendered useless.

It seems, therefore, desirable that the use of steel should if possible be avoided in the construction of the balances of chronometers for sea use: the force of the balance-springs is also probably affected by the same cause, for chronometers with gold balance-springs, though more difficult to adjust, yet keep the best rates at sea.

v. *On the Measurement of Snowdon by the Thermometrical Barometer, By the* Rev. F. J. H. Wollaston, B.D. F.R.S.

In this paper Archdeacon Wollaston gives an account of an actual measurement made with the instrument which he has described in the *Philosophical Transactions* for 1817; he adverts to the calculations necessary in the measurement of considerable heights by means of it, and compares his own resu ts with those of General Roy. The author makes the height of Snowdon 3546.25 feet.

vi. *On Sounds inaudible by certain Ears.* *By* William Hyde Wollaston, M.D., P.R.S.

This paper contains some interesting elucidations of the physiology of the sense of hearing. Dr. Wollaston observes that deaf persons are in general more susceptible of sharp than low sounds; and that in the healthy state of the ear this partial insensibility may be brought on by throwing the membrane of the tympanum into a state of tension from external pressure, by attempting forcibly to draw breath when the nose and mouth are closed. In this way the ear may be rendered insensible to sounds below F marked by the base cliff; and in illustration, Dr. Wollaston observes, that, in such state of the ear, listening to the sound of a carriage, the deep rumbling noise of the body is not heard, but the rattle of a chain, or loose screw, remains at least as audible as before. "Although," continues the author, "I cannot propose such an experiment as a means of improving the effect of good music, yet, as a source of amusement, even from a defective performance, I have occasionally tried it at a concert with singular effect, since none of the sharper sounds are lost, but, by the suppression of a great mass of louder sounds, the shriller ones are so much the more distinctly perceived, even to the rattling of the keys of a bad instrument, or scraping of catgut unskilfully touched."

In the healthy state of the ear there seems no limit to the discernment of low sounds; but if we turn our attention to the opposite extremity of the scale of audible sounds, and, with a series of pipes exceeding each other in sharpness, examine their effects successively upon the ears of several persons, we find a striking difference in their powers of perceiving very sharp sounds. It is thus that certain persons are perfectly insensible to the chirping of the cricket and grasshopper, to the squeak of the bat, and even to the chirping of the sparrow.

Since there is nothing in the constitution of the atmosphere to prevent vibrations much more frequent than any of which we are conscious, we may imagine that animals, like the *grylli*, whose powers appear to commence nearly where ours terminate, may have the faculty of hearing still sharper sounds which at present we do not know to exist; and that there may be other insects hearing nothing in common with us, but endued with a power of exciting, and a sense that perceives, vibrations of the same nature indeed as those which constitute our ordinary sounds, but so remote that the animals who perceive them may be said to possess another sense, agreeing with our own solely in the medium by which it is excited, and possibly wholly unaffected by those slower vibrations of which we are sensible.

vii. *On the Compressibility of Water.* *By* JACOB PERKINS, Esq.

The experiments of Canton long ago demonstrated the elasticity of water. In this communication Mr. Perkins details to the Society some less exceptionable means of determining the compressibility of water than any hitherto employed. A small metallic cylinder, flattened at one part so as to yield to expansion within, is closed and water-tight at the lower end. At its upper extremity is a small aperture closed by a very sensible valve opening inwards. This instrument, called by the author a *Piezometer*, being perfectly filled with water, the weight of which was accurately known, was put into an hydraulic press, and subjected to a pressure of about 326 atmospheres. When it was taken out and weighed, there was found an increase of water amounting to 3.5 per cent. I.

II. *An Historical and Practical Treatise on the Internal Use of the Hydrocyanic or Prussic Acid in Pulmonary Consumption,* &c. *Second Edition.* *By* A. B. GRANVILLE, M.D., F.R.S., &c.

There are several circumstances which induce us to notice Dr. Granville's book. The first part of it affects scientific arrangement; and the subject of which it treats was first brought before the British Public in this Journal. We also wish to point out an error or two into which the Doctor has fallen, and to advertise him of two or three samples of bad taste which have probably escaped his notice; upon these heads we propose to be very brief.

The hydrocyanic, or prussic acid, in its dilute state, was first separated from Prussian blue by the celebrated Scheele, who obtained it in the form of a colourless transparent liquid, having in an eminent degree the odour of bitter almond, in which, as well as in the cherry-laurel, the leaves and kernels of the peach, and several other vegetable products of the same odour, the same principle has been shown to exist: some of our less chemical readers may, perhaps, also remember its presence in Noyau, Maraschino, and certain other species belonging to that genus of *liqueurs*.

For our knowledge of the real nature of the prussic acid, we are indebted to the admirable researches of Gay-Lussac. He has shown its base to be a peculiar inflammable gaseous compound of carbon and nitrogen, on which he has conferred the title of *cyanogen*, a term derived from the Greek, and signifying the *producer of blue*; whether, because it burns with a blueish purple flame, or because it is essential to the production of the pigment called *Prussian blue*, or for both reasons, we are not quite clear: this cyanogen, however, combined with hydrogen, he has shown to produce the prussic acid, which he accordingly terms *hydrocyanic acid*. It is a highly-volatile liquid, lighter than water, and becoming concrete when exposed to the air, in consequence of the cold generated by its rapid evaporation; its odour is similar to that of Scheele's acid, but infinitely stronger; and it is so deleterious in its action upon the living frame as to occasion the death of large animals in the dose of a drop or two only, and even to excite very disagreeable sensations in the human body when its vapour is inhaled, largely diluted with atmospheric air.

It is a curious but correct axiom, that the more mischievous the qualities of a substance are, the more likely is it to prove of value in the *Materia Medica;* and, when properly diluted by or blended with other remedies, to become of importance in the practice of physic. Accordingly, formidable as the prussic acid is, it has found its way into the apothecaries' shop, and has long been used in one form or other; but the merit (whatever it may be) of using the pure acid, duly diluted, as a substitute for the less definite and certain combinations of it, belongs chiefly to Mr. Magendie, to whose essay formerly published in this Journal we have already adverted; and to the author now before us.

Dr. Granville proceeds in the first and second sections of his book, to treat of the chemical history of the prussic acid; and, after adverting superficially to the discovery of Prussian blue, and to the researches of Gay-Lussac, Scheele, and others, points out the methods usually employed for preparing this substance for pharmaceutical use. He gives us the processes of Scheele, Vauquelin, and Magendie, and, lastly, that of the Apothecaries' Company; and without any sufficient remarks upon the principles of these processes, passes judgment (not always tempered with mercy) upon their respective merits.

Scheele's process consists in boiling Prussian blue with red oxide of mercury and decomposing the solution of cyanuret of mercury thus obtained by nascent hydrogen gas; that is, by mixing its solution with iron filings and sulphuric acid, and proceeding to distillation; the prussic acid passes over in aqueous solution, and may be rendered more pure by re-distillation with a little chalk.

This is not a bad process, provided Prussian blue were always of equable purity, but as it is a very heterogeneous compound as usually found in the shops, the liquid obtained by boiling it with oxide of mercury, is of variable composition; those processes, therefore, are preferable, in which the pure cyanuret is used.

Vauquelin's process is directed in the *Paris Pharmacopœia,* and is, in our opinion, extremely objectionable. It consists, in decomposing solution of cyanuret of mercury by sulphuretted hydrogen gas, filtering to separate the sulphuret of mercury thus produced, and adding carbonate of lead to remove the remaining sulphuretted hydrogen. The prussic acid remains dissolved in the filtered liquor.

We have frequently tried this method, and have not been able to obtain the *pure* prussic acid by means of it, though we cannot pretend to say with what other compounds it is blended.

M. Magendie prepares prussic acid for pharmaceutical use by diluting Gay-Lussac's acid, for which, however, the formula is not given. In this paragraph Dr. G. falls into some sad errors respecting the specific gravities of the pure and diluted prussic acid. He tells us that M. Magendie employs Gay-Lussac's acid, diluted with six times its volume, or eight times and a half its weight of distilled water. "The density of the pure hydrocyanic acid at the temperature of 45° being=70.583, (that of water being = 1,) it follows that the weight of the diluted acid will be 9.20583, since the weight of fluids are equal to their volumes multiplied by their densities," p. 20. We do not profess to understand this sentence, but we believe it will be found that the specific gravity of Gay-Lussac's liquid acid is 0.70583, and that the liquid acid of the above strength has not the medium density of 0.920583, (which Doctor G. probably means,) but that its specific gravity is 0.99000, or thereabouts, great increase of density resulting from the mixture of the pure acid with water.

This brings us to the Apothecaries' Company's process, which Dr. Granville speaks of in the following terms :—

"The formula they employ has been supplied by Professor Brande, as he himself informed me, and is the following : prussiate of mercury lbj., muriatic acid lbj., water lbv. Draw off four pints, and rectify through chalk. I have not had an opportunity of trying this acid, as I am satisfied with that which Mr. Garden prepares for my patients *, but I should conceive that the same objections which exist respecting Scheele's process, may

* "Mr. Garden, Chemist, of Oxford-street, is the only person, to my knowledge, who has prepared the *medicinal* prussic acid, according to both the above methods, (meaning Scheele's and Vauquelin's,) each of which is perfectly good for the purpose of practice, and I have satisfied myself by ocular demonstration, that every precaution was taken to ensure the accurate result of the operations." Note at p. 19.

be urged likewise in the present case; for the acid cannot by this process be obtained in an uniform and well-known degree of concentration. I know besides that the acid, thus prepared, is of a turbid yellowish colour, instead of being colourless and transparent, and that it deposits a considerable sediment, both which circumstances seem greatly to militate against its purity: there is, moreover, no novelty in the process itself, being, in fact, that of Gay-Lussac, changing unnecessarily the proportions, and omitting two or three essential precautions, proposed as indispensable by that chemist in the preparation of the acid."

We should have conceived it more decorous on the part of Dr. Granville, finding the above preparation objectionable, as he has asserted it to be, (but which we shall presently show it is not,) to have stated the objections to the Apothecaries' Company, instead of publishing their process with a view to depreciate it, and to employ it as a vehicle of a *puff oblique* in favour of the Doctor's chemist, Mr. Garden. But let us see how the matter really stands. On making inquiry at Apothecaries' Hall, we find that in consequence of the occasional demand for prussic acid, and the want of any official directions for its preparation in the London Pharmacopæia, Mr. Brande, who holds the office of Professor of Chemistry, and Materia Medica to the Society, received orders from the Laboratory Committee, to examine the various processes for preparing that acid, and to make a report accordingly. That this report was nearly as follows:—

"Having tried the methods of Scheele and Vauquelin, I find them uncertain as to the products, and more especially in the latter case, the specific gravity of Vauquelin's acid always exceeding that of distilled water, probably from the presence of sulphocyanic acid; I consequently recommend to the Committee the following process, founded on Gay-Lussac's method of obtaining the pure acid:

"Prepare a *cyanuret of mercury* as follows:—Take 1 lb. of finely levigated peroxide of mercury, (the *hydrargyri oxidum rubrum*, of the *Pharmacopæia*,) and 2 lbs. of powdered Prussian blue; boil them in a glass vessel in 8 pints of distilled water for one hour, filter and collect the crystals which the filtered liquor deposits on cooling; evaporate the mother liquor to half its bulk, and set aside to crystallize, repeating this operation as long as crystals form. Take 1 lb. of crystallized cyanuret of mercury in powder, 1 lb. of muriatic acid, (specific gravity 1.150,) and 6 pints of water; introduce this mixture into a tubulated retort, and distil 6 pints by measure. If these proportions be observed, the prussic acid, dissolved in water, will pass over free of muriatic acid, and of a specific gravity of 0.995."

We allow, with Dr. Granville, that this process has no merit on account of *novelty*, but we aver, in opposition to the Doctor, that it affords a very uniform and a very pure product, of a good strength for pharmaceutical use, which the other processes do not; and therefore that the acid which Dr. G. administers is open to the objections which he erroneously urges against the improved formula above given. But the Doctor insinuates, though he must know better, that the acid sold at Apothecaries' Hall is *always* turbid, yellowish, and *impure;* this we deny, and if occasionally yellow and turbid, it is in consequence of age, and rather an indication of its purity than otherwise, for dilute hydrocyanic acid, when free from iron, sulphur, and chlorine, deposits, upon long keeping, a yellowish powder, which we believe to be a solid *carburet of nitrogen,* consisting, perhaps, of 1 atom of carbon and one of nitrogen; for when collected, dried, and burned in oxygen, it affords carbonic acid and nitrogen only; this, however, we merely throw out by the way, not having hitherto examined it with due precision.

One word more with respect to the Apothecaries' Company: we believe ourselves correct in stating that their Laboratories at Blackfriars are always open to the inspection of members of the College of Physicians, upon proper application; that they have no secrets, but on the contrary, solicit inspection; that they are at all times ready to adopt any real improvements and necessary alterations in their processes; and that they implicitly follow the directions of the *London Pharmacopœia* in the preparations therein contained, excepting that they have the sanction of the proper officers of the college for certain modifications rendered in many instances necessary by the magnitude and extent of their operations.

But to proceed with our author's book:—The third section treats "on the presence of prussic acid in animal, vegetable, and mineral substances." We are told at its outset, "that although the blood contains the principles of the prussic acid, it is not there found ready formed, since they require the presence of an alcali to influence that peculiar attraction and combination of their molecules which constitutes the acid in question." This, and the two following paragraphs, one of them purporting to be an extract of the author's notes taken at Vauquelin's lectures, are to us quite unintelligible: there is alcali enough in the blood, if alcali only were wanted to influence "that peculiar attraction and combination of molecules," as the Doctor learnedly expresses it, which constitutes prussic acid. This part of the work is especially bare, and betrays a "poverty in the land," which we had little expected. The formation of prussic acid by the combustion of animal matter, is a very curious but difficult branch of chemical inquiry, upon which we had hoped to have gained some information from Dr. Granville's book; he either should not have meddled with it, or given a clear epitome of what is known upon the subject.

The formation of prussic acid in certain diseases, its presence in vegetable products, and its supposed existence in the mineral kingdom, are next superficially adverted to; in relation to the latter subject, "we may instance," says Dr. G., "as the only mineral substance which has been found to contain prussic acid, the *fer azuré* of Haüy." Whereas Haüy himself (*Traité de Minéralogie*, IV., 121,) speaking of the term *Native Prussian Blue* applied to that substance, says, "Cette dénomination ne paroît être fondée sur aucune expérience décisive;" and, moreover, we have the joint authority of Klaproth, Fourcroy, and Proust, for regarding the *fer azuré* of Haüy as a *phosphate of iron*. The Doctor's suggestion of the existence of Prussian blue in *lapis lazuli*, is equally improbable. The colour of *lapis lazuli* is not injured by a low red-heat, and it is used by the painter under the name of *ultramarine*, as a most permanent colour;

whereas, the blue of prussiate of iron is destroyed at a heat below redness, and is any thing but permanent.

The fourth section contains an account of the physical properties of the prussic acid, unnecessarily separated from its chemical history and preparation, and followed by "Physiological experiments made with the pure *hydrocyanic acid*," constituting the 5th section. We consider the curious investigations of Mr. Brodie upon this subject as the most satisfactory that have hitherto been made, and as they are not even alluded to, we shall decline troubling our readers with those here detailed, which seem chiefly intended to persuade the public that, although prussic acid is perhaps the most virulent of all vegetable poisons, it is only dangerous when improperly or empirically administered. In this section, Dr. G. has the bad taste to retail a conversation which passed between a "*person of rank*," afterwards called "his lordship," and a "*popular physician*," respecting the Doctor's book, of which we doubt not a new edition will soon be wanted, and in which we recommend him altogether to erase it.

Passing over the 6th section, "on the analogy which led to the use of prussic acid as a medicine," we find in the 7th, among other matters, an enumeration of the diseases in which prussic acid has proved beneficial, and which are briefly these; coughs of all sorts,—hectic fever,—consumption,—asthma,—chronic inflammation, and abscesses of the lungs,—spitting of blood and pleurisy, and a long list of nervous, local, and organic diseases; but here the Doctor proves too much.

The 8th and the concluding sections, relate to the m ans of detecting prussic acid, and preventing its poisonous effects, and to the history of its introduction into medicine; in neither of which do we remark any thing either very new or very important, and with which, therefore, we shall not prolong our review.

The second part of this *Treatise*, and which the medical reader will consider as the most important, contains a detail of cases in which prussic acid has been administered, and of the effects which it has produced. As far as we can understand this kind of evidence, it appears that prussic acid is a very efficacious remedy in whooping cough; and that it is of singular service in allaying the symptoms which usually attend the early stage of pulmonary consumption. If Dr. Granville substantiates his opinion, respecting its usefulness in the latter disorder, we shall consider him as having conferred a greater obligation upon the human species than even Jenner himself, and we trust that he will reap a proportionate reward. O.

III. *Recherches experimentales, sur les Chaux de Construction, les Bétons, et les Mortiers ordinaires. Par* L. J. Vicat, *Ancien Elévc de l'Ecole Polytechnique,* &c.

This work, containing many important facts relating to calcareous Cements, and Hydraulic and Common Mortars, is the fruit of an immense number of experiments, the results of which are arranged in 25 tables. These are preceded by a series of remarks and details in which the general laws and effects are deduced and stated, but which lose the greater part of their value if separated from the tabular results. The following selection contains some of the results obtained by M. Vicat, and will give an idea of the importance of his work. It is only, however, by referring to the work itself that a true estimate of its value can be obtained.

" *Trial of Limestones.*—It is of the utmost consequence, especially in public works, to ascertain the quality of the lime to be employed, and yet chemical analysis requires practice and knowledge which every architect does not possess. It is always more easy and certain to submit a fragment of the stone which is to be tried to common calcination in a lime-kiln, or as we frequently do, with charcoal in a forge, (for coal forms clusters with the stone,) to slake it in the common way, and make a paste of it, which is to be placed at the bottom of a vessel filled with pure water. If, at the end of 8 or 10 days, this paste has become hard, and resists the finger, it is a proof that the stone tried will furnish hydraulic lime; if, on the contrary, it remains soft, it is the character of common lime."—p. 5.

"*To convert Common Lime into Hydraulic Lime.*—The operation to be described is a true synthesis, which, by the action of fire, unites, in an intimate manner, the essential principles which analysis separates from hydraulic lime. The lime to be modified is to be left in a dry and covered place, until reduced spontaneously into powder, and afterwards mixed, by the aid of a little water, with a certain quantity of grey or brown clay, or simply with brick-earth, and this made into balls, which when dry, are to be heated to the necessary degree.

"Common lime will require 20 per cent. of clay; intermediate limestone will require 15 per cent.; and 10, or even 6, will be sufficient for those which already possess hydraulic properties to a certain extent. When the quantity is raised to 33 or 40, the lime obtained will not slack, but is easily pulverized, and will make a paste that hardens very readily under water. When the clay is mixed with pebbles, *&c.*, it is to be thrown into a large quantity of water, well mixed with it, and the finer part run off into a convenient place. It may then be mixed with the lime in powder, and made into balls; the quantity required is easily ascertained by a little practice.

"It must not be supposed that the clay baked alone, and then added to common lime, in the proportions mentioned, will give the same results as when the two substances are mixed before being heated. The fire modifies the one substance by the other, and gives rise to a new compound, which enjoys new properties."—p. 7.

"If fragments of common lime, and a mixture of coal and charcoal be placed, stratum super stratum, in a small brick furnace, and burnt; and if, as the substances fall by the dissipation of the combustible, the lime, which passes through with the cinders, be returned with fresh fuel to the furnace, and the process be continued 15 or 20 hours, according to the size of the fragments, an over-calcined lime will be obtained, which will not slake, but which, when reduced into a fine powder, and made into a ductile paste, has the property of hardening under water." —p. 15.

" *On Slaking.*—Quick-lime, thrown into a proper quantity of water, splits with noise, produces a bubbling, liberates hot and slightly caustic vapours, and at last forms a thick paste. One part of common lime, thus slaked, is expanded into 3.1 parts of paste. If a sufficient quantity of water has not been added in the first instance to the lime, it is necessary to wait till it is cold before more be given. If a second portion be poured on whilst the lime is hot from the effect of the first insufficient quantity, then the lime does not divide well, but remains granular. This process is called *ordinary slaking*, or the first process.

" Quick-lime plunged into water for a few seconds, and withdrawn before it begins to split, then hisses, splits, and bursts apart with noise, and falls into powder. This powder, when cold, does not heat by the addition of more water. One part of lime thus slaked, expands into 1.5, or 1.7 parts by measure. This is called *slaking by immersion*, or the second process.

" If quick-lime be exposed to the air it falls to powder, one part increasing in volume to 1.75, or even to 2.55. This is called *spontaneous slaking*, or the third process."—p. 16.

" *Hydrates of Lime.*—Lime slaked in these three ways was made into masses with water; these were dried in the sun, and then their resistance or tenacity, and their hardness, ascertained; and the result was, that for all kinds of lime the resistance and the hardness was in proportion to the expansion in bulk by the process of slaking, *i. e.*, the process which divided the lime most completely, gave a hydrate of the greatest strength."—p. 25.

" *Effects of Slaking on the Hydraulic Mortars.*—It appears from experiments on the hydraulic mortars made from common lime, intermediate or meagre lime, and lime slightly hydraulic, that the three modes of slaking, arranged according to the order of their superiority, are the third, the second, and the first; but that for highly hydraulic lime, the order becomes reversed. If common lime be considered as the commencement of the scale, and we pass from that through the various shades of difference to the hydraulic lime which is most meagre, and which will form the last degree on the scale, the differences resulting from the methods of slaking diminish, at last disappear, and then increase in the opposite direction."—p. 42.

"*Action of Water on Hydraulic Mortars.*—The dissolving action of water on hydraulic mortars appears to cease when it has removed the excess of lime that was either in weak combination, or entirely at liberty; and, it was found that the quantity which remained after the action ceased, was never far from that which had been ascertained to be the best; from whence the following rule: "To find in all possible cases the proportion of lime which is most fit for any puzzolana, a ball of the hydraulic mortar, nearly an inch in diameter, must be made, having rather an excess of lime; this must be exposed for a year under pure water which is to be changed frequently, and then the quantity of lime which has disappeared is to be ascertained either by analysis or otherwise, and subtracted from the whole quantity, and the difference gives the proportion required for the quantity of puzzolana taken."—p. 55.

"*Effect of Lime on Hydraulic Mortars.*—1. Excess of lime in a hydraulic mortar retards its setting: the proportions most favourable to the setting are also those which give the greatest strength.

"2. Powerful puzzolanas, combined with common lime, harden much sooner than if combined with hydraulic lime; but hydraulic lime has the advantage when combined with a less active puzzolana.

"3. The second and third process of slaking appears generally to accelerate the setting more than the first.

"4. Hydraulic mortars made with common lime harden more, or make more progress from the second to the third year, than from the first to the second, so that it may be said the rapidity of their progress is accelerated.

"5. The resistance of hydraulic mortars, made with lime slightly hydraulic, also undergoes an acceleration, but much less than in the former case.

"6. The progress of mortars, made with lime eminently hydraulic, begins to diminish at the end of the second year.

"Hence hydraulic mortars, made with common lime, require more time than the others to attain their maximum of resistance." —p. 58.

"*On the Nature of Hydraulic Lime.*—The modification which the action of the fire has caused in the small proportion of silex and alumine, mixed with the pure calcareous matter, gives to the compound which results, the power of acting chemically by the intervention of water on the new siliceous substances, added in the state of sand. It is this circumstance which constitutes the distinctive and essential character of hydraulic lime."—p. 73.

"*On the Use of the Siliceous Sand.*—According to our results, the different sands range in the following order of superiority.

"For highly hydraulic lime: 1. Fine sand. 2. Sand of unequal size resulting from the mixture of fine sand, either with coarse sand or with small gravel. 3. Coarse sand.

"For lime moderately hydraulic: 1. Mixed sand. 2. Fine sand. 3. Coarse sand.

"For common lime: 1. Coarse sand. 2. Mixed sand. 3. Fine sand.—p. 74.

"*Rapidity of Desiccation.*—Mortars made from hydraulic lime, which have the power of solidifying all the water they contain, require to be dried slowly. They lose, according to circumstances; namely, by common desiccation, three-tenths, and by rapid desiccation eighth-tenths of the force which they would acquire by slow desiccation."—p. 77.

Art. XIX.—*Astronomical and Nautical Collections.* No. IV.

i. *Remarks on the Calculation of Parallax for a Spheroid.*

It is not unusual to employ, in the calculation of eclipses, instead of the true latitude, a latitude reduced according to a table of Mayer, in order to determine the effect of the earth's ellipticity on the lunar parallax: and the same correction may, in some cases, be thought necessary for the very accurate computation of lunar distances, for the purpose of determining the longitude.

Professor Vince, in his valuable *System of Astronomy,* observes, (vol. i. n. 173,) that "the most elegant and simple method of finding the parallax in latitude and longitude on a spheroid, is the following, given by Mayer. Subtract the angle [formed by the vertical line with the earth's semidiameter] from the latitude on the spheroid, and you get the . . latitude of the point reduced to a sphere. Also the horizontal parallax must be adapted to the [corrected radius.] . . The latitude thus reduced, and the horizontal parallax thus found, are to be employed in computing the moon's parallaxes in longitude, latitude, right ascension, and declination, which will now be performed by the rule founded on the hypothesis of the earth being a sphere."

Thus, if the latitude of Greenwich is 51° 28′ 40″, we are to deduct 14′ 29″, and employ 51° 14′ 11″, which, when the moon is near the equinoctial, and on the meridian of the place, makes alone a difference of about 8″ in the parallax; that is, taking the ellipticity $\frac{1}{230}$: which is a correction not wholly inconsiderable.

But, supposing the moon, as may easily happen, to be considerably to the north of the east or west, this correction will be not merely superfluous, but absolutely erroneous, since in truth a smaller correction of an opposite nature is required.

When the moon is due east, or due west, her altitude is not affected by the obliquity of the surface; since the perpendicular to the meridian is obviously parallel to the surface of the sphere:

and when she passes beyond these points, her apparent altitude, instead of being diminished by the ellipticity, is actually increased by it.

The whole effect appears to be most conveniently computed by considering the place of the apparent zenith as always brought nearer to the pole than the true zenith, by a quantity depending on the latitude only, and which is equal to $\frac{1}{306}$ cos. 2 (lat.—45°)=$\frac{1}{306}$ cos. 2 lat.—90°=$\frac{1}{306}$ sin. 2 lat. or to 11′ 14″ × sin. 2 lat.; and the correction of the apparent altitude will be obtained by multiplying this quantity into the cosine of the azimuth. The computation being referred to this point instead of the apparent zenith, it will be necessary to apply the same correction, in the case of a lunar distance, to both the altitudes concerned, according to the respective azimuths, by means of the following rule:

Azimuthal Correction of Parallax.

Add together the logarithmic cosecant of twice the latitude, the secant of the azimuth, and the constant logarithm 1.2048, the sum will be the proportional logarithm of the correction of the altitude for the earth's ellipticity, to be added when the moon or star is north of the east or west in the northern hemisphere, and to be subtracted when south, and the reverse in the southern hemisphere. The altitudes so corrected will give the true parallax, from the *reduced* equatorial parallax, by the ordinary rules, and the refraction without sensible error.

ii. *Places of the Comet of* 1822, *computed by* Professor Encke, *and communicated by* Dr. Olbers.

Professor Encke has considered the effects of Saturn, Jupiter, Mars, the Earth, Venus and Mercury, on the Comet, throughout the whole interval from 1786 to 1819; but he has found that the attraction of Jupiter only will have any material effect on the time of the next perihelium, in 1822; this effect, however, being very considerable, since the least distance of the comet from Jupiter will be only 1.136, so that the passage through the perihelium will be retarded by it more than 9 days. The particulars will be inserted at large in the Berlin Almanac for 1823. In order to extend the calculation beyond the probable grounds of uncertainty, Professor Encke has assumed two hypotheses; the one assigning a period longer by a day than the other, and a mean distance greater in proportion: the corresponding elements are these—

	I	II
Passage of the Perihelium, mean time at Seeberg	1822, May, 24.0	1822, May, 25.0
Log. mean distance	.3472191	.3474612
Longitude of the Perihelium from the mean equinox, 24 May, 1822	157° 12′ 7″	
Longitude of the ☊	334 23 40	
Inclination of the orbit	13 20 36	
Angle of eccentricity	57 38 30	
Daily motion	1069″,49307	1068″,59904

Mean Time at Seeberg.	AR. Com. app. I	AR. Com. app. II	Decl. Com. app. I	Decl. Com. app. II	Mean Time at Seeberg.	AR. Com. app. I	AR. Com. app. II	Decl. Com. app. I	Decl. Com. app. II
NOON	° ′	° ′	° ′	° ′	NOON	° ′	° ′	° ′	° ′
Feb. 25	0.44	0.40	+7.19	+7.16	June 7	97.41	98.11	+13.21	+14.16
Mar. 1	2.28	2.24	8. 4	8. 1	9	99.32	100. 8	11.24	12.24
5	4.17	4.12	8.51	8.47	11	101.24	102. 5	9.17	10.22
9	6.11	6. 5	9.40	9.35	13	103.19	104. 5	6.57	8.10
13	8.11	8. 4	10.30	10.26	15	105.20	106.10	4.22	5.43
17	10.16	10. 8	11.23	11.18	17	107.32	108.25	+ 1.30	+ 3. 1
21	12.28	12.19	12.18	12.13	19	109.59	110.53	— 1.43	— 0. 1
25	14.47	14.37	13.15	13. 9	21	112.43	113.38	5.21	3.23
29	17.15	17. 3	14.13	14. 8	23	115.51	116.43	9.24	7.11
April 2	19.51	19.38	15.14	15. 9	25	119.29	120.17	13.59	11.25
6	22.38	22.24	16.16	16 11	27	123.45	124.23	19. 2	16. 7
10	25.37	25.21	17.20	17.14	29	128.48	129.12	24.28	21.12
14	28.49	28.30	18.25	18.19	July 1	134.48	134.51	30.11	26.34
18	32.17	31.55	19.31	19.24	3	141.55	141.26	35.53	32. 1
22	36. 3	35.38	20.36	20.30	5	150.15	149. 5	41.10	37.13
26	40.10	39.42	21.40	21.35	7	159.47	157.47	45.40	41.50
30	44.42	44. 9	22.41	22.36	9	170. 9	167.18	49. 6	45.34
May 4	49.41	49. 0	23.36	23.31	11	180.51	177.15	51.20	48.15
8	55.12	54.22	24.20	24.17	13	191.11	187. 6	52.27	49.58
12	61.16	60.23	24.49	24.51	15	200.37	196.30	52.40	50.37
16	67.48	66.56	24.51	25. 0	17	208.54	204.42	52.16	50.40
20	74.38	73.50	24.16	24.34	19	215.58	211.57	51.28	50.13
24	81.12	80.33	23. 0	23.27	21	221.57	218.14	50.27	49.28
28	86.55	86.36	20.57	21.34	23	226.58	223.35	49.19	48.34
June 1	91.47	91.50	18.18	19. 2	25	231.17	228.10	48.11	47.36
3	93.53	94. 6	16.47	17.34	27	234.55	232. 6	47. 2	46.35
5	95.49	96.11	15. 8	15.59					

Logarithms of the Distance.			
Mean Time at Seeberg.	From the ☉	From the ⊖	Degree of Light.
NOON			
Feb. 25	0.21777	0.39085	0.011
March 5	0.19003	0.38074	0.013
13	0.15899	0.36792	0.016
21	0.12389	0.35214	0.020
29	0.08371	0.33315	0.027
April 6	0.03702	0.31062	0.037
14	9.98171	0.28410	0.054
22	9.91472	0.25277	0.084
30	9.83159	0.21545	0.146
May 8	9.72737	0.16927	0.293
16	9.60748	0.10781	0.675
24	9.53834	0.01902	1.396
June 1	9.60748	9.90237	1.737
9	9.72737	9.77450	1.802
17	9.83159	9.64064	2.066
25	9.91472	9.50899	2.583
July 3	9.98171	9.43059	2.723
11	0.03702	9.47483	1.721
19	0.08372	9.59215	0.809
27	0.12389	9.71315	0.385

In the determination of the degree of brightness, the unit is supposed to be that which the comet exhibited the 5th January, 1819. It then appeared in the field of the telescope at the same time with a fine nebula in the head of Aquarius, n. 77 of *Bode*, or n. 2 of the *Connaissance des Tems*, and was precisely of the same magnitude, form, and brightness. When the comet was first discovered, on the 20th October, 1805, its brightness, in terms of this unit, was 1.321, and it was then considered as equal to that of a star of the fifth magnitude. In the beginning of July, 1822, therefore, it will be brighter than a star of the fourth magnitude.

It appears from this ephemeris, that we shall not be able to see the comet in Europe in the *spring* of 1822, because it will be too faint in comparison with the evening twilight. Whether it may be possible to discover it in *December* 1821, or *January* 1822, with very powerful telescopes, is very doubtful. But in southern latitudes it will be easily discoverable as soon as the 9th or 10th of June, 1822, since it will be sufficiently emerged from the solar rays, and of the brightness of a star of the fifth magnitude. The delay of nine days in the passage of the perihelium, from the action of Jupiter, prevents the subsequent approach of the comet so near to the earth, as Dr. Olbers had computed, on the hypothesis of its arrival at the perihelium on the 15th or 16th of May.

iii. *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.

[Continued from Vol. IX. p. 149.]

SECTION II.

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

§ 20.

The suppositions which have been made the foundation of the approximate solutions, § 11, lead, when geometrically considered, further than the conclusions which have been drawn from them. Assuming that the path of the comet is a right line, described with an equable velocity, the distances of the comet from the earth may be found by equations of the first degree. The supposition, that the chord is divided by the revolving radius, in the proportion of the times, leads to equations of the second degree, from which these distances may be determined. These equations require so much the more a particular investigation, as they have been recommended, not only by their inventors, but by other mathematicians, far beyond their real merits; and have been condemned, on the other hand, by those who have justly rejected them, upon grounds not altogether satisfactory.

§ 21.

The problem of finding a line, which shall be cut by three others in a given proportion, is of an indeterminate nature. It is known that the condition is fulfilled by any of the tangents of a parabola, of which the three given lines, together with one line which is cut by them in the required ratio, are also tangents, so that the curve is completely ascertained by its four tangents. But the problem is only indeterminate, so long as the three lines remain in one plane. When they are not in one plane, there is only a single position, in which a line passing through a given point in one of them, will intersect the two others. If we add the condition, that this line must be divided by them in a given proportion, the points through which it must be drawn are all given, and by an equation simply linear. In this manner BOUGUER thought it possible to determine the distances from the earth, and even the elements and nature of the orbit, from three observations at short intervals of time. The equations deduced from this supposition, retaining the notation already adopted, would be $t' : t'' = (x' - x'') : (x'' - x''') = (y' - y'') : (y'' - y''') = (z' - z'') : (z'' - z''')$, whence ϱ', ϱ'', and ϱ''' might be deduced by means of linear equations only; and since these values of ϱ' and ϱ''' would be independent of the motion in a parabola, we might obtain from them, if they were perfectly accurate, by comparing them with the whole time intervening, not only the situation and dimensions, but also the nature of the conic section in which the comet revolves.

§ 22.

There is, however, a case in which the problem again becomes undetermined, even when the lines are not in a single plane. If they are all parallel, no right line can cut them all; and in other cases, there is only a single line for each point of one line that can cut the other two, but it may happen that all such lines must necessarily be cut in the same proportion; and this will occur when the points to which the three lines tend, speaking astronomically, are found in a great circle, or geometrically, when they are parallel to three lines lying in any one plane. Such must be the case whenever more than one line is divided in the same proportion by the three which it intersects. Hence it follows, that if the portion of the earth's orbit, described in the given interval, were a straight line, and the earth's velocity equable, this line would be divided by the line of direction in the same proportion with the supposed portion of the comet's orbit; so that the relative position of these lines, with respect to the lines of direction, would remain completely undetermined, and Bouguer's equation would lead to no conclusion whatever. The conclusion obtained from it must therefore depend on the deviation of the earth's orbit from a straight line, and on the change of its velocity, whilst the curvature of the comet's orbit, which is often considerably greater, is totally neglected. In fact, if we suppose the earth's motion equable and rectilinear, we shall have the equations $t' : t'' = (\text{R}' \cos. \text{A}' - \text{R}'' \cos. \text{A}'') : (\text{R}'' \cos. \text{A}'' - \text{R}''' \cos. \text{A}''') = \text{R}' \sin. \text{A}' - \text{R}'' \sin. \text{A}'') : (\text{R}'' \sin.$

A″ — R‴ sin. A‴), and by comparison with the equations in § 21, $t' : t'' = (\varrho' \cos. \alpha' - \varrho'' \cos. \alpha'') : (\varrho'' \cos. \alpha'' - \varrho''' \cos. \alpha''') = (\varrho' \sin. \alpha' - \varrho'' \sin. \alpha'') : (\varrho'' \sin. \alpha'' - \varrho''' \sin. \alpha''') = (\varrho' \text{tang.} \beta' - \varrho'' \text{tang.} \beta'') : (\varrho'' \text{tang.} \beta'' - \varrho''' \text{tang.} \beta''')$, which obviously ndicate only the proportions of ϱ', ϱ'', and ϱ''', to each other, and not their actual value. Hence we may understand the remark of LAMBERT, that BOUGUER had attempted to find the distance of the comet by means of the minute verse sine of the earth's orbit, and LAGRANGE's observation, that BOUGUER's solution is not correct, even for infinitely small portions of the orbit; for both the magnitudes, compared with each other in their evanescent state, are of the same order. But it is not correct to infer, as this great geometrician, and PINGRE′ after him, have done, that no portion of a comet's orbit must be assumed as straight, even for the purposes of approximation; since Boscovich's construction, for example, proceeding on this supposition, affords a result approaching to the truth, and becomes even perfectly accurate when the interval is evanescent. In fact, Boscovich supposes only that the verse sine vanishes in comparison with the length of the arc, and the difference of the velocity in comparison with the whole velocity, which is perfectly justifiable. Nor does LAPLACE's objection to this method appear to be much more important, which is, that it may sometimes indicate a retrograde motion instead of a direct one, or the reverse; for since the equation of the sixth degree, on which the solution depends, may have several real roots, and must have two, the ambiguity is inseparable from the nature of the problem, and LAPLACE himself has only avoided it by means of a supernumerary equation, which he calls an equation of security. We may easily understand how it happened that BOUGUER was so fortunate in applying his method to the comet of 1729; for this comet having been much more remote from the sun than the earth, its orbit was much less curved than that of the earth, so that it might, without any great inaccuracy, be considered as comparatively straight; and it is only in such cases as this, when the comet is very remote, and the arc which it describes comparatively short and little curved, that BOUGUER's method is capable of affording any thing like a true result; in all other cases it is totally useless.

§ 23.

The same remark may be applied, and for a similar reason, to another problem, which has excited much attention among those who have cultivated the theory of comets; that is, having four right lines given, to find a fifth that shall cut them in a given proportion. Wren, Newton, Gregory, Cassini, and Lambert, have given solutions of this problem; and it has frequently been proposed to consider the path of a comet, between four observations, not remote from each other, as a right line, described with an equable velocity; and by means of this proposition to deduce, from the four observed longitudes, the curtate distances of the comet from the earth. If, indeed, the four given lines are not in one plane, the position of the fifth, which cuts them all, is determined without any regard to the proportions of the segments; so that, if we took the latitudes into consideration, we might determine the orbit from the four observations, merely upon the supposition that the portion considered is a right line, without any regard to the velocity. The position of this line would, however, require the solution of an equation of the eighth degree, and in form somewhat complicated. It would also require the same limitations as the method of Bouguer, though it would be considerably more useful, for the velocity of the comet is always the most unequal when its motion is the most nearly rectilinear, and the reverse. It must, however, be remarked, that no person seems to have made an experiment of the method in question, at least with any success. Even Cassini, who founded his whole theory of comets upon it, never actually reduced it to practice. The method by which he succeeded in determining the distance of the comet in 1729, is different from this, though not very essentially, and the problem of Wren might have been applied to this comet, for the same reason that the error of Bouguer's result was inconsiderable. Cassini indeed attempted to apply it to the comet of 1742, but he complains that the observations are not sufficiently accurate for the purpose; this, however, is not the true reason of his failure, for the method is as little adapted to finding the true distance, as that of BOUGUER. If we suppose the earth to describe a right line during the observations, with an equable velocity, the problem will become indeterminate; consequently, the curvature of the earth's orbit must afford the result which is obtained, while that of the comet's orbit is neglected. This omission is by no means warrantable, and the method can be of no manner of service even if the intervals be infinitely small, and the observations perfectly correct, unless the comet be many times further from the sun than the earth. It might, for example, have been employed with some advantage in the case of the Georgian planet, before the true nature of this body was discovered. I omit, for the sake of brevity, the demonstration of my assertion that the problem becomes indeterminate when the earth is supposed to describe a right line with a uniform velocity, although it may be exhibited in different forms; and I shall only remark, that the four lines of direction, and the two portions of the orbits, are, upon this supposition, tangents of one and the same parabola, of which every other tangent is divided in the same proportion by the lines of direction. This ambiguity seems to have escaped the penetration of the celebrated LAMBERT, notwithstanding the labour he employed on the problem, for the proposal, by which he attempted to improve it, renders it completely indeterminate, and consequently useless. LALANDE informs us that BOSCOVICH had long ago shown the insufficiency of this method, as well as of BOUGUER's; but I am not acquainted with the nature of his demonstration.

§ 24.

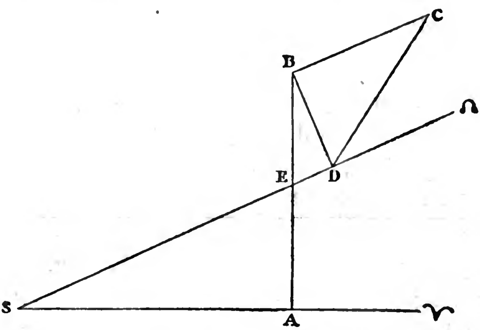
On the whole, therefore, it appears that equations of the first degree are insufficient for the solution of the problem, since the distance must be determined from magnitudes of the same order with those which are neglected, when the motion of the comet is supposed to be equable and rectilinear.

§ 25.

If we assume that the chords of both orbits are divided by the revolving radii in the proportion of the two intervals of time, the proportion of the true or of the curtate distances of the comet from the earth may be found for the first and third observations, as will appear in the next section. Now a fourth and a fifth observation may again be combined with the third, and we may obtain, by means of these, the proportion of the first, third, and fifth, distances; and having this proportion, we may determine the distances themselves, from the condition that the sun must be in the plane of the orbit.

§ 26.

In order to show this, we need only to find an equation between x, y, z, and the longitude of the node and the inclination of the orbit of the comet. Let S be the centre of the sun, S♈ a line pointing to the vernal equinox, S☊ the line of the



nodes; let SA be x, $AB = y$, BC, perpendicular to the ecliptic $= z$, and C the place of the comet. Now if BD be perpendicular to S☊, BDC will be the inclination of the orbit, and calling ☊S♈, the longitude of the node, h, and $BDC = i$, we have $AE = x$ tang. h; $BE = y - x$ tang. h; $BD = BE$ cos. $h = y$ cos. $h - x$ sin. h, and $BC = z = BD$ tang. $i = y$ cos. h tang. $i - x$ sin. h tang. i. We shall therefore obtain, from three observations, three equations of the form $z = y$ cos. h tang. $i - x$ sin. h tang. i, each containing, when the proportions of the curtate distances are given, only three unknown quantities, ϱ, h, and i, which may therefore be deduced from them; for x, y, and z, are all dependent on ϱ. (§ 7).

§ 27.

Let us now put $\varrho'' = M\varrho'$, and $\varrho''' = N\varrho'$; we shall then have $z' = \varrho'$ tang. β', $z'' = M\varrho'$ tang. β'', and $z''' = N\varrho'$ tang. β'''; hence the three equations may be thus expressed:—

$$\frac{\varrho'}{\cos. h \text{ tang. } i} = \frac{y' - x' \text{ tang. } h}{\text{tang. } \beta'} = \frac{y'' - x'' \text{ tang. } h.}{M \text{ tang. } \beta''} = \frac{y''' - x''' \text{ tang. } h}{N \text{ tang. } \beta'''}$$

Consequently ($y' - x'$ tang. h) M tang. $\beta'' = (y'' - x''$ tang h) tang. β'; and ($y' - x'$ tang. h) N tang. $\beta''' = (y''' - x'''$ tang. h) tang. β: and if we substitute in these equations the values of x and y, we obtain two equations containing the unknown quantities ϱ' and h only; either of which may be found by means of an equation of the second degree. If we prefer the formula for h, the solution will greatly resemble that of Professor Hennert; if for ϱ', we shall obtain expressions analogous to those which Mr. Duséjour has invented, and which he considers as extremely convenient.

§ 28.

It will be sufficient at present to give an example of the latter method, and to find the value of ϱ'. If we exterminate tang. h from the two equations, we obtain

$$\frac{y'' \text{ tang. } \beta' - M y' \text{ tang } \beta''}{x'' \text{ tang. } \beta' - M x' \text{ tang } \beta''} = \frac{y''' \text{ tang. } \beta' - N y' \text{ tang. } \beta'''}{x''' \text{ tang. } \beta - N x' \text{ tang. } \beta'''}$$

consequently tang. β' $(y'' x''' - y''' x'')$ + M tang. β'' $(y''' x' - y' x''')$ + N tang. β''' $(x'' y' - x' y'') = 0$; which is an equation of the second degree. Now we have, from § 7, $x' = \varrho'$ cos. α' — R′ cos. A′, $x'' = M\varrho'$ cos. α'' — R″ cos. A″, $x''' = N\varrho$ cos. α''' — R‴ cos. A‴, $y' = \varrho'$ sin. α' — R′ sin. A′, $y'' = M\varrho'$ sin α'' — R″ sin. A″, and $y''' = N\varrho'$ sin. α''' — R‴ sin. A‴. Substituting these values, we obtain, after some easy reductions, making

P = M tang. β'' R′ R‴ sin. (A‴ — A′) — tang. β' R′ R‴ sin. (A‴ — A″) — N tang. β''' R′ R‴ sin. (A″ — A′)

Q = M tang. β'' (R‴ sin. (A‴ — α') + N R′ sin. [α''' — A′]) — tang. β' (M R‴ sin. (A‴ — α'') + N R″ sin. [α''' — A″]) — N tang. β''' (R″ sin. (A″ — α') + M R′ sin. [α'' — A′])

S = M N (tang. β'' sin. (α''' — α') — tang. β' sin. (α''' — α'') — tang. β''' sin. [α'' — α'])

the quadratic equation $S\varrho'^2 - Q\varrho' + P = 0$; whence

$\varrho' = \frac{Q}{2S} \pm \sqrt{\left(\frac{Q^2}{4S^2} - \frac{P}{S}\right)} = \frac{Q \pm \sqrt{(Q^2 - 4SP)}}{2S}$. This is essentially the same with the formula of Duse'jour, but the way of obtaining it appears to be much easier and shorter than that of this great mathematician: and a quadratic equation for h may be derived from the same equations much more conveniently than in Professor Hennert's manner.

§ 29.

Pingre' has attempted to make computations according to both these methods, and has found the results of both extremely erroneous. The coefficients S, Q, P, were always very small, and therefore the smallest errors of the observations had a great influence on the magnitudes to be determined; so great indeed, that he consider's Hennert's solution as quite useless. The same must, however, be true of that of Duséjour, for both depend on the same equations.

§ 30.

It will be worth our while to examine more particularly the cause of this insufficiency, Now it is obvious that the solution would be mathematically correct, if the observations were perfect, and the proportions, expressed by M and N, truly ascertained. But observations can never be free from all error: and the proportions are obtained from a supposition not precisely accurate. In Duse'jour's formulas, the value of ϱ' depends only on the apparent curvature of the orbit of the comet, or in its deviation from a great circle: for if the three places lay in a great circle, the coefficient of ϱ'^2, or S, would be $= 0$, since, in this case, tang. β'' sin. $(\alpha'' - \alpha')$ — tang. β' sin. $(\alpha''' - \alpha'')$ — tang. β''' sin. $(\alpha'' - \alpha') = 0$; which may be shown by making φ the distance of the comet in longitude from the intersection of the great circle in question with the ecliptic, and its inclination to the ecliptic μ; so that tang. $\beta' =$ tang. μ sin. φ, tang. $\beta'' =$ tang. μ sin. $(\varphi + \alpha'' - \alpha')$ and tang. $\beta''' =$ tang. μ sin. $(\varphi + \alpha''' - \alpha')$; and, substituting these values in the equation, we obtain sin. $(\varphi + \alpha'' - \alpha')$ sin. $(\alpha'' - \alpha')$ — sin. φ sin. $(\alpha''' - \alpha'')$ — sin $(\varphi + \alpha''' - \alpha')$ sin. $\alpha'' - \alpha'$), which is obviously $= 0$.

[For sin. $(a+b)$ sin. c — sin. $(a+c)$ sin b is, in general $=$ sin. a cos. b sin. c — sin. a cos. c sin. $b =$ sin. a sin. $(b-c)$. Tr.]

Duséjour finds a quadratic equation, not for ϱ', the curtate distance, but for the true distance, which he calls Δ'. But his co-efficient for Δ'^2 becomes also $= 0$, when the three places of the comet are in a great circle. This co-efficient is equivalent to sin. β' cos. β'' cos. β''' sin. $(\alpha''-\alpha''')$ + sin. β'' cos. β' cos. β''' sin. $(\alpha'''-\alpha')$ + sin. β''' cos. β' cos. β'' sin. $(\alpha'-\alpha'')$, which, divided by cos. β' cos. β'' cos. β''', becomes equal to S.

§ 31.

It might also be shown that the two other co-efficients, in this case, which is essentially the same with the supposition of a rectilinear and equable motion, must both vanish: but we have already sufficient evidence of the degree of utility of this method: and since three neighbouring observations of the comet must always be very nearly in a great circle, the co-efficients S, P, Q, which depend only on the curvature of the apparent path, must always be very small, so that their values may be materially altered by the errors of observation. When we add to this consideration the want of perfect accuracy in the determination of M and N, or of the proportions of the three curtate distances, we shall find that this method is utterly useless for neighbouring observations, and will in general afford a very erroneous result. If, however, we had a sufficient number of accurate observations, following each other at small distances, the first, middle, and last of them being tolerably remote from each other, so that we might determine M and N for them by means of the intermediate ones, we might obtain something like a solution of the problem from this method: and the most readily where the apparent path of the comet deviated most from a great circle; which is most likely to happen when the distances of the comet and the earth from the sun are very different from each other, and when the comet is near the quadrature, or remote from the conjunction or opposition. But after all, the calculation would be not a little tedious, and its result too uncertain to be put in competition with those of other methods of approximation.

§ 32.

It does not appear that either DUSE'JOUR or HENNERT was aware of the natural reason of the inutility of this method; but the former appears to have been practically convinced of it, since he has, in his later work, substituted another, which, with all possible respect for this celebrated mathematician, may be confidently pronounced very troublesome, and prolix, and inaccurate. He applies a very ingenious mode of analysis to the determination of the proportional distances, but the expressions include a factor depending on the distance from the sun; which must therefore be supplied by a previous approximation. He thus reduces the distances to a single unknown quantity, whence he determines the length of the chords, and compares these, by means of NEWTON's approximation, with the times. This method requires very laborious preparatory calculations, and can only be employed when we have a long series of accurate observations; nor is the result, after all, correct. We may therefore conclude, that neither the equations of the first nor of the second degree, that have been hitherto proposed, can be employed in practice with real advantage.

SECTION III.

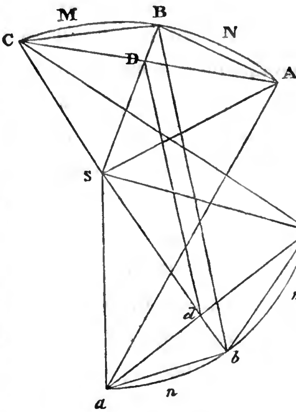
A short and easy Method of finding the approximate Elements of the Orbit of a Comet.

§ 33.

From the preceding observations it may be inferred, that unless, like LA CAILLE, we attempt the determination of the orbit of a comet by numberless trials almost entirely conjectural, we must begin with some supposition, not mathematically true, although approaching to the truth, respecting the properties of its motion. BOSCOVICH's assumption of rectilinear and equable motion is somewhat too bold, being at once doubly erroneous. We may approach much nearer to the truth by adhering to the supposition, that the chord of the comet's orbit is divided by the middle radius in the proportion of the times; and if we assume at the same time that the chord of the earth's orbit is divided in the same proportion, we shall obtain an approximate solution, which is indirect, but more easy and convenient than could well have been imagined, considering the intricate nature of the problem.

§ 34.

Let S be the sun, A, B, and C, three places of the comet not remote from each other, and *a*, *b*, *c* the corresponding places of the earth; we shall assume that the middle revolving radii SB, S*b* divide the chords AC, *a c*, in D and *d*, in the proportion of the intervals; so that $ad : dc = AD : DC = t' : t''$; a supposition which is very near the truth when the arcs are small; for,



first, the difference between the sectors proportional to the times, and the triangles, which are in the exact proportions of the two segments of the chords, is very small, and of an order higher than the sectors themselves; secondly, the differences, or the portions of the segments contained between the chords and the arcs, are greater as the sectors are greater, though not generally in the same proportion; while, in the third place, there is always one position of the revolving radius, for each parabolic or elliptic arc, which divides the chord in the precise proportion of the arcs or of the times. In what cases this last circumstance occurs, for a parabolic curve, has been investigated by Newton, by Gregory, and, more recently, by Lambert, who have shown that the proportion can never be very different from that which is here supposed, unless the two intervals employed be very unequal. For the earth, the difference must be the less considerable, when the times are nearly equal, as the orbit approaches so nearly to a circle.—*Newton, Principia, III, lemm.* viii. *Gregory, Astr. Phys.* V. xviii. *Lambert, Beyträge,* III. *Propr. ins. orb. com. Astr. Jahrb. Berl.* 1779.

(To be continued.)

Art. XX. *Corrections in Right Ascension of Thirty-Six principal Fixed Stars to every Day of the Year, together with some Observations on the Use of the Transit Instrument, &c.* By James South, F. L. S., *Honorary Member of the Cambridge Philosophical Society, and Member of the Astronomical Society of London.*

To the practical Astronomer there is nothing more important, than an accurate knowledge of his Time, and when his situation will allow of its use, there is *no* Instrument so *well* able to afford it, as a Transit; accordingly, in well-appointed Observatories, it is considered an Instrument, which is *absolutely* indispensable.

As there found, however, it is, from its bulk, but little adapted to the purposes of the Amateur, and still less, to the convenience of the Traveller; but its peculiar aptitude to the object, for which it is designed, has occasioned the construction of the portable Instrument.

But whether the one, or the other, be the subject of our employment, if we are desirous of arriving at the *greatest* accuracy, which the instrument is *capable* of affording, (and without this, *either* becomes a mere toy,) we must *carefully* guard against *every* source of error; thus, the Collimation of the Instrument, the Horizontality of its axis, and the Meridionality of the Telescope, should claim our attention; the former of these, *viz.*, the collimation, not being easy of derangement, requires only *occasional* examination; but the two latter, demand *all* our solicitude, *as often as* the Instrument is employed.

The usual mode of detecting the error of collimation, as also of correcting it, is too well known to require comment; it will however, be advisable, *never* to allow the milled heads which are applied to the collimating screws, to remain on them, as their resemblance to the adjusting screw of the common refracting telescope, renders them *very enticing* to Visitor's fingers. It will also be equally prudent to remove the similar contrivance, for giving to the instrument, its azimuthal motion.

As to the horizontal position of the axis, any deviation from *it*, may generally be detected and corrected by its own level; but when the situation of the instrument renders it practicable, comparisons made of transits taken by direct Vision, with those obtained by Reflection, will afford a more *satisfactory* Result. Olive-oil may be frequently used as the reflecting surface, when Quicksilver cannot.

We have now arrived at the most *important*, and I fear, the most frequent Source of error, namely, the Deviation of the Instrument from the Meridian; and, upon the most *mature* consideration, I hesitate not in attributing it to the *improper* Confidence usually placed, in what is denominated a Meridian Mark. The Instrument, if large, and capable of bearing a power of two hundred or two hundred and fifty, is most correctly placed in the Meridian, by observations of the superior and inferior Transits of the Pole-star; if small, and having a power equal to 50 or 60, of Circumpolar Stars, at a greater distance from the Pole, whose motion over the wires is *therefore* better cognizable to the Instrument: such are α Ursæ Major, β Ursæ Minor, α and β Cephei; and when *these* cannot be observed, which is sometimes the case, recourse is had to the Transits of *high* and *low* stars.

These observations shewing no discrepancy in the error of the clock, a mark is set up, so as to be bisected by the meridian wire; and *if* the Astronomer, having assigned to it, its station, *could* deprive *it*, as well as *its support*, of the usual Attributes of Matter, it would *indeed* deserve the name it bears; but till that be within his reach, it should be *only* considered as what *it is*, namely, an *Approximate Adjustment* to the Instrument.

But it may be urged, that *any* Error in the position of *this* Mark, (supposing it to be towards the South) may be corrected by another, placed towards the North; now although this seems plausible *enough* in Theory, yet when put into practice, it will be found to bring with it, *it's* Inconveniences. Thus, the Meridian wire *will* occasionally bisect the *one*, but *not* the *other*; and as each Mark has been erected under similar Precautions, it becomes a matter of no *small* difficulty to *decide*, *which* is *right*, or *which* is *wrong*; nor can the *Instrument* with *any propriety* be called in to *settle* the Dispute, seeing that the very act of placing *it* under such guidance, *implies*, that *it* is considered *incapable* of taking care of *itself*. Again, the Instrument will now and then deviate from *both*, and when corrected for *one*, will be found to deviate from the *other*; hence arises *another* Difficulty, from *which*, as in the former instance, Sidereal Observations *alone* can extricate us.

In alluding, however, to the fixed Stars, those *only*, whose right ascensions are *well* settled, are fitted for the purpose, and with such, the Greenwich Catalogue, presents us.

But they, in consequence of various causes, are constantly varying in apparent Right Ascension; Corrections, therefore, the result of these, must be applied to the Star's mean R. A. before its apparent right ascension *can* be determined. Now these are Calculations requiring the Sacrifice of much Time and Labour; to obviate which, the late Astronomer Royal published his seventeenth and eighteenth tables. The first of these, contains the Sum of the corrections, in Aberration, Precession, and Solar Inequality of Precession; the second, that, arising from Lunar Nutation: the former is calculated, for every tenth day of the Year, the latter for every tenth degree of the Moon's Node; and when the equations are required, for any other day of the year, or any *other* place of the Node, they *must* be found by proportion.

Now, although the labour is thus materially diminished, still, *what* remains is far from *inconsiderable*, and it is to *this* circumstance, that we must attribute the observations of these Stars being by *no means* so *general* as *might* be wished.

But with regard to the Sun, things are different: *his* Right Ascension is *given* for every day in the year; accordingly we find *his* Transit *regularly* taken, whilst *those* of Dr. Maskelyne's catalogue, of *infinitely greater importance*, are very *generally* neglected.

This allusion to the Sun brings me to the mention of a circumstance, *not* I believe generally known; namely, that the Error of the Clock, as shewn by *his* Transit, is *not* unfrequently *very far* from the TRUTH: indeed it is a FACT, upon which *reliance* may be placed, that whilst the Errors of the Clock, as deduced from *several Sidereal* Transits, shall not differ from each other above *two* or *three hundredths* of a Second, the Sun's Transit will give one varying *three* or *four* TENTHS; and that, too, where the Instruments employed, are of the larger sort, and where *every* Precaution is *constantly* taken, to prevent the Sun's Rays from deranging the position of the instrument.

Now, if such be the case, where *large* Instruments are used, *surely* where *small* ones are employed, *greater* Errors may be *expected*; for *these* are liable, to *all* the Errors of *large* Instruments, as well as *those* which their diminutive Form, and Metallic Supports, are, from exposure to the Sun's Rays, *sure* to entail upon them.

Hence, therefore, the *impropriety* of depending upon the Sun's Transit, to *correct* the Error of the Clock, and the *absolute necessity* of recurring to those of the Stars, for Accuracy of Time, not less than Accuracy of Position.

If, then, what I have hinted at, is the principal cause of error, the Remedy is *obvious;* let *no* inducement be held out to Observations of the Sun, which is *not* afforded to *those* of the Stars; and *then*, and not *till* then, will the observations made in *private* Observatories be *generally* worth recording.

Under *this* persuasion it is, that I beg to offer to the public, the accompanying Corrections in Right Ascension of the Thirty-six Principal Fixed Stars, to *every* day of the present Year; they are computed from the Tables of Dr. Maskelyne's, before alluded to, and will be found, I believe, *tolerably* free from Error. The mean places of the Forty-six Stars are deduced from the Catalogue published in the *Nautical Almanac* for 1823; and, for the Convenience of the Observer, the Declinations have been added. The daily Right Ascension of the Pole-star is *that*, given by Dr. Struve, and is the same as was lately published by Mr. Baily: *it* has been added, from a Conviction of its Utility.

Before I conclude, let me *impress* upon the *young* Astronomer, the *absolute necessity*, of his observing as *often* as *possible*, the Transits of the Stars forming the Maskelynean Catalogue; by the mean of several of these, the Error of Observation will be confined *indeed* within *narrow* Limits;—the Error of the Clock *well* ascertained, and *any* Deviation of the Instrument *from its* Meridian *immediately* detected. To *this* Catalogue he stands *indebted*, for his *Original* Accuracy, and to IT he may *safely trust* his *Future*. *Each Star* he may consider a *Guide* to his Instrument, and a *Zero* to his Clock; and, taken collectively, *They* will afford him a MERIDIAN MARK, of *which* ALONE it may be said, NON IMBER EDAX, NON AQUILO IMPOTENS, POSSIT DIRUERE.

Blackman-street, January 1, 1821.

P.S. In observing, it will be found convenient to place the Five Observations of every Star, under each other, prefixing the Hour and Minute to that of the Meridian Wire only; the Observer will then *readily* obtain the Mean Transit, by adding together the Seconds, multiplying the sum by Two, cutting off the Two Figures to the right as Decimals, and adding or subtracting 12, 24, or 36, to, or from, those on the left; and in some Instances doubling the sum of the Seconds will, without further Trouble, give the Transit over the Meridian Wire.

Oil and Quicksilver have been alluded to, as affording good Reflecting Surfaces; they are, however, apt to collect Dust, but which may be easily separated by passing the former through Bibulous Paper, and by pressing the latter through folds of Linen.

THE MEAN PLACES OF 46 GREENWICH STARS,
Reduced to Jan. 1, 1821.

NAMES OF STARS.	Right Ascension.			N. P. D.			Declination.			
γ Pegasi	0	4	1,92	75	48	39,95	14	11	20,05	N
α Cassiopeiæ	0	30	24,30	34	26	44,28	55	33	15,72	N
Polaris	0	57	15,77	1	38	46,12	88	21	13,88	N
α Arietis	1	57	6,36	67	23	17,29	22	36	42,71	N
α Ceti	2	52	56,01	86	37	3,44	3	22	56,56	N
α Persei	3	11	35,45	40	47	4,37	49	12	55,63	N
Aldebaran	4	25	39,71	73	51	31,72	16	8	28,28	N
Capella	5	3	29,00	44	11	43,92	45	48	16,08	N
Rigel	5	5	56,44	98	24	54,93	8	24	54,93	S
β Tauri	5	14	59,19	61	33	13,06	28	26	46,94	N
α Orionis	5	45	29,17	82	38	4,67	7	21	55,33	N
Sirius	6	37	15,51	106	28	35,03	16	28	35,03	S
Castor	7	23	9,96	57	43	43,37	32	16	16,63	N
Procyon	7	29	55,82	84	19	24,23	5	40	35,77	N
Pollux	7	34	21,16	61	33	0,45	28	26	59,55	N
α Hydræ	9	18	47,56	97	53	12,38	7	53	12,38	S
Regulus	9	58	49,95	77	9	40,95	12	50	19,05	N
α Ursæ Major	10	52	35,82	27	17	5,90	62	42	54,10	N
β Leonis	11	39	55,52	74	25	37,97	15	34	22,03	N
β Virginis	11	41	22,47	87	13	34,66	2	46	25,34	N
γ Ursæ Major	11	44	22,33	35	18	35,25	54	41	24,75	N
Spica Virginis	13	15	46,60	100	13	22,23	10	13	22,23	S
η Ursæ Major	13	40	28,81	39	47	23,41	50	12	36,59	N
Arcturus	14	7	30,17	69	52	50,88	20	7	9,12	N
1 α Libræ	14	40	48,32	105	14	40,80	15	14	40,80	S
2 α Libræ	14	40	59,76	105	17	24,93	15	17	24,93	S
β Ursæ Minor	14	51	20,04	15	6	46,53	74	53	13,47	N
α Coron. Bor.	15	27	6,87	62	40	35,27	27	19	24,73	N
α Serpentis	15	35	27,61	83	0	13,06	6	59	46,94	N
Antares	16	18	25,89	116	1	24,72	26	1	24,72	S
α Herculis	17	6	29,56	75	23	50,07	14	36	9,93	N
α Ophiuchi	17	26	37,93	77	18	3,65	12	41	56,35	N
γ Draconis	17	52	27,27	38	29	9,15	51	30	50,85	N
α Lyræ	18	30	52,92	51	22	36,45	38	37	23,55	N
γ Aquilæ	19	37	45,13	79	48	53,84	10	11	6,16	N
α Aquilæ	19	42	3,07	81	35	46,33	8	24	13,67	N
β Aquilæ	19	46	31,36	84	1	55,56	5	58	4,44	N
1 α Capricorni	20	7	43,23	103	3	9,80	13	3	9,80	S
2 α Capricorni	20	8	6,98	103	5	26,55	13	5	26,55	S
α Cygni	20	35	20,10	45	21	15,96	44	38	44,04	N
α Cephei	21	14	18,13	28	10	13,35	61	49	46,65	N
β Cephei	21	26	18,77	20	13	24,94	69	46	35,06	N
α Aquarii	21	56	35,35	91	11	3,18	1	11	3,18	S
Fomalhaut	22	47	44,30	120	34	6,70	30	34	6,70	S
α Pegasi	22	55	51,22	75	45	16,53	14	14	43,47	N
α Andromedæ	23	59	9,44	61	53	50,32	28	6	9,68	N

	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran.	Capella.	Rigel.	β Tauri.	α Orionis.	Sirius.
	″	″	″	″	″	″	″	″	″
Jan. 0	− 0,07	+ 0,57	+ 0,99	+ 1,39	+ 1,99	+ 1,53	+ 1,68	+ 1,58	+ 1,60
1	08	56	98	39	99	53	68	58	61
2	09	55	98	39	99	53	68	59	61
3	10	54	97	39	99	53	69	60	62
4	11	53	97	39	99	53	69	61	63
5	12	52	96	39	99	53	69	61	64
6	13	50	96	39	99	53	70	62	65
7	14	49	95	38	99	53	70	63	66
8	15	48	95	38	99	53	70	63	67
9	16	47	94	38	99	53	70	64	67
10	17	46	93	38	99	53	71	64	68
11	18	45	92	37	98	53	71	64	68
12	19	43	91	36	98	52	71	64	68
13	20	42	90	36	98	52	71	64	69
14	21	41	89	35	97	51	70	64	69
15	22	40	88	34	97	51	70	64	70
16	23	39	87	33	96	50	70	64	70
17	24	38	86	32	96	50	70	64	70
18	25	37	85	31	95	49	70	64	71
19	26	35	84	30	95	49	69	64	71
20	27	34	83	30	94	48	69	64	71
21	27	33	82	29	93	48	68	64	71
22	28	31	80	29	92	47	68	64	71
23	29	30	79	28	90	47	67	64	70
24	30	29	78	28	89	46	67	63	70
25	30	28	77	27	88	46	66	63	70
26	31	26	76	27	87	45	65	62	70
27	32	25	75	26	86	44	64	62	70
28	33	24	74	26	84	44	64	61	70
29	33	23	72	25	83	43	63	61	69
30	34	22	71	24	82	42	62	60	69
31	35	21	70	23	80	41	61	59	68
Feb. 1	36	19	68	21	79	40	60	58	67
2	36	18	67	20	77	39	59	57	67
3	37	16	66	19	76	38	58	56	66
4	38	15	65	18	74	37	57	56	65
5	39	13	63	17	72	36	56	55	64
6	40	12	62	16	71	35	55	54	63
7	40	11	61	15	69	34	53	53	62
8	41	10	59	14	68	33	52	53	62
9	42	08	58	13	66	32	51	52	61
10	43	07	57	12	65	31	50	51	60
11	43	05	55	10	63	30	49	50	59
12	43	04	54	09	62	28	48	49	58
13	44	03	52	07	61	27	46	48	57
14	44	02	51	06	59	25	45	47	56
15	45	01	49	05	57	23	44	46	55
16	45	00	48	03	55	22	42	45	54
17	45	−0,01	47	02	53	20	41	43	53
18	45	03	46	00	51	19	39	42	52
19	46	04	44	+ 0,99	49	17	38	41	51
20	46	05	43	97	47	15	36	40	50
21	47	06	41	96	45	14	35	38	49
22	47	07	40	94	43	12	33	37	48
23	47	08	38	93	41	10	31	36	46
24	47	09	37	92	39	09	30	35	45
25	48	10	35	90	37	07	28	33	43
26	48	11	34	89	35	05	27	32	42
27	48	12	32	87	33	03	25	30	40
28	49	13	31	86	31	02	24	29	39

	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica virg.	Arcturus.
	″	″	″	″	″	″	″	″	″
Jan. 0	+2,02	+1,61	+1,93	+1,23	+1,27	+0,79	+0,62	−0,05	−0,07
1	04	62	95	25	30	82	65	01	04
2	05	64	96	27	32	85	69	+0,01	01
3	07	65	98	30	35	89	72	05	+0,02
4	09	67	99	32	38	92	76	08	05
5	11	68	+2,01	34	41	95	79	12	09
6	13	70	03	37	44	99	83	16	12
7	14	72	05	39	47	+1,02	86	19	15
8	16	73	07	42	50	06	89	23	18
9	17	75	08	44	52	09	93	26	21
10	19	76	10	46	55	12	96	30	24
11	20	77	11	48	57	15	99	33	27
12	21	78	12	50	60	18	+1,02	37	31
13	22	78	13	52	62	21	05	40	34
14	23	79	14	54	64	24	08	44	37
15	24	80	15	56	67	27	11	47	40
16	25	81	16	58	70	30	14	51	44
17	26	82	17	60	72	33	17	54	47
18	27	83	18	62	74	36	20	58	50
19	28	84	19	64	76	39	23	61	54
20	29	85	20	66	78	42	26	64	57
21	29	86	21	68	80	45	29	67	60
22	30	86	22	70	82	47	31	71	64
23	30	87	22	72	84	50	34	74	67
24	31	88	23	73	86	53	36	77	71
25	31	88	24	74	88	56	39	80	74
26	32	88	25	76	90	59	42	84	78
27	33	89	25	77	92	61	44	87	81
28	33	89	26	78	94	64	47	90	84
29	34	90	26	79	96	66	49	93	87
30	34	90	27	80	98	69	52	96	90
31	34	90	27	81	99	71	54	99	93
Feb. 1	34	90	27	82	+2,01	74	56	+1,02	96
2	34	90	27	83	03	76	58	05	99
3	34	90	28	84	04	79	60	08	+1,02
4	34	90	28	84	05	81	62	10	06
5	34	89	28	85	07	84	65	13	10
6	34	89	28	86	08	86	67	16	13
7	33	89	28	87	09	88	69	19	16
8	33	89	28	88	11	91	71	22	19
9	33	89	28	89	12	93	73	25	22
10	33	88	27	90	13	95	75	27	25
11	32	87	27	91	14	97	77	30	28
12	32	87	27	91	15	99	80	33	31
13	31	86	27	92	16	+2,01	82	36	34
14	31	86	26	92	17	03	84	38	37
15	30	85	25	92	18	05	86	41	40
16	29	84	25	93	19	07	88	43	43
17	28	84	24	93	20	09	90	46	46
18	28	84	24	94	21	11	92	48	49
19	27	83	23	94	22	13	94	51	52
20	26	83	22	94	23	14	95	53	54
21	25	82	21	94	24	16	97	56	57
22	24	81	20	94	24	18	99	58	59
23	23	80	19	94	25	19	+2,00	61	62
24	22	80	18	94	25	20	02	63	64
25	21	79	17	94	25	22	03	66	67
26	20	78	16	94	26	23	05	68	69
27	19	77	15	94	26	24	06	70	72
28	18	76	14	94	27	26	08	73	74

	α Libræ.	α Cor. Bor.	α Serpent.	Antares.	α Herculis.	α Ophiuch.	α Lyræ.	γ Aquilæ.	α Aquilæ.
	″	″	″	″	″	″	″	″	″
Jan. 0	−0,55	−0,58	−0,64	−1,02	−1,00	−1,02	−1,58	−1,09	−1,06
1	52	55	61	−0,99	−0,98	00	57	08	05
2	48	52	58	96	96	−0,98	56	08	05
3	45	49	55	93	93	96	55	07	04
4	41	46	52	90	91	93	54	06	04
5	38	43	49	86	88	91	53	06	03
6	34	39	46	83	86	89	51	05	03
7	31	36	43	80	84	87	50	04	02
8	28	33	40	77	81	85	49	03	01
9	24	30	37	74	79	83	48	02	00
10	21	27	34	71	77	81	46	01	−0,99
11	18	24	31	68	75	79	44	00	98
12	15	21	28	65	72	77	43	−0,99	97
13	11	18	25	61	70	74	41	98	96
14	07	15	22	58	67	72	40	97	95
15	04	12	19	55	65	69	38	96	94
16	01	08	15	51	63	67	36	94	92
17	+0,02	05	12	48	60	65	34	93	91
18	06	02	09	45	58	62	32	92	89
19	10	+0,01	06	41	55	60	30	90	88
20	13	04	03	38	53	58	28	89	87
21	16	08	00	35	50	56	26	88	85
22	20	11	+0,03	31	48	53	24	86	83
23	23	15	06	28	45	51	22	85	82
24	27	18	10	24	42	48	20	83	80
25	30	22	13	21	39	45	18	82	79
26	34	25	16	17	36	42	16	80	78
27	38	28	19	14	34	40	14	79	76
28	41	32	22	11	31	38	12	77	75
29	45	35	25	07	28	35	10	75	73
30	48	38	28	04	25	33	08	74	72
31	52	41	31	01	22	30	06	72	70
Feb. 1	55	45	34	+0,03	19	27	03	70	69
2	59	48	37	06	17	25	01	69	67
3	62	51	40	10	14	22	−0,98	67	66
4	65	54	43	13	11	19	96	65	64
5	68	58	47	16	08	16	93	63	62
6	71	61	50	20	05	13	91	61	60
7	75	64	53	23	02	11	88	60	58
8	78	68	57	27	+0,01	08	86	58	56
9	81	71	60	30	04	05	83	56	54
10	85	74	63	34	07	02	80	54	52
11	88	77	66	37	10	+0,01	77	52	50
12	91	80	70	41	13	03	74	50	48
13	94	83	73	45	16	06	71	48	46
14	97	86	76	48	19	09	69	46	44
15	+1,01	90	79	52	22	12	66	44	42
16	04	93	82	56	25	15	63	42	40
17	07	97	85	59	28	18	60	40	38
18	10	+1,00	88	63	31	21	58	38	36
19	13	03	91	66	34	24	55	36	34
20	16	06	94	69	37	27	52	34	32
21	19	09	97	73	40	30	49	31	30
22	22	12	+1,00	76	43	33	46	29	27
23	25	16	03	80	46	36	43	27	25
24	28	19	05	83	49	39	40	25	23
25	31	22	08	87	52	42	37	22	20
26	34	25	11	90	55	45	34	20	18
27	37	28	14	94	58	48	31	17	15
28	40	31	17	97	61	51	28	15	13

	β Aquilæ.	α Capricor.	α Cygni.	α Aquarii.	Fomalhaut	α Pegasi.	α Androm.	Polaris.
	″	″	″	″	″	″	″	
Jan. 0	—1,03	—0,88	—1,81	—0,55	—0,05	—0,45	—0,30	H. M. S.
1	02	88	81	55	06	46	31	0.57. 1,61
2	02	87	82	56	07	47	32	0,93
3	01	87	82	56	08	48	33	0,22
4	00	86	83	56	09	49	34	0.56.59,46
5	—0,99	86	83	56	10	49	35	58,66
6	98	85	84	57	11	50	36	57,82
7	98	84	84	57	12	51	38	56,99
8	97	83	85	58	13	52	39	56,19
9	97	83	85	58	14	53	41	55,41
10	96	82	85	59	14	53	42	54,67
11	95	81	85	59	15	54	43	53,97
12	94	80	85	59	16	54	44	53,30
13	93	80	85	59	16	55	46	52,66
14	92	79	85	59	17	56	47	52,01
15	91	78	84	59	18	56	48	51,34
16	89	77	84	59	18	56	49	50,64
17	88	76	84	59	19	57	50	49,91
18	87	75	83	59	19	58	51	49,12
19	86	74	83	59	20	58	52	48,32
20	85	73	83	59	20	59	53	47,48
21	84	72	82	58	21	59	54	46,68
22	82	70	82	58	21	59	55	45,88
23	81	69	82	58	21	60	56	45,13
24	80	67	81	58	22	60	57	44,41
25	79	66	81	58	22	60	58	43,74
26	77	64	80	57	22	61	59	43,11
27	75	63	79	57	23	61	60	42,51
28	74	61	79	57	23	62	61	41,89
29	73	59	78	57	24	62	62	41,25
30	71	58	78	57	24	63	63	40,58
31	69	56	77	57	24	63	64	39,89
Feb. 1	68	55	76	56	24	63	65	39,15
2	66	53	75	56	25	64	65	38,39
3	64	52	74	56	25	64	66	37,63
4	63	50	73	56	25	64	66	36,88
5	61	48	71	55	25	64	67	36,18
6	59	47	70	55	25	64	68	35,52
7	57	45	69	55	25	64	68	34,90
8	55	43	68	54	25	64	69	34,32
9	53	41	66	54	25	64	69	33,77
10	51	39	65	53	25	64	70	33,25
11	49	37	63	51	25	64	70	32,71
12	47	35	62	50	24	64	71	32,15
13	45	33	60	49	24	64	71	31,57
14	43	31	59	49	24	64	72	30,94
15	41	29	57	48	24	63	73	30,32
16	39	27	56	47	24	63	73	29,67
17	37	25	54	46	23	63	74	29,02
18	35	23	53	45	23	63	74	28,40
19	33	21	51	44	23	63	75	27,80
20	31	19	49	43	23	63	75	27,28
21	29	17	47	42	22	62	76	26,80
22	26	15	45	41	22	62	76	26,37
23	24	13	43	40	21	61	76	25,97
24	21	11	41	39	20	61	76	25,55
25	19	08	39	38	19	60	76	25,17
26	17	06	37	37	19	60	77	24,76
27	15	03	35	35	18	60	77	24,32
28	12	01	33	34	17	59	77	23,83

	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran.	Capella.	Rigel.	β Tauri.	α Orionis.	Sirius.
	″	″	″	″	″	″	″	″	″
Mar. 1	−0,49	−0,14	+0,29	+0,84	+1,29	+1,00	+1,22	+1,27	+1,37
2	49	15	28	82	27	+0,98	21	26	35
3	49	16	27	81	25	96	19	25	34
4	49	17	26	79	23	95	18	23	32
5	48	18	25	78	21	93	16	22	31
6	48	19	24	76	19	91	14	20	29
7	48	20	23	75	17	89	12	18	28
8	47	21	22	73	15	88	11	17	26
9	47	22	21	71	12	87	10	15	24
10	47	23	20	70	10	85	08	14	23
11	47	24	19	68	08	83	06	12	21
12	47	25	18	67	06	81	04	10	20
13	46	25	17	66	03	80	02	09	18
14	46	26	16	64	01	78	01	07	17
15	45	27	15	63	+0,98	77	+0,99	06	15
16	45	27	14	61	96	76	98	05	13
17	44	28	13	59	94	74	96	03	11
18	43	28	12	58	92	72	94	01	09
19	43	28	11	57	90	71	93	+0,99	08
20	42	28	10	56	88	69	91	98	07
21	42	29	09	54	86	68	89	96	05
22	41	29	09	53	84	66	88	95	03
23	40	29	08	51	82	65	86	94	+1,01
24	39	29	07	50	80	63	85	92	+0,99
25	38	30	06	48	78	61	83	91	97
26	37	30	06	47	76	60	81	89	95
27	36	30	05	46	74	58	79	87	93
28	35	31	04	44	72	56	77	86	91
29	35	31	03	43	70	54	76	84	89
30	34	32	02	42	68	53	74	83	87
31	33	32	01	41	67	51	73	82	85
April 1	32	32	01	40	65	50	72	81	84
2	30	32	00	39	63	48	70	79	82
3	29	32	00	38	61	47	69	78	81
4	28	32	00	37	59	46	68	76	79
5	26	31	00	36	58	45	67	75	77
6	24	31	00	35	56	43	65	73	75
7	23	31	00	34	54	42	64	72	73
8	22	30	00	33	52	40	62	71	72
9	20	30	−0,01	32	51	39	61	70	70
10	19	29	01	31	49	37	60	68	67
11	18	29	01	30	48	36	59	67	65
12	16	28	01	29	46	35	58	65	64
13	14	28	01	28	45	33	56	64	62
14	12	27	01	27	43	32	55	63	61
15	11	27	01	27	42	31	54	62	60
16	09	26	02	26	40	30	53	60	58
17	08	25	02	25	39	29	52	59	56
18	06	24	02	25	37	27	51	58	54
19	05	23	02	24	36	26	50	57	53
20	03	22	01	23	35	26	49	56	51
21	01	21	01	23	34	25	48	55	50
22	+0,01	20	00	22	33	24	47	54	48
23	03	19	00	22	32	24	46	54	47
24	05	18	00	21	31	23	45	53	45
25	08	17	00	21	30	22	45	52	44
26	10	15	+0,01	21	29	21	44	51	43
27	12	14	01	21	28	20	43	50	42
28	15	13	01	20	27	20	43	50	41
29	17	12	02	20	27	19	42	49	39
30	19	11	02	20	26	19	41	48	38

	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virgin.	Spica Vir.	Arcturus.
	″	″	″	″	″	″	″	″	″
Mar. 1	+2,17	+1,75	+2,13	+1,94	+2,27	2,27	+2,09	+1,75	+1,77
2	16	74	12	94	27	28	10	77	79
3	15	73	11	94	27	29	12	79	82
4	14	72	10	94	28	30	13	81	84
5	12	71	09	93	28	31	14	83	87
6	11	71	08	93	28	32	15	85	89
7	09	69	07	92	28	33	16	87	92
8	08	68	06	92	28	34	17	89	94
9	07	66	05	91	28	35	18	91	96
10	05	65	03	91	28	36	19	93	99
11	04	64	02	90	28	37	20	95	+2,01
12	02	63	01	90	28	38	21	97	03
13	01	61	00	89	28	38	22	99	05
14	+1,99	60	+1,98	89	28	39	23	+2,00	07
15	98	59	97	88	27	39	23	02	09
16	97	58	96	87	27	40	24	03	11
17	95	57	94	87	27	41	24	05	13
18	94	56	93	86	26	41	25	06	15
19	92	55	91	86	26	42	25	08	17
20	91	54	90	85	26	42	26	09	19
21	89	53	88	84	25	43	26	11	21
22	87	51	87	83	24	43	27	12	23
23	85	50	86	82	24	43	27	14	25
24	84	48	84	81	23	44	27	15	27
25	82	46	82	80	22	44	28	16	29
26	81	45	81	79	22	44	28	17	30
27	79	43	79	77	21	45	28	19	32
28	77	41	77	76	20	45	28	20	33
29	76	39	75	75	20	45	29	21	35
30	74	38	74	74	19	45	29	22	36
31	73	36	73	73	19	45	29	23	38
April 1	71	35	71	72	18	45	30	24	39
2	69	34	69	71	18	45	30	25	40
3	68	32	68	70	17	45	31	26	42
4	66	31	66	69	16	45	31	27	43
5	64	29	64	68	15	45	32	28	44
6	62	27	62	66	14	45	33	29	46
7	60	26	60	65	13	45	33	30	47
8	59	25	59	64	12	45	34	31	48
9	57	24	58	63	11	44	35	32	50
10	56	22	56	62	10	44	35	33	51
11	54	20	54	61	09	44	35	34	52
12	53	19	53	60	08	44	34	35	54
13	51	17	51	58	07	44	34	35	55
14	49	16	50	57	06	43	33	36	56
15	48	14	48	56	05	43	32	36	56
16	46	12	46	54	03	43	31	37	57
17	44	11	45	53	02	43	31	37	58
18	43	09	43	52	01	42	30	38	59
19	41	08	42	51	00	42	30	38	60
20	40	06	40	50	+1,99	42	29	39	61
21	38	05	39	49	98	41	29	39	62
22	37	03	38	47	97	41	28	40	63
23	35	02	36	46	95	40	28	40	64
24	34	+1,00	35	44	94	40	27	40	64
25	32	+0,99	33	43	93	39	27	41	65
26	31	98	32	42	92	38	26	41	66
27	30	97	31	41	91	38	26	41	66
28	28	96	29	40	90	37	25	42	67
29	27	94	28	38	89	37	25	42	67
30	25	93	26	37	88	36	24	42	68

	α Libræ.	α Cor. Bor.	α Serpent.	Antares.	α Herculis.	α Ophiuch.	α Lyræ.	γ Aquilæ.	α Aquilæ.
	"	"	"	"	"	"	"	"	"
Mar. 1	+ 1,43	+ 1,34	+ 1,20	+ 1,01	+ 0,64	+ 0,54	− 0,25	− 0,12	− 0,10
2	46	37	23	04	67	57	22	10	08
3	48	40	26	08	71	60	19	07	05
4	51	43	29	11	74	63	15	05	03
5	53	46	32	15	77	66	12	02	00
6	56	49	35	18	80	69	08	+ 0,01	+ 0,02
7	59	52	38	22	83	72	05	03	04
8	61	55	41	25	86	75	02	06	07
9	64	58	44	28	89	78	+ 0,02	08	09
10	66	61	46	32	92	81	05	11	12
11	69	64	49	35	95	84	08	13	14
12	71	66	52	39	98	87	11	16	17
13	74	69	54	42	+ 1,01	90	15	18	19
14	76	71	57	45	03	93	18	21	22
15	79	74	59	48	06	96	21	23	24
16	81	76	62	51	09	99	24	26	27
17	84	79	65	55	12	+ 1,02	28	29	30
18	86	81	67	58	15	05	31	31	32
19	89	84	70	61	17	08	35	34	35
20	91	86	73	65	20	11	38	36	38
21	94	89	76	68	23	14	42	39	41
22	96	91	78	71	26	17	45	42	44
23	98	94	80	74	29	20	49	45	46
24	+ 2,00	96	83	77	32	23	52	48	49
25	02	99	85	80	35	26	55	51	52
26	04	+ 2,01	87	83	38	29	58	54	55
27	06	04	89	86	41	32	62	57	58
28	08	06	91	89	44	35	65	60	61
29	10	08	94	92	47	37	68	63	64
30	12	11	96	96	50	40	72	65	66
31	14	13	98	99	53	43	75	68	69
April 1	16	15	+ 2,01	+ 2 03	56	46	78	71	72
2	18	17	03	,06	58	48	82	74	75
3	20	20	05	10	61	51	85	77	77
4	22	22	07	14	64	54	89	80	80
5	24	24	09	18	67	57	92	83	83
6	26	27	12	22	70	60	96	86	86
7	28	29	14	26	72	63	99	89	89
8	30	31	16	30	75	66	+ 1,03	92	91
9	32	33	18	33	77	68	06	95	94
10	34	35	20	37	80	71	10	98	97
11	35	37	22	39	82	73	13	+ 1,01	+ 1,00
12	37	39	24	42	85	76	16	04	03
13	38	41	26	45	87	78	20	06	06
14	40	43	28	48	90	81	23	09	09
15	41	44	29	50	92	83	26	12	12
16	42	46	31	53	95	86	29	15	15
17	44	48	33	55	97	88	32	18	18
18	45	50	35	58	+ 2,00	91	36	21	21
19	47	51	37	60	03	93	39	24	24
20	48	53	39	63	06	96	42	27	27
21	49	54	41	65	08	98	45	30	30
22	51	56	42	67	10	+ 2,00	48	33	33
23	52	57	44	70	13	03	51	36	36
24	53	59	46	72	15	05	55	39	39
25	54	60	48	74	17	07	58	42	42
26	56	61	49	77	19	10	61	45	45
27	57	63	51	79	22	12	64	48	48
28	58	64	53	82	25	14	67	51	51
29	59	66	54	84	27	17	70	54	54
30	60	67	56	87	29	19	73	57	57

	β Aquilæ.	α Capricor.	α Cygni.	α Aquarii.	Fomalhaut	α Pegasi.	α Androm.	Polaris. H. M. S.
	″	″	′	″	″	″	″	
Mar. 1	−0,10	+0,01	−1,31	−0,33	−0,16	−0,59	−0,77	0.56.23,31
2	08	03	29	31	15	58	77	22,82
3	05	06	26	29	14	57	77	22,35
4	02	09	24	28	13	56	77	21,89
5	+0,01	12	21	27	12	55	77	21,46
6	03	14	19	26	11	54	77	2 ,07
7	05	17	16	24	10	53	77	20,79
8	08	19	13	23	09	52	77	20,55
9	10	21	11	22	08	52	77	20,32
10	13	24	08	21	07	51	77	20,08
11	15	26	06	19	06	50	77	19,83
12	17	29	03	17	05	49	77	19,59
13	20	32	01	16	03	48	76	19,32
14	22	34	−0,98	14	02	47	76	19,01
15	25	37	95	12	00	45	75	18,67
16	27	40	92	11	+0,01	44	75	18,32
17	30	43	89	09	02	42	74	18,05
18	32	46	86	07	04	41	74	17,80
19	35	49	83	05	05	40	73	17,59
20	37	51	80	03	07	38	72	17,43
21	40	53	77	01	09	37	71	17,31
22	43	56	74	+0,01	11	36	70	17,27
23	45	59	71	03	12	34	69	17,24
24	48	61	68	05	14	33	68	17,20
25	51	64	65	07	16	31	67	17,13
26	54	67	62	09	18	30	66	17,03
27	57	70	58	11	20	29	65	16,96
28	60	73	55	13	22	27	64	16,84
29	63	76	51	15	24	26	63	16,72
30	65	79	48	17	26	24	62	16 58
31	68	82	45	19	28	22	61	16,48
April 1	71	85	42	21	30	20	60	16,47
2	74	88	38	24	32	18	58	16,48
3	77	91	35	26	35	17	57	16,55
4	80	94	31	29	37	15	55	16,65
5	83	97	28	31	39	13	54	16,77
6	86	+1,00	24	34	41	11	52	17,08
7	89	03	21	36	43	09	51	17,23
8	92	06	17	38	46	07	49	17,35
9	95	09	14	41	48	05	47	17,43
10	98	12	10	43	51	03	46	17,47
11	+1,01	15	07	46	53	01	44	17,56
12	04	18	03	49	56	+0,01	43	17,65
13	07	21	00	51	58	03	41	17,76
14	10	24	+0,04	54	61	05	39	17,91
15	13	27	07	56	63	07	38	18,09
16	16	31	11	59	65	10	36	18,38
17	19	34	14	61	68	12	34	18,69
18	22	37	18	64	70	14	32	19,03
19	25	40	21	67	73	16	30	19,36
20	28	44	25	70	76	18	28	19,69
21	31	47	29	73	79	20	26	20,03
22	34	50	32	75	82	23	24	20,33
23	37	53	36	78	85	25	22	20,60
24	40	56	40	81	88	28	20	20,84
25	43	59	43	84	91	31	18	21,07
26	46	62	47	87	94	34	15	21,35
27	49	66	51	90	97	36	13	21,66
28	52	69	55	93	+1,00	39	11	22,01
29	55	72	59	95	03	41	08	22,40
30	58	75	63	98	06	44	06	22,84

	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran.	Capella.	Rigel.	β Tauri.	α Orionis.	Sirius.
	″	″	″	″	″	″	″	″	″
May 1	+0,21	—0,10	+0,03	+0,20	+0,26	+0,19	+0,41	+0,48	+0,37
2	24	08	04	20	25	18	40	47	36
3	26	07	05	20	25	18	40	47	35
4	28	06	06	20	24	17	39	46	34
5	30	05	07	20	24	17	39	45	33
6	33	03	08	20	24	17	39	44	32
7	35	02	09	20	24	16	39	43	31
8	37	01	09	20	23	16	39	43	30
9	40	+0,01	10	21	23	16	39	43	29
10	42	03	11	21	23	16	39	42	28
11	45	05	12	21	23	16	39	42	27
12	47	07	14	22	23	16	39	41	26
13	50	09	16	22	23	16	39	41	26
14	52	11	17	23	23	16	39	41	25
15	55	13	18	23	23	16	39	41	24
16	58	15	20	24	24	16	39	41	23
17	60	17	21	24	24	16	39	41	23
18	63	19	23	25	24	16	39	41	22
19	65	22	24	26	24	16	39	40	21
20	68	24	26	27	24	16	39	40	20
21	71	26	27	28	25	16	39	40	19
22	74	29	29	29	25	17	40	40	19
23	77	31	31	30	26	17	40	41	19
24	80	33	33	31	27	18	41	41	18
25	83	35	34	31	28	18	41	41	18
26	86	38	36	32	29	18	42	41	18
27	89	40	38	33	30	19	42	42	17
28	92	42	40	34	31	19	43	42	17
29	95	45	42	35	32	20	44	42	16
30	99	47	44	36	33	20	45	43	16
31	+1,02	50	46	37	34	21	46	43	16
June 1	05	52	48	39	36	22	47	44	16
2	09	55	50	40	37	23	48	44	16
3	12	57	52	42	38	24	49	45	16
4	15	60	54	43	39	25	50	45	16
5	19	63	57	45	41	26	52	46	16
6	22	66	59	47	42	27	53	47	16
7	25	69	61	48	43	28	54	48	16
8	28	72	64	50	45	29	55	48	16
9	31	75	66	52	46	30	56	49	16
10	34	78	68	54	48	31	57	50	16
11	37	81	71	56	50	33	59	51	17
12	41	84	73	57	51	34	60	52	17
13	44	87	76	59	53	35	62	53	18
14	47	91	78	61	55	36	63	54	18
15	50	94	80	63	57	38	65	56	18
16	53	97	83	65	59	39	66	57	19
17	57	+1,00	85	67	61	40	68	58	19
18	60	03	88	69	63	42	70	59	20
19	63	07	90	71	65	43	72	60	20
20	67	10	93	73	67	45	74	61	21
21	70	13	95	75	70	47	76	63	21
22	73	16	98	78	72	49	78	64	22
23	76	19	+1,00	80	75	50	80	66	23
24	79	22	03	82	77	52	82	67	24
25	83	26	06	84	80	54	84	68	25
26	86	29	09	86	82	55	86	70	26
27	89	32	12	89	85	57	88	71	27
28	92	35	15	91	88	59	90	73	28
29	96	39	17	94	90	61	92	75	29
30	99	42	20	96	93	63	94	77	30

	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Virg.	Arcturus.
May 1	+1″,24	+0″,92	+1″,25	+1″,36	+1″,87	+2″,36	+2″,24	+2″,42	+2″,68
2	22	91	23	34	86	35	23	42	69
3	21	90	22	33	85	35	23	43	70
4	20	89	21	31	84	34	22	43	70
5	19	88	20	30	83	33	21	43	70
6	17	87	19	29	82	32	20	43	71
7	16	86	18	27	81	31	20	43	71
8	15	85	17	26	80	31	19	44	71
9	14	84	16	25	79	30	19	44	72
10	13	83	15	24	78	29	18	44	72
11	12	82	14	23	77	29	18	44	72
12	11	81	13	22	76	28	17	44	72
13	10	80	12	20	74	27	17	44	72
14	09	79	11	19	73	26	16	44	72
15	08	79	10	18	72	26	15	44	72
16	07	78	09	17	71	25	14	44	73
17	06	77	08	16	70	24	13	44	73
18	05	76	07	15	69	24	13	43	73
19	04	76	06	14	68	23	12	43	73
20	03	75	05	13	67	22	11	43	73
21	02	74	04	12	66	21	10	43	73
22	02	74	03	11	65	20	09	42	73
23	01	73	03	09	64	19	08	42	73
24	01	73	02	08	63	18	07	41	73
25	00	72	01	07	62	18	07	41	73
26	00	72	00	06	61	17	06	41	72
27	00	72	00	05	60	16	06	40	72
28	+0,99	71	00	04	59	15	05	40	72
29	99	71	+0,99	03	58	14	04	40	71
30	98	71	99	02	57	13	03	39	71
31	98	71	99	01	56	12	03	38	71
June 1	97	70	98	00	55	11	02	38	71
2	97	70	98	00	54	10	01	37	71
3	97	70	98	+0,99	53	09	00	37	70
4	97	70	98	98	52	08	00	36	70
5	97	69	97	97	51	07	+1,99	36	70
6	97	69	97	96	50	06	98	35	69
7	97	69	97	96	49	05	97	35	69
8	96	68	96	95	48	04	96	34	69
9	96	68	96	94	47	03	95	34	68
10	96	68	96	93	46	02	94	33	68
11	96	68	96	93	45	01	94	33	68
12	97	68	96	92	45	00	93	32	67
13	97	68	96	91	44	+1,99	92	31	66
14	97	68	96	91	44	98	91	31	66
15	98	69	97	91	43	97	90	30	65
16	98	69	97	90	42	96	89	29	64
17	99	69	97	89	42	95	88	28	63
18	99	69	97	89	41	94	87	28	63
19	99	69	98	88	40	93	86	27	62
20	99	69	98	88	39	92	85	26	61
21	+1,00	70	99	87	39	91	84	25	60
22	00	70	99	87	38	90	83	25	59
23	01	71	+1,00	87	38	89	82	24	58
24	01	71	00	86	37	88	81	24	58
25	02	71	00	86	37	87	80	23	57
26	03	72	01	86	36	86	79	22	56
27	04	72	01	85	36	85	78	21	55
28	04	73	02	85	35	84	77	20	54
29	05	74	03	84	35	84	76	19	54
30	06	75	04	84	35	83	75	19	53

	α Libræ.	α Cor. Bor	α Serpent.	Antares.	α Herculis.	α Ophiuchi.	α Lyræ.	γ Aquilæ.	α Aquilæ.
May 1	+2″,61	+2″,68	+2″,57	+2″,89	+2″,31	+2″,21	+1″,76	+1″,60	+1″,60
2	62	69	59	91	33	24	79	63	63
3	63	71	60	93	35	26	82	66	66
4	64	72	61	95	37	29	85	69	69
5	65	73	62	97	39	31	87	71	72
6	66	75	63	99	41	33	90	74	75
7	67	76	64	+3,01	43	36	93	77	78
8	68	77	66	03	45	38	96	80	81
9	69	78	67	05	47	41	99	83	84
10	70	79	68	07	49	43	+2,02	86	87
11	71	80	69	09	51	45	05	89	90
12	71	81	70	10	53	48	08	92	93
13	72	82	71	12	55	50	10	95	96
14	72	83	72	14	57	52	13	98	99
15	73	84	73	16	59	54	15	+2,01	+2,02
16	73	85	74	18	61	56	18	04	05
17	74	86	75	19	63	58	20	07	08
18	74	86	76	21	65	60	23	10	11
19	75	87	77	22	66	62	25	12	14
20	75	87	78	24	68	64	28	15	17
21	76	88	79	25	69	66	30	18	20
22	77	89	80	26	71	67	32	20	22
23	78	89	81	28	72	69	35	23	25
24	78	90	82	29	74	70	37	25	27
25	78	90	82	30	75	72	39	28	30
26	79	91	83	31	76	74	41	31	33
27	79	91	84	32	78	75	43	33	35
28	79	92	85	34	79	77	46	36	38
29	80	92	85	35	81	78	48	38	40
30	80	93	86	36	82	80	50	41	43
31	80	93	86	38	84	81	52	43	45
June 1	80	94	87	39	85	83	54	46	48
2	80	94	87	40	86	84	56	48	50
3	81	94	87	41	87	86	58	51	53
4	81	94	87	42	88	87	60	53	55
5	81	95	88	43	90	89	62	56	58
6	81	95	88	44	91	90	64	58	60
7	81	95	88	45	92	91	65	61	63
8	81	95	89	46	93	93	67	63	65
9	81	95	89	47	94	94	69	66	68
10	80	95	90	47	95	95	70	68	70
11	80	94	90	48	96	96	72	70	72
12	80	94	90	48	96	97	73	72	75
13	80	94	90	49	97	98	75	74	77
14	80	94	91	49	98	99	76	76	79
15	80	93	91	50	99	+3,00	77	79	81
16	79	93	91	50	+3,00	02	79	81	83
17	79	93	91	51	01	03	80	83	86
18	79	92	92	51	02	04	82	85	88
19	79	92	92	52	03	05	83	87	90
20	79	91	92	52	03	06	84	89	92
21	78	91	92	53	04	07	85	91	94
22	78	90	92	53	04	08	87	93	96
23	78	90	92	54	05	08	88	95	98
24	78	89	92	54	05	09	89	97	+3,00
25	77	89	91	55	06	10	90	99	02
26	77	88	91	55	06	10	91	+3,01	04
27	76	88	91	56	07	11	92	03	06
28	76	87	90	57	07	11	93	05	08
29	75	87	90	58	08	12	94	07	10
30	74	86	90	58	08	12	94	08	11

	β Aquilæ.	α Capricor.	α Cygni.	α Aquarii.	Fomalhaut	α Pegasi.	α Androm.	Polaris. H. M. S.
	″	″	″	″	″	″	″	
May 1	+1,61	+1,78	+0,67	+1,01	+1,09	+0,47	−0,04	0.56.23,34
2	64	81	70	04	12	50	01	23,86
3	67	85	74	07	15	53	+0,01	24,38
4	70	88	77	10	18	56	04	24,87
5	72	91	81	13	21	59	06	25,32
6	75	94	85	16	25	62	09	25 78
7	78	97	88	19	28	65	11	26,21
8	81	+2,01	92	22	32	68	14	26,62
9	84	04	95	25	35	71	17	27,01
10	87	07	99	28	38	74	20	27,43
11	90	10	+1,03	31	41	77	23	27·91
12	93	13	07	34	45	80	26	28,43
13	96	17	11	37	48	83	29	28,99
14	99	20	14	40	51	86	32	29,60
15	+2,02	23	18	44	55	89	35	30,23
16	05	26	22	47	58	92	38	30,90
17	08	29	25	51	62	95	41	31,55
18	11	32	29	54	65	98	44	32,18
19	14	35	32	56	69	+1,01	47	32,77
20	17	38	36	59	72	04	50	33,32
21	19	41	39	62	76	07	53	33,87
22	22	44	43	65	79	10	56	34,40
23	25	47	46	68	82	13	59	34,93
24	28	50	49	71	86	16	62	35,48
25	30	52	52	74	89	19	65	36,06
26	33	55	56	77	92	22	68	36,70
27	36	58	59	80	96	25	72	37,40
28	39	61	62	83	99	28	75	38,14
29	41	64	66	86	+2,03	31	78	38,89
30	44	67	69	89	06	35	81	39,63
31	46	70	73	93	10	38	84	40,38
June 1	49	73	76	96	14	41	88	41,10
2	51	76	79	99	17	44	91	41,79
3	54	79	82	+2,02	21	47	95	42,43
4	56	82	85	05	24	50	98	43,04
5	58	85	89	09	28	54	+1,02	43,65
6	61	88	92	12	31	57	05	44,28
7	63	91	95	15	35	60	08	44,92
8	66	93	99	18	38	64	12	45,60
9	68	96	+2,02	21	42	67	15	46,32
10	71	98	05	24	45	70	18	47,09
11	73	+3,01	08	27	49	73	22	47,91
12	76	03	10	30	52	77	25	48,74
13	78	06	13	33	56	80	29	49,56
14	80	08	16	35	59	83	32	50·35
15	82	11	19	38	63	87	36	51,12
16	84	13	22	41	66	90	39	51,85
17	87	16	24	44	70	94	43	52,54
18	89	18	27	47	73	97	47	53,21
19	91	21	30	50	77	+2,00	50	53,86
20	93	24	32	53	81	03	53	54,53
21	95	26	35	56	84	06	57	55,23
22	97	29	37	59	88	09	61	55,97
23	99	31	40	62	91	12	64	56,75
24	+3,01	33	42	65	95	15	67	57,57
25	03	35	45	68	98	18	71	58,42
26	05	37	48	71	+3,02	21	74	59,27
27	07	40	50	74	05	24	77	0,57, 0,10
28	09	42	53	77	09	27	81	0,91
29	11	44	55	80	12	30	84	1,69
30	12	46	57	82	15	33	87	2,42

Art. XXI. *Miscellaneous Intelligence.*

I. Mechanical Science.

§ Agriculture, the Arts, &c.

1. *Prize Questions in Agriculture and the Arts.*—The following questions have been announced by the Haerlem Philosophical Society. Memoirs on them will be received till January 1, 1822.

i. " What information has been obtained respecting the nature, habits, and production of those little insects which are so injurious to plants cultivated in hot-houses; and what method would such information suggest for preventing the propagation of such insects, or for extirpating them?"

ii. " As extensive hot-houses are now heated by steam in England, might not this method be adopted among us for small hot-houses; and what would be the most proper apparatus for such a purpose?"

iii. " Has experience clearly proved that there are certain trees and plants, particularly of the most useful species, which cannot vegetate when close to each other? and, in this case, what experiments can be adduced as proofs? Can this antipathy between some species be any way accounted for by what we know of the nature of plants, and what useful information does it supply us with for the cultivation of trees and useful plants?"

iv. " What are the insects most hurtful to trees and shrubs in forests? In what consists the injuries they produce? What are the remedies proper to prevent such injuries, or to remove them?"

v. " How far are we acquainted with the œconomy of moles, and what means does it suggest as most efficacious for ridding lands of them where they prove destructive? Are there, on the contrary, any observations tending to prove that moles are ever useful by destroying other vermin, and how may it be known when moles ought to be tolerated?"

vi. " Dry yeast having been substituted for moist in brewing, the society demands ' a comparison founded upon chemical analysis of the nature of yeast, both in the moist and dry forms; and a statement of their relative qualities.' 2. That a method be pointed out by which liquid yeast may be freed from the bitter and disagreeable flavour occasioned by the hop used in brewing? 3. That some means be shewn by which liquid yeast might be preserved, for at least some time, so as not to lose the power of fermenting dough?"

vii. "It having been observed in many places, and it being still observed, that a variety of plants, whose growth is rapid, produce a kind of peat, the society wishes to have a succinct and exact statement and comparison of whatever has been described, or may be observed on this subject; likewise, to have it discussed what methods ought to be observed, in order to promote the growth of some species of peat."

2. *Remedy for Mildew in Wheat.*—Dr. Cartwright, during his investigation of the effect of salt upon vegetables, was led to apply it as a remedy for the mildew in wheat. The mode of applying it is to sprinkle the corn with a solution of the salt, the object being to wet the straw in which the mildew exists. The experiments, upon trial, were very successful, scarcely any remains of the disease being to be found forty-eight hours after the sprinkling. Six or eight bushels will serve an acre, and the expense of the salt will be repaid by the improvement of the manure made from the salted straw. Two men, one to spread, and the other to supply him with the salt, will get over four acres in a day. The effect of the remedy depends upon the circumstance, that though the solution of salt has no injurious action on the stem and fibrous parts of vegetables; yet, on getting to the roots in sufficient quantity, they languish and die. The salt is considered as acting on the fungus which occasions mildew in the same manner as on weeds.—*Phil. Mag.* lvi. p. 395

3. *Yeast as a Manure.*—Mr. P. Taylor, of Bromley, made trial this summer of common porter-yeast as a manure upon a grass field. The effect in the month of June was very evident,

for all that part manured by the yeast was covered with grass of a deeper colour and more luxuriant growth than elsewhere; hence, it is probable, that yeast may in many cases be found a very convenient, advantageous, and useful manure.

4. *Reaping of Corn.*—The French claim the merit of a new discovery, of great importance to agriculture and public œconomy in the advantages which, according to them, result from the practice of reaping corn before it is perfectly ripe. This theory, which has just been promulgated by M. Cadet de Vaux, originates with M. Salles, of the Agricultural Society of Beziers. The following are the particulars:—Corn, reaped eight days before the usual time, is, in the first place, secured from the dangers which threaten it at that time; this is only accidental, but a positive advantage is, that the grain is fuller, larger, finer, and that it is never attacked by the weevil. The truth of these assertions has been proved by the most conclusive comparative experiments upon a piece of corn, one half of which was reaped before the usual time, and the other half at the degree of maturity fixed by the ordinary practice. The first portion gave a hectolitre (26.4 gallons) of corn more for half a hectar (5,980 square yards) of land. Afterwards an equal quantity of flour from the wheat of each portion was made into bread; that of the corn reaped green, gave seven pounds of bread more than the other in six decalitres (15.84 gallons). Lastly, the weevil attacked the corn which was cut ripe; the other was exempt from it. The proper time for reaping is when the grain, on being pressed between the fingers, has a doughy appearance, like the crumb of bread just hot from the oven, when pressed in the same manner.—*New Monthly Magazine*, 1820, p. 447.

5. *Spade Husbandry.*—A field of seven acres, situated in the county of Surrey, in the last year, was prepared for barley by the spade. The labourers employed, earned in the winter at the rate of 15*s.* per week, 2*d.* per rod being given for digging,

and the proprietor considers that it would have cost him double the expense if he had it ploughed.

Mr. Falla, of Gateshead, Northumberland, has this year grown upon land worked by the spade, pieces of wheat transplanted from a seed-bed into rows six inches apart, which produced 17 coombs per acre; and one 12 inches, which produced 15 coombs; a fourth piece sown in drill, and a fifth in broad cast, yielded 19 coombs per acre. The produce of the land by ploughing is usually about 6 coombs.

6. *Ripening Wall-Fruit.*—Mr. H. Davis, of Slough, has published the result of an experiment for facilitating the ripening of wall-fruit, by covering the wall with black paint. The experiment was tried on a vine, and it is stated that the weight of fine grapes gathered from the blackened part of the wall was 20 lbs. 10 oz., while the plain part yielded only 7 lbs. 1 oz., being little more than one-third of the other. The fruit on the blackened part of the wall was also much finer, the bunches were larger, and ripened better than on the other half; the wood of the vine was likewise stronger, and more covered with leaves on the blackened part.

7. *Protection of Fruit from Wasps.*—Mr. Knight has found his vinery to be perfectly protected from the attacks of the wasp, in consequence of the vicinity of some young yew trees, which, since they have come into bearing, and produced berries, have constantly attracted these insects from the vines. The wasps feed upon the berries with much avidity, and from the sweetness of their taste, and the quantity of mucilage they contain, they are probably very nourishing.

8. *Dry Rot.*—Colonel Gibbs of the United States, in speaking of the dry rot, mentions some facts of great importance that had been stated to him by Colonel Perkins of Boston. Several ships built at Boston have been filled in between the timbers with salt, whilst on the stocks, and after 10 or 15 years, the wood has invariably been found to be quite sound. A large

ship, belonging to Colonel Perkins, which had been salted when built fourteen years ago, required extensive repairs, and a complete examination was made as to the state of the timbers, they were found every where perfectly sound. A vessel of 500 tons required 500 bushels of salt, and after two years 100 bushels more to fill up the space left by salt dissolved away.

9. *Preservation of Eggs.*—It is proposed to preserve eggs by covering them with a coat of gum arabic, rather than of varnish, and then to imbed them in charcoal. The gum is readily removed by water, and the charcoal preserves the eggs from any great and sudden change of temperature in passing from one country or situation to another.

10. *Le Bateau Roulant.*—Some trials of a boat, on a new construction, are said to have lately been made at Paris. In the second trial the inventor placed himself, with his apparatus, below the platform of the Pont-Neuf. He set out from this point at ten minutes before ten o'clock, having on board M. Dacheux, an experienced mariner, who took charge of the helm; Messrs. Marlet and Thibault, inspectors of the navigation, followed in another boat to observe the operations. In twenty minutes at the utmost he proceeded beyond the Pont Royal, after having passed and re-passed under the arches, and landed opposite the Quay d'Orsay. There he made his land apparatus act, and roll the boat to the school of natation, which was the end of his expedition.

The inventor asserts that his machine will roll the boat on the land, or navigate it in the water with equal ease, and that neither motion is interrupted, or the velocity impeded. The boat may go with the wind, or against it, and tack, ascend, and descend a river at pleasure, and that with more rapidity than a common boat.

11. *Whale-Torpedoes.*—A vessel has recently been fitted at New Bedford, in America, bound on a whaling voyage, with an

apparatus on board for the purpose of blowing them up. Torpedoes of an arrow form are thrown from a gun on board the vessel, which are calculated to sink into the body of the whale, and there explode.

12. *Terrestrial Globe in Relief.*—Charles P. Khummer, of Berlin, has lately published a globe, on which the mountains are beautifully executed in relief. It is admirably calculated for communicating permanent and accurate ideas of the distribution and grouping of the great ranges of mountains and table lands in the different quarters of the globe. There are globes of this description of different sizes and prices. Globe, 16 inches diameter, eight dollars without names; with names, eleven dollars. Globe, 26 inches diameter, 25 dollars without names; with names, degrees, and finely finished, fifty dollars.

13. *Light-House.*—A new light-house is erecting at the Tour des Baleines, Isle of Rhé. The light will make one revolution in six minutes. In the interval there will be four appearances of a white and very brilliant light. At each interval of 90 seconds a very sparkling lustre will be seen for about fifteen seconds, and will gradually diminish till it disappears.

The board of the Marine in Sweden has recently published a notice, announcing that the light-house of the Tower of Carlsten, near Marstrand, will be pulled down and rebuilt in the course of 1822. The execution of this project to commence April 16th, 1821, and the flame to be extinguished from the 15th of the same month.

14. *Mathematical Prize Question.*—The class of mathematics of the Royal Academy of Sciences of Prussia, has proposed the following question:—"To give a mathematical explanation of the luminous and coloured rings, which are sometimes observed round the sun and moon, agreeing with experiments on light and the constitution of the atmosphere, and with observations of the phenomena, made with all the precision possible." The memoirs to be sent in before the expiration of

March, 1822. The prize of fifty ducats will be adjudged at the public setting, on the anniversary of Leibnitz, on the 3rd of July following.

II. Chemical Science.

§ Chemistry.

1. *On the Application of Chromate of Lead to Silk, Wool, Linen, and Cotton, by* M. J. L. Lassaigne.—The colouring matters fixed on these substances were formerly obtained from organic bodies. Mineral substances, so abundant in unalterable coloured combinations, give none to the dyer. It is only within these last few years that mineral preparations have been applied in dyeing. M. Raymond of Lyons is the first, who, by a simple and ingenious process, fixed Prussian blue on silk; and last year, M. Braconnot of Nancy, by applying the sulphuret of arsenic to cloth, *&c.*, furnished a yellow colour not less durable.

In the course of some experiments on the chromate of lead, I succeeded by an analogous process to that of M. Raymond, in combining this salt with all the substances mentioned above. Skeins of silk were placed at the common temperature in a weak solution of sub-acetate of lead for a quarter of an hour, and then removed and washed in abundance of water. These skeins were then put into a weak solution of neutral chromate of potash; they immediately became of a fine yellow colour, which increased for ten minutes. When they had obtained the maximum of colour, they were taken out, washed and dried.

This colour is unalterable in the air. By varying the proportions of sub-acetate of lead and chromate of potash, various tints may be produced.

The same process succeeds with wool, cotton, and linen, but it is better to place these substances in a solution of sub-acetate of lead, raised to the temperature of 55° or 60°. (130° to 140° F.)

The circumstance that this, like the other mineral colours, is in part decomposed by soap, induces me to suppose that it will only be useful in dyeing silk.

In place of the neutral chromate of potash, the solution of the native chromate of iron, acted on by nitre, and neutralized by nitric acid, may be used with the same advantage.—*Annales de Chimie*, xv. p. 76.

2. *Sulphuret of Chrome.*—M. J. L. Lassaigne has succeeded in preparing this substance, by acting on the chloride of chromium with sulphur. A chloride is first prepared, by boiling chromic acid with muriatic acid in excess, and evaporating to dryness; the dry mass is then mixed with five times its weight of sublimed sulphur, and heated to whiteness in a bent glass tube; a sulphuret of chromium is obtained. The sulphuret is of a blackish grey colour, of an unctuous feel, very light, easily falling to powder, and when rubbed on bodies leaving marks similar to those of plumbago. When heated red in a platinum crucible, it burns like pyrophorus, gives out fumes of sulphurous acid, and a deep-green coloured oxide of chromium remains. Nitric acid does not act readily on it, but aqua-regia dissolve it. It is composed of chromium 100, and sulphur 10.54.

3. *Preparation of the Oxide of Chromium.*—In consequence of the preceding experiments, M. Lassaigne has devised a new and economical process for the preparation of the green oxide of chromium. It consists in calcining a mixture of equal parts of chromate of potash and sulphur in a close earthen crucible, at a red heat, and in washing the green mass which is produced, to dissolve out the sulphate and sulphuret of potash. The oxide of chromium remains, and by repeated washings is rendered pure.

It is not necessary that the chromate of potash should be in a crystalline state. The oxide was obtained of an equally fine colour, by calcining sulphur with the produce of the evaporation of the solution of chromate of iron, treated by nitre, to which had previously been added a little sulphuric acid to precipitate the alumine and silex that had been taken up in the operation. —*Annales de Chimie*, xiv. p. 299.

4. *Chromates of Potash.*—Dr. Thomson gives the following as the composition of these salts:

Chromate of Potash.

Chromic acid.......	52.	.... or	108.33
Potash............	48.	....	100

Bi-chromate of Potash.

Chromic acid.......	63.421	.... or	216.98
Potash............	31.579	....	100.

5. *Metallographical Application of fusible Metal.*—This alloy is composed of eight parts of bismuth, five of lead and three of tin, and its property of fusing at the boiling point of water is well known. M. Gassicourt has proposed a metallographical use of it, founded upon the extreme accuracy with which, in casting, it preserves the marks and traces on the mould. He illustrates his new application of it in the following manner: Paste a piece of white paper at the bottom of a china saucer, and let it dry: then write on it with common writing-ink, and sprinkle some finely-powdered gum-arabic over the writing, which will produce a slight relief. When well dried, brush off the powder that does not adhere, and pour fusible metal into the saucer, taking care to cool it rapidly that crystallization may not take place. In this way a counterpart of the writing will be obtained, impressed on the metal. By immersing the cast in slightly-warm water, any adhering gum may be removed, and then, if examined by a glass, the writing may easily be read and seen to be perfect. Afterwards, by using common printers' ink, impressions may be taken from it, all of which will be true *fac-similes* of the first writing.

The difficulties in this new application of the fusible alloy, are, to avoid unequal thickness in the plate of metal, which causes it to alter in form and break under pressure; and to prevent the surface from crystallizing, when the ink will adhere where it is not required.

6. *Reduction of Chloride of Silver.*—Chloride of silver is, from the various processes of analysis, &c., constantly accumulating

in the laboratory. In order to reduce it economically, put it into a small vessel of zinc or cast-iron, containing a little water and leave it there for a short time. If the vessel be clean, the decomposition will soon be effected, otherwise a little muriatic or sulphuric acid may be added. When decomposed, wash it with a little muriatic acid. (See vol. viii., p. 374.)—*Annales de Chimie*, xiv., p. 319.

7. *Sulphate of Platinum a Test for Gelatine.*—Mr. E. Davy recommends the use of the sulphate of platinum in detecting small quantities of gelatine. From comparative experiments made with it, and astringent infusions, he found, that when the quantity of gelatine was so small as not to be effected by strong infusions of oak-bark, nut-galls, or catechu, still there was an immediate precipitate on adding the sulphate of platinum. Where the proportion of gelatine was so reduced as not even to affect sulphate of platinum at first, the precipitate was immediately produced on boiling the fluid.

The different astringent infusions, as of oak-bark, nut-galls, catechu, *&c.*, do not act uniformly on the various kinds of gelatine: thus, an infusion of catechu would produce no precipitate in solutions of paper-hangers' size, but the sulphate of platinum acts equally on all kinds of size, and throws down precipitates which appear to be always similar, not being affected even by the presence of free acid in the solution.

8. *Spontaneous Combustion of Oatmeal.*—A gentleman removed with his family from Glasgow to Largs, in May last, and shut up his house, which was not re-opened until the end of August. The house stands on the side of a steep declivity, so that the kitchen, which is in the back part, though sunk considerably below the level of the street, is entirely above ground, and is well lighted and ventilated. In an opening of the wall near the kitchen fire-place (originally intended, it is supposed, for an oven) there was placed a wooden barrel, bound with iron hoops, and filled with oatmeal. This meal had

heated during the absence of the family, had at last caught fire, and was totally consumed, together with the barrel which contained it, nothing remaining but the iron hoops and a few pieces of charcoal. It is presumed that the meal had been somewhat moist, and that it had heated precisely in the same way that hay does when stacked moist. The kitchen did not seem to be unusually damp on the day when the house was opened. Dr. Thomson remarks, that the great avidity which oatmeal has for moisture, and the heat generated by the absorption of it, must be familiar to every one who has been in the habit of seeing oatmeal. Mr. Leslie has taken advantage of its avidity for moisture, and has applied it in the place of sulphuric acid in his well-known and ingenious process of freezing in the exhausted receiver of the air-pump.—*Annals of Philosophy*, vol. xvi., p. 390.

9. *Effects produced by Time on Wood buried in the Ground.*—Whilst cutting and carrying away a part of Castle-Field, near Manchester, an ancient well was discovered about four yards below the level of the field. It was square and formed of four upright posts driven at the angles into the clay, and closed in by other logs of wood, placed one upon another on the outside, so as to form a kind of chest which was floored with the same material. The logs were rudely hewn, had never been sawn, and were five or six inches square. The upper logs were level with the top surface of a bed of clay by which the well was surrounded, and into which the timber was inserted. The wood, when first discovered, had little more consistency than paste, but, on its exposure to the air, became much harder and more wood-like; it was perfectly black, and had so much of a coal-like appearance as to favour the theory of those who suppose that pit-coal was originally a vegetable substance. At the bottom of the well some large stones, such as in this neighbourhood are called bowlers, were found. They were black and dirty as though they had been taken from a sewer, and the clay which adhered to the timber had also changed its colour from the rusty iron tinge of the native clay to the appearance

of the inferior potters' clay found in Dorsetshire. Over the well were various unbroken strata of sand and gravel, which, as the bank was broken down, gave proof that, except for about a yard and a half below the surface of the field, it had never been exposed to day-light since the strata had been deposited. The foundations of some ancient Roman fortifications occur a few yards to the west of the well, which, from the appearances, must have been laid after the well was formed. The well is supposed to have been the work of the ancient Britons, and to be upwards of 2,000 years old, "for it is 1,741 years since the Romans settled here, and the section of the foundation which intersects the line of strata above the well is proof that they were not aware of its existence."—*Gentleman's Magazine*, 1820, p. 350.

10. *Test Infusion of Violets.*—M. Pagenstecher says that a concentrated infusion of violets may be preserved good for a long time if it be exposed in a corked bottle to the action of boiling water for a quarter of an hour; it is then to be taken from the water, and set aside without having been uncorked. This process was first proposed by M. Appert.

11. *Wodanium.*—It appears that M. Stromeyer has been engaged in analyzing the minerals in which M. Lampadius found this new metal; his object being to verify the discovery. He could, however, obtain nothing but copper, iron, nickel, cobalt, lead, antimony, arsenic, and sulphur. The wodanium was wanting.

12. *On Iodine and its Existence in Sponge.*—M. Straub of Hofwyl, as early as December, 1819, appears to have shown the existence of iodine in sponge, and proposed the preparation of an artificial substance, containing iodine, to be used instead of the *spongia usta* in medicine. In order to obtain the iodine from sponge, the latter, after being burnt, was washed with water, and the solution decomposed by sulphuric acid; and in this way so much was obtained from half an ounce of sponge

as to confirm the ideas previously entertained that its medicinal properties were owing to this substance.

M. Straub recommends trials of preparations of iodine in medicine, and thinks, that where salts formed from it, cannot be obtained, an alcoholic extract of burnt sponge is much to be preferred to the burnt sponge itself.

M. Straub also asserts the existence of iodine in turf. He was led to examine this substance in consequence of the peculiar odour he observed in the neighbourhood of those buildings where turf is burnt. Repeated experiments confirmed this conjecture; and, by acting on 2 lbs. of turf, abundant evidence of the existence of iodine in it may be obtained. It was found also in the cinders of the *helmintocorton*, though in very small quantities.—*Bib. Univ.*, xiv, p. 301.

13. *Cantharadin.*—Dr. J. F. Dana states that the *lytta vittata*, or common potato-fly, of North America, contains cantharadin as well as the *meloe vesicatoria*, and that the vesicatory powers of this fly are superior to those of the Spanish-fly. The experiments were made on a small scale, from the difficulty of procuring a sufficient number of the flies.

14. *Preparation of Specimens of Animals.*—It is usual in preparing specimens of animals to apply an arsenical or other poisonous preparation to them, to prevent the attacks of insects, which so frequently injure and destroy them. A soap, containing arsenic, is often used for this purpose; but M. Drapiez has found, that soap, made of potash and fish-oil, is not only as much, or more, destructive of insects, but more readily applied in general, more applicable to parts to which the former cannot be used, and free from many of the faults of the first. The soap is prepared by dissolving one part of caustic potash in a sufficient quantity of water, and adding to it one part of fish-oil; the mixture is to be triturated until of sufficient consistence, and then hardened by evaporation. When well dried, it is to be rasped into a very fine powder, and then mixed with an equal weight of camphor minutely divided by the assistance of an

alcoholic tincture of musk. The external and delicate parts of animals, as the feathers, skin, *&c.*, may be easily preserved by this powder. For this purpose they are to be sprinkled with the powder, and then, the excess being removed, they are to be placed in a damp situation; the particles will attract water, and will form a sort of gum on the parts. They may then be placed in a dry atmosphere: the covering, without interfering in the slightest manner with the appearance, will preserve them perfectly. If the soap be required in a soft state for application to the skin, it may easily be made so by adding the camphor, whilst in solution in the tincture of musk, to the powdered soap, and making the whole into a paste.

M. Drapiez ascertained the perfect security afforded by this process by placing specimens so prepared under glasses with the larvæ, which are so generally destructive to them. They remained untouched, and in perfect preservation.

15. *Observations made during the late Solar Eclipse.*—During the solar eclipse which took place on the 7th of last September, some observations were made by M. Necker, of Cologny, near Geneva, with two very delicate thermometers, graduated by Reaumur's scale, one of which was placed so as to receive the full force of the sun's rays, and the other near the first, but on the north side of a tree in the shade; both were about four feet from the ground. The following are some of the results:

H.	M.	Thermometer exposed. °	°	Thermometer in the Shade. °	°
At 1		28.5 R. or	96 F.	16.5 R. or	69.1 F.
1	30	29.	97.25	17	70.25
2		23.5	84.9	16	68.
2	35	16.5	69.12	14.75	65.18
2	55	20.5	78.1	15	65.75
3	15	25.	88.25	16	68.
4	10	26.	90.5	17	70.25

Ther. in the sun's rays, max. 29 (97.25 F.) min. 16.5 (69.12 F)
Ther. in the shade, ...max. 17 (70.12 F.) min. 14.75 (65.18)
Difference of the maxima of the two thermometers 12 R. (27° F.)
Difference of the minima of the thermometers . . . 1.75 (4° F.)

It was remarked at Cologny, that whenever the sun's rays during the eclipse penetrated through the foliage of the trees, so as to pass on to the ground, the images formed, instead of being circular, as is usually the case, were crescents, varying in form with the progress of the eclipse. This, though naturally to be expected, had a singular effect from the number of images grouped together.—*Bib. Univ.* xv. p. 14.

16. *On the Dip of the Needle and Intensity of the magnetic Force.*—The following observations, on the dip of the needle and the intensity of the magnetic force, have been collected and calculated by Professor Hansteen:

	Dip.	Intensity of the Magnetic Force.
	°	
Peru	0 0	1.0000
Mexico	42.10	1.3155
Paris	68.38	1.3482
London	70.33	1.4142
Christiana	72.30	1.4959
Arendahl	72.45	1.4756
Brassa	74.21	1.4941
Hare's Island	82.49	1.6939
Davis' Straits	83.8	1.6900
Baffin's Bay	84.25	1.6685
" "	84.39	1.7349
" "	84.44	1.6943
" "	85.54½	1.7383
" "	86.9	1.7606

Edin. Jour. iii. p. 401.

17. *The Coe Fire of Derbyshire.*—Mr. Bainbridge thus describes this phænomenon. " It resembles a column of smoke rising up from the woods that clothe the sides of many of the peak hills, and is observed when there is a thick atmosphere, or a light mist is setting upon the hills. Sometimes a single column is seen, becoming divided shortly into several smaller ones, and again re-uniting. The idea given to a stranger is that of

the smoke of a fresh-kindled cottage fire ascending from the bosom of the wood; and it is not until, by a closer attention, he observes the inconstancy and mutability of this aërial phantom, that he can be undeceived."

The "coe-fire" is observed when the atmosphere is unagitated by the least breath of wind, and is attributed by Mr. Bainbridge to the electricity of the clouds which hang over the place where the phenomenon exists. The electricity acting more at one point than another, is supposed to cause the condensation of aqueous vapour, and so to alter the specific gravity of the atmosphere at that spot. This would give rise to current in the air, and the shifting motion of the influence under which they are produced, would account for the variation in size, number, and place, that they are liable to.—*Monthly Magazine*, 1820, p. 206.

18. *Discharge of Lightning through a bad Conductor.*—On May 13, the lightning fell at 9 o'clock in the evening, on a house at Berne, in Switzerland, furnished with a conductor too small to convey away the whole of the electricity. In consequence of the illustration which this circumstance offered, of the utility of good conductors and danger of bad ones, M. Tretchsel was appointed to examine into the phænomena which had taken place.

The house stood alone on a plain elevated above the river Aar. It was thirty feet long, and covered by tiles. Three families lived in the eastern part of it. The western part was stables, &c.; the lightning conductor was fixed to a rod of wood, attached to the roof of the house near to two chimneys. On leaving the rod it descended without being in contact with the roof on the south side of the house, and entered the earth near the trunk of a tree.

When the lightning descended, the light was intense; and a woman, with her child and a domestic, who were in the house, were thrown to the earth senseless. A woman in the kitchen said she saw the fire descend by the chimney and roll towards the door; and a man standing at the window saw the lightning roll on the earth near the lightning-rod. The chimney nearest to the lightning-rod was much broken above, as was also the hearth beneath, from whence upwards two lines of tiles wêre shattered to pieces. Within the roof two pieces of carpentry were broken to pieces at the place where an iron bolt had fastened them together, and from this spot the course of the lightning could be traced by its effects to the place where it had struck the domestic (one of the three before spoken of) on the shoulder and thence to the ground, and the courses also of two other branches of the lightning, one within, and one on the roof of the house, could be traced.

The effects on the lightning-rod were as follows :—The brass point was slightly fused, but the iron head, being very strong and solid, had not been affected. The conducting wire of iron was three lines in diameter, and at the place where it communicated with the iron head, had been heated red hot for the length of a fathom (brasse) as was proved by the black colour it had evidently very recently assumed, and by the softened or annealed state of the metal : still more decided marks of a red heat were found on the iron wire, which descended along the trunk of an apple-tree, and which was only two lines in thickness. The earth in this place had been moved, and notwithstanding the heavy rain which had fallen, the part at the foot of this tree was dry though covered with verdure.

Hence it appears that the lightning had first descended entire on the conductor, but, that the wire being too small to convey the whole current away, the electricity divided there into several portions ; the larger, probably, of these was led off by the conductor heating the wire in its passage ; and the circumstance that the upper wire had not been heated red near the roof may be explained by supposing a portion of the electricity to be dissipated on the roof itself. The second wire, forming the continuation of the conductor was much thinner than the first, and passed down over the trunk of the tree. In consequence of its smallness it was more highly heated, and had, with the electricity, carbonized the part of the tree over which it passed. The earth at the foot of the conductor, being dry and sandy, was a bad conductor, and hence the electricity passed over its surface to a moistened and better conducting place.

Whilst M. M. Treschsel and Schenk were making their observations, a second storm arose, and they had an opportunity of observing the effects on the rod; these were not so powerful as before, but were still of a very imposing kind. The crackling noise of the electricity could be distinctly heard at the point, which, at the same time, was surmounted by the luminous star. Both these effects ceased whenever M. Schenk, by holding a steel key in the air, formed a second lightning-rod, and divided the effect with the one on the house. This, however, was too dangerous an experiment to be long continued.—*Bib. Univ.* xv., page 19.

19. *Sea-Salt in Vesuvius.*—M. Gimbernat has observed, that within a few days after the late eruption from Mount Vesuvius the crater was covered with crystals of sea-salt.

20. *Meteoric Stone.*—A meteoric stone fell on October 13, 1820, near Kostritz, in Russia, and has lately been analyzed by Stromeyer, who found it to contain

Silica	38.0574
Magnesia	29.9306
Alumina	3.4688
Protoxide of iron	4.8959
Oxide of manganese	1.1467
Oxide of chromium	1298
Iron	17.4896
Nickel	1.3617
Sulphur	2.6957
	99.1762

21. *On the Chromate of Iron in the Shetland Islands.*

TO THE EDITOR.

Sir.—It is with some surprise that I perused a notice in your last *Journal* in the following words: "It has been recently asserted that the chromate of iron was discovered in Shetland by Dr. Hibbert. Without wishing to undervalue Dr. Hibbert's labours, we must, in justice to Dr. Trail, remark that he pointed out the existence of this mineral in Unst many years ago. It is true that he calls it magnetic iron ore, but the existence of a chromate was then unknown."

In reply to this statement I have first to observe, that both magnetic iron ore and chromate of iron are found abundantly in the island of Unst. But waving this circumstance, I am totally unacquainted with any report published by Dr. Trail upon the mineralogy of this island, in which the existence even of magnetic iron ore was pointed out. Dr. Trail's description of the rocks of Shetland appeared in Mr. Neil's tour through that country in the year 1803, and from this work it was copied verbatim into the 15th volume of *Nicholson's Journal.* It will be there found that not a word is mentioned of any metallic substance occurring in the island of Unst except bog iron ore, with which mineral it is needless to observe that the chromate of iron could not possibly have been confounded.

I can consider the information which gave rise to your notice, in no other light than as an unintentional mistake. On this account, any further remarks that suggest themselves in obvious refutation of the statement are, at present, better suppressed, since they might only have the improper tendency of leading you to imagine that I entertain a contrary supposition.

I am, &c.,

10, *Argyle-square, Edinburgh,* SAMUEL HIBBERT.
Nov. 22, 1820.

22. *On rendering Cloth incombustible.*—M. Gay Lussac has proposed a means of rendering the various tissues of cloths, stuffs, *&c.*, incombustible; and the means he recommends appear superior to those which as yet have been proposed; that the combustibility of these substances is diminished by their having been immersed in solution of certain salts, as of alum, muriate of soda, *&c.*, has been long known. M. Gay Lussac considered that those salts should possess this property most eminently, which entered most readily into fusion, being enabled

by that means to cover perfectly the fibre of the substances, and preserve them from the contact of the air. Guided by this thought, he substituted phosphate of ammonia and borate of soda for alum, *&c.*, and he found that muslins thus treated could be placed in contact with ignited bodies without danger. They were carbonized, but would not inflame.

23. *On an Improvement in Gas Illumination.*

(In a Letter to the Editor.)

SIR.—As I apprehend any information which may tend to the removal of a common inconvenience, is within the province of your Journal, I beg leave to enclose you some remarks on the management of gas lights.

In the year 1806, I erected a small gas apparatus for the supply of my own premises; but, in consequence of the great daily attention it demanded, and the rapid destruction of my conducting-pipes, which were of tin, I was induced to abandon it at the end of a few months. The experience which I had gained during that time, of the superioriy of the gas lights above oil lamps, in cleanliness, manageability, and immense saving of time, (except in attention to the furnace) led me to become a tenant of the chartered Gas Light Company, soon after its establishment. One great inconvenience, however, attending these lights, I did not find it easy to remedy for a considerable time, I mean the quantity of aqueous vapour produced by the combustion of the gas; the condensation of this vapour on the walls of the apartments where the gas is burnt, or on the articles placed in them, except where a very free ventilation can be kept in constant operation, has prevented the use of the gas by ironmongers, and others who have polished metallic goods exposed to sale on their premises. I have often seen this vapour condensed, and hanging in drops on my ceiling, and by falling upon dust, it formed a most troublesome and perpetual source of annoyance. The first remedy I attempted, was to place a wide horizontal copper tube along the ceiling of my shop (forty feet in length), which terminated outside the house, in the open air; to the sides of this tube were fixed smaller horizontal ones at right-angles, and to these, vertical tubes of four feet each were attached, terminated by a bell-glass, which enclosed about an inch of the cylindrical glass of the burners. In a few months I found this plan very inefficient; the current through the tubes, in consequence of the numerous angles, was too slow to carry off more than a very small portion of the vapour, and the tubes became leaky by corrosion: I then ordered a leaden tube to be fixed by a curved joint, to the vertical tube immediately over the burner, and suffering it to pass horizontally about three feet, I directed it to be turned vertically downwards, with a curve at the angle, to the length of nine feet, through the floor of my shop into the cellar.

I was repeatedly assured by the maker of my apparatus, who is one of the fitters, that my plan *could not* answer, because the column of cold air in the long tube would be more than an equivalent for any rarefaction that could take place in the four-feet tube over the burner. I persevered, however, in my determination to make the experiment, and it succeeded beyond my expectation. I have found it necessary to have a separate tube to each burner, but I consider this expense well repaid by the advantage derived. The current through the tubes is so strong, as instantly to blow out a candle at the lower end, when the gas is burning; and so complete is the condensation, that two ounces of water per hour is produced from each light The water thus obtained is perfectly bright, and not unpleasant to the taste, nor does it exhibit any impurity except a slight portion of sulphuric acid. The fitter, who so obstinately opposed my plan, informs me, that he has put up similar apparatus since mine, with complete success.

I am, Sir, your most obedient Servant,

Wm. B. Hudson.

III. Natural History.

1. *Medical Prize Questions.*—The question proposed by the Cercle Médical of Paris, in 1819, not having been treated in a

satisfactory manner, is again proposed for the next year:—it is "to determine the influence of pathological anatomy on the progress of medicine in general, and especially on the diagnosis and treatment of internal diseases."

The society request the concurrents—1. to inquire whether or not pathological anatomy may, in its present state, give rise to applications and interpretations injurious to science.—2. To indicate the means which they believe to be the most proper to prevent these inconveniences: in a word, it engages them to take the sense of the word influence in its bad as well as in its good relations."

The prize will be a medal of 300 francs' value. The memoirs, written in French or Latin, are to be sent before July, 1821, to M. C. D. Chardel, Sécrétaire, *&c.*, Rue Cassette, No 26.

The following prize question, among others, has been proposed by the Haerlem Philosophical Society. The papers are to be sent in before January 1, 1822:—

1. "How far is it actually demonstrated, that fumigation with chlorine gas has prevented the propagation of contagious disease. What are the contagious diseases in which it ought to be tried, and what ought to be principally observed in such experiments? Is there any reason to expect more salutary effects from any other method hitherto employed, or proposed for this purpose?" It is requested that a succinct enumeration be given of the cases in which such fumigation has proved effectual in preventing various contagious diseases.

2. "How far does the physiology of the human body afford just grounds for supposing, or how far has experience satisfactorily proved, that oxygen gas is one of the most efficacious remedies for recovering persons who are drowned, suffocated, or in a syncope? And what are the most prompt and certain methods to be employed for this effect?"

3. "What is to be considered as justly proved with regard to the gastric juice of the human body, and its influence on the

digestion of food? Is its existence sufficiently proved by the experiments of Spallanzani and Senebier, or is it rendered doubtful by those of Montegre? What has been demonstrated in this respect by comparative anatomy, and particularly by opening the stomachs of animals which have been killed either fasting or shortly after taking food? And supposing the existence of gastric juice in the human body to be well proved, what ought to be avoided in order that its effect on the digestion may not be impeded?"

4. "How far are we acquainted, from the chemical experiments of Vauquelin, with the various species of cinchona; likewise from the experiments and observations of others. 1. What is the different nature and quantity of their constituent parts? 2. To what particular principle ought we to ascribe the febrifuge powers of cinchona? 3. What criteria can we deduce from it, so as to distinguish the best species, and the various barks used as substitutes? 4. Are any rules to be obtained for preserving the principle, in which consists its febrifuge power, entire in the various preparations of cinchona?"

5. "Although a general introduction of vaccination has almost every where put a stop to the epidemic small-pox, yet within these few years past that disease has re-appeared, both here and elsewhere; and as a species of variolous pustules have recently shewn themselves in those who have been vaccinated, it is inquired, 1. Of what description are these pustules? In what do they differ from the real small-pox? Is it the latter that is produced in these individuals who have been previously vaccinated? Does it arise from constitution, from indisposition, from the matter employed in vaccination, or from other circumstances, and what is the method of preventing it? 2. What can be safely asserted, with regard to the duration of the preservative virtue of vaccination? Would it prove of any service to re-vaccinate on the re-appearance of the disease? Are the methods employed by us for the encouragement of vaccination sufficient, and do they tend to cause the entire disappearance of the small-pox? In case they are not, what more efficacious ones could be adopted?"

6. "What is the cause owing to which oysters are occasionally so prejudicial to health? Is it in consequence of a small worm that is found in them? In this case, of what species is it, and whereabout is it most easily detected? Are oysters subject to it only at certain times of the year? Has the venom of oysters any analogy with that which, from time to time, renders muscles poisonous and unwholesome? What are the disorders occasioned by such oysters and muscles, and what are the most efficacious remedies either for averting the evil or for removing it?"

7. "To what is it owing that shrimps are sometimes pernicious? How are such shrimps to be distinguished? What kind of disorders do they occasion, and what are the remedies to which, in such cases, recourse ought to be had?"

2. *Meadow Saffron.*—Mr. Todd Thomson concludes, from comparative experiments made on the *colchicum autumnale*, taken up at different times, and prepared in different manners, that the month of July is the best period of taking up the plant, as the bulb has then attained its full growth and perfection, whilst the vegetation of the lateral progeny for the support of which the bulb is intended, has scarcely commenced: that the bulb, when taken up, should be cut as soon as possible into transverse slices, equal in thickness to a half-crown, which should be spread upon clean white paper, and dried without artificial heat in an airy situation, screened from the sunshine: and that the slices, when dried, should be nearly oval, but not notched or panduriform, friable, of a white or cream colour, somewhat granular on both surfaces, inodorous, bitter to the taste, and altogether free from sweetness, and should afford a fine cærulean blue colour, when rubbed with a few drops of vinegar, and the alcoholic solution of guaiacum.—*Med. Journal,* 1820, p. 282.

3. *On the Vitality of Plants.*—M. de Candolle, in speaking of those plants which can preserve their existence without receiving fresh nourishment, mentions the following remarkable instance:

"M. Christian Smith, who has since perished so unfortunately in the Congo expedition, gave me, in February, 1816, a great number of plants, dried by him at Teneriffe in July, 1815. Among these was a *semper-vivum,* which I preserved for eleven months in my herbal; but having, in January, 1817, perceived in it a small white point, which it had pushed out, I withdrew the plant, and placed it in the earth. It grew and expanded, and I thus obtained a new species of *semper-vivum,* since made known by Sims under the name of *semper-vivum ciliatum,* it having previously passed eighteen months as a dried plant, in the herbals of myself and Mr. Smith. It afterwards flowered several times, and produced many young plants. The length of time during which the *semper-vivum ciliatum* preserved vitality without nourishment, is rendered more remarkable, from the circumstance that the part of the stem preserved with it was very small, and could not lose matter nearly in the proportion of the strong spreading branches of other plants.—*Annales de Chimie,* xv. p. 82.

4. *Luminous Phænomena produced by a Flower.*—Mr. Johnson had, last July, a fine plant, the *Polyanthus Tuberosa,* about five feet in height, in blossom in a room, which, he observed, emitted its effluvium most strongly after sunset. One sultry evening after thunder (it is believed the 16th July, on which day the thermometer stood at 81° in the shade,) when the atmosphere was evidently highly charged with the electric fluid, Mr. Johnston was surprised at seeing small sparks, or scintillations, of a lurid flame-colour, darted, with apparently excessive rapidity and momentum from two or three of the expanded flowers, which were beginning to fade, and at the same time the odour was so powerful as to be palling and unpleasant. He could not perceive any difference in the strength of the odour at different intervals, but during the whole evening its intensity seemed to be equable. He has subsequently noticed that the smell from the flower is most diffused in the light, but he has not again observed the singular electric phænomenon, though he has nightly and attentively looked for it. During the time of the appearance of the flashes, or sparks, he was anxious to

know whether their emission was attended by a crackling or snapping noise, as is the case when the electric spark is elicited from a charged jar; but, though he was most attentive, he was not conscious of hearing the least noise.—*Edin. Journ.* iii. p. 415.

5. *The Potato.*—This plant, the *solanum tuberosum* of botanists, grows wild in the environs of Lima, in Peru, and fourteen leagues from Lima, on the coast, and has been found wild in the kingdom of Chili. It is cultivated by the Indians in Peru and Chili, who call it papas. It grows spontaneously in the forests near Santa Fé de Bogotá, and among the rocks on Monté Video. The wild plants, however, produce only very small roots of a bitter taste. The native country of this plant is therefore at length ascertained.—*N. Monthly Mag.* 1820, p. 678.

6. *Geology of the Himáláyá Mountains.*—At a late meeting of the Geological Society, a paper on the valley of the Sutlej river in the Himáláyá Mountains, by Henry Thomas Colebrooke, Esq., Vice-president, G.S., was read.

The banks of the Sutlej, in the lower valley, at the elevation of 2,000 feet above the level of the sea, are composed of lime-stone, which is apparently primitive. The general inclination of the strata is stated to be 10 or 15°, and the direction much diversified. At Jauré, on the northern bank, hot springs issue within two or three feet from the river. A thermometer plunged into one of them rose to 130½° of Fahrenheit, while the temperature of the river was 61°. The water has a strong sulphurous smell, and incrusts the pebbles among which it rises with a yellow substance. Lime-stone seems the rock in the hills which bound the adjacent valleys. Among the specimens is a stalactite from the roof of a cave near the top of the Carol mountains, and about 6,500 feet above the level of the sea.

In crossing the Himáláyá Mountains at the Bruang Pass, which is the route of communication between the middle valley of the Sutlej and the valley of the Paber, and of which the extreme altitude is 15,000 feet, mica slate, gneiss, and granite, (some of the specimens containing garnets, others tourmalin) were found; and veins of quartz and mica, and of quartz and hornblende, were observed in the specimens which have been transmitted.

The mean height of the Sutlej, near to its confluence with the Bespa, is 6,300 feet. The rocks which here form its banks, are inclined at an angle of 25°—30°, and dip eastward; they consist of granite, gneiss, quartz-rock, granular quartz and mica, and granite with hornblende. Between this spot and Rispé, from 6,500 to 9,800 feet above the level of the sea, the rocks are chiefly formed of a whitish crumbling granite. The Cailasor Raldang mountains on the south, an assemblage of pointed peaks covered with snow, and more than 20,000 feet in height, are to all appearance covered with the same kind of rock.

Overhanging the town of Marang is a mountain of clay slate; upon it, at an elevation of 12,000 feet, heath, juniper, and gooseberry-bushes were growing. In advancing to the Tungrang Pass, which is 13,740 feet above the sea, rocks formed chiefly of compact quartz with chlorite were observed. The pass itself exhibited clay slate, with pyrites and globular mica. A few miles further on, granite, gneiss, mica slate, cyanite, quartz and mica, actinolite and quartz with garnet, pyrites in quartz, a bluish grey lime-stone with white veins, and calcareous tufa were found. Here the strata, according to Lieutenant Gerard's observation, run N.W. and S.E., and dip to the N.E. at an angle of 40° or 45°.

In the neighbourhood of Namptú-sangó the bed of the Sutlej is 8,220 feet above the sea, and consists of only two sorts of rock, *viz.*, mica slate and granular quartz, with imperfectly crystallized hornblende. At its confluence with the Li river, the banks are composed of granite. On ascending the latter stream the banks were found to exhibit specimens of slate, potters' clay, marl or loam, sand, and stalactitical carbonate of lime. Higher up the same river, and in the vicinity of Chango, where the bed is not less than 9,900 feet above the sea, primitive lime-stone (blue and likewise grey and white,) with disseminated pyrites was found; also mica slate with fragments of veins; in one instance, white quartz with mica, hornblende, and garnet; in another actinolite with quartz, mica, and garnet.

Between Namgia and Shipké, where the survey eastward terminated, and where the bed of the river is 9,000 feet above the level of the sea, the rocks are composed of granite with and without tourmalin and garnets, gneiss, mica slate, compact quartz, cyanite with quartz and mica, and compact feldspar with hornblende.

North of the Pass of Shipké-ghâté, is situated the Tarhigang Mountains, which Lieutenant Gerard ascended to the prodigious height of 19,411 feet above the level of the sea, and within two miles of the top, which is estimated at 22,000 feet. The rocks here lie in immense detached masses heaped upon one another.

One specimen of whitish primitive lime-stone, and another of granite with tourmalin and garnet, which were found at this station, have been preserved. The Ról or Shátúl pass over the Himáláyá Mountains, by which the surveyors returned from their arduous journey, is nearly 15,000 feet high. The rocks at the summit of this pass consist of gneiss, and the pass on each side, rising to an elevation of nearly 3,000 feet more, appear to be formed of the same materials. On descending on the southern side from the Ról Pass, at the height of 12,000 feet above the sea, the rocks are found to be generally composed of gneiss; and on the northern side, the prevalent rock was ascertained to be granular quartz.

Seeds of a species of campanula were gathered at the elevation of 16,800 feet above the sea, on a spot where the thermometer at noon in the middle of October, was at 27° Fahr. Shrubs were found in a vegetating state, at a still greater altitude.

7. *On the Tape-worm in the Pointer and Spaniel, by* Captain Bagnold.—Sir, on an estate where a great quantity of rabbits are annually destroyed in the month of November, I have observed that several dogs who were previously in good health

and condition, soon became weak, listless, and excessively emaciated, frequently passing large portions of the tape-worm: this induced me to examine the intestines of several hares and rabbits, and, with very few exceptions, I found each to contain a perfect tape-worm, from three to four feet in length. I then caused two of the dogs whose cases appeared the worst, to be separated from the others, feeding them on potatoes, *&c.*; and in eight or ten days, after voiding several feet of the worm, they were perfectly restored to their former strength and appearance. The vermicular disease, hitherto so formidable to the spaniel and pointer, may therefore in a great measure be fairly attributed to the custom of giving them the intestines of their game, under the technical appellation of "*the paunch.*" The facts above stated, in explaining the cause of the disease, at the same time suggests the remedy.

I am, Sir, your obedient Servant,

T. M. Bagnold.

7, *High Row, Knightsbridge,*
December 20, 1820.

III. General Literature.

1. *Classical Manuscripts.*—The Abbé Amadeus Peyron, Professor of oriental languages in the university of Turin, has discovered some fragments of Cicero, in a MS., from the monastery of St. Colomban di Bobbio, a town on the Trebia, in the King of Sardinia's dominions. This MS. contains important new readings of orations already known, and confirms the identity of several texts which have been tortured by indiscreet critics. It contains, besides fragments of the orations, *pro Scauro, pro M. Tullio in Clodium Orationis,* which are unfortunately lost. Some of these fragments have been already published by M. Mai, after a MS. of the same library at Colomban, preserved in the Ambrosian library at Milan, so that at the first sight these two MSS. would appear to have originally made but one. But the difference of the writing, that of the parchment, the circumstance that one of these MSS. is written in three columns, and the other in two, as well as

that several deficiencies in the Ambrosian MS. are supplied by that of Turin, leave no room to doubt of their being copies essentially different.

2. *The Black Prince.*—Extract of a letter from Bourdeaux.—"A few days ago were discovered among the ruins of the castle of Castlemar in Medoc, several silver coins, or *demi-gros* of Aquitaine, which exhibit on one side the effigy of the prince of Wales, in a ducal attire, armed with a sword, standing under a Gothic canopy; and on the reverse two fleur-de-lys and two leopards, symmetrically separated by a full cross, marked with six points, indicative of the value of the coin. Round the portrait of the prince is the legend, Ed. Ps. gns. Reg. Agl. B. (Eduardus primogenitus regis Angliæ B.); and on the reverse, Acit. Prncps. (Aquitaniæ Princeps)."

3. *Prize Question.*—The programme of the prize of eloquence, which is to be distributed by the French Academy in August, 1821, is as follows: "To determine in what consists the poetic genius, and how it is to be discriminated, independent of the diversity of languages and the forms of versification, in all the different kinds, from the Apopee to the Apologue." The works of the candidates to be sent before the 16th of May.

The subject of the prize of eloquence for 1822 is, "An Elogium on Le Sage."

4. *Cleopatra's Needle.*—This well-known monument of antiquity is expected to arrive shortly from Alexandria, a present from the Pasha of Egypt to His Majesty George IV. It is rumoured that it will be set up in Waterloo-place, opposite to Carlton-house. The weight of the mass is about 200 tons, the diameter at the pedestal seven feet. It is understood that we are indebted to the influence of S. Briggs, Esq., British-resident at Grand Cairo, with the Pasha of Egypt, for this magnificent monument.

5. *Island rent asunder.*

Japava, 27th Jan. 1820.

"During the late stormy weather, since the 3d instant, an island, which we find by the map of Java is called Fisherman's-island, has been rent asunder. It is known to the natives under the name of Pulo Pentangan. As soon as the weather will permit a further investigation will be held respecting this extraordinary event."—*Bat. Courant, Feb.* 1.—*Phil. Mag.*, 56, p. 396.

6. M. Soret requests us to mention that the apparatus, described at p. 168, are not of his invention, but belong to M. Biot. We have described them only as being "quoted" by M. Soret.

7. Mr. Salt.—*Information from Egypt.*—The earnest zeal with which Mr. Salt has investigated the antiquities of Egypt, the liberality which has induced him to do this at his own expense, and the very valuable fruits which have already resulted from his efforts, make every information respecting him highly interesting and important. We understand that he has been very ill for nearly ten months, but is now slowly recovering; that Nathaniel Pierce, after fourteen years' residence in Abyssinia, is returning to England, with all his papers; and that he is intrusted by Mr. Salt with several papyri, two fine vases, and many inscriptions, as a present for the British Museum. The following are extracts from one of Mr. Salt's letters:

"We have not lately had any great discoveries of antiquities, except of some Greek mummy-cases. They have short Greek inscriptions, mentioning the birth, death, and age of the persons; they are covered externally and internally with hieroglyphics; some have the zodiac painted inside, which is much like that on the ceiling of Dendera, and probably of the same antiquity. They have all got papyri inside, in a character much like the inscription on the Rosetta stone. One of these is of a commandment of Hahes, and another of his son."

"The Pacha having finished his Hidjas expedition, is now on the point of sending another, of four or five thousand men, into the interior of Africa. One division is to move on Darfoor, by way of the Desert which Brown passed over. The second is to keep along the hill, and to advance as far as Sennaar. This force is commanded by his son Ismael Pacha. They carry cannon, and provisions for three months; they have already been joined by a great part of the Mamelukes at Dongola, who have come in and made their submission; and that part of the country and its adjoining districts have been put under the command of Abdeen Cachief, who is to pass from the Theban Oasis with 4,000 Arabs, and to take up his residence at Dongola, his orders being to keep open the communications with the two advanced armies. There are several European engineers employed on this service, from whom we may expect to receive accurate accounts of what may occur.

8. *On the Root resembling the Potato, lately imported from South America.*

(To the Editor of the Journal of Science and the Arts.)

It is remarkable that the root resembling the potato, lately brought to England for cultivation from Santa-Fe de Bogota, is an African root, which grows in the territory of Lower Suse, near the Southern Atlas, inhabited by the Arabs of Woled Abbusebah, and called by them by the same name which it bears in South America, *viz.*, *Arak Atshan* or *Atshu*, unquestionably true Arabic words, signifying the absorbing, or thirsty root: *Aruk*, or root; *Atshun*, thirsty.

How has this root found its way to South America, and there retained its original Arabic name? Were the ancient Arabs possessed of more nautical knowledge than we are aware of? Did they, at some remote period, and many centuries before the discovery of America by Columbus, cross the Western Ocean, transporting their plants to America? Or are we to confirm the opinion of a submerged continent, which, before its submersion, afforded a communication between Africa and South America, countries now divided from each other by an ocean of 30° across from shore to shore?

James Grey Jackson.

ART. XXIII. METEOROLOGICAL DIARY for the Months of September, October, and November, 1820, kept at EARL SPENCER's Seat at Althorp, in Northamptonshire. The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

For September, 1820.

		Thermometer		Barometer		Wind	
		Low	High	Morn.	Eve.	Morn.	Eve.
Friday	1	37	63	30,00	29,98	SE	NE
Saturday	2	39	66	29,98	29,98	NE	ENE
Sunday	3	40	67	30,05	30,05	N	NE
Monday	4	40,5	69,5	30,08	30,03	E	E
Tuesday	5	37,5	68	30,03	29,94	E	E
Wednesday	6	35,5	67	29,97	29,98	E	E
Thursday	7	38	68,	29,99	29,99	SSE	SSE
Friday	8	45	69	30,08	30,20	NW	NW
Saturday	9	38,5	73	30,28	30,19	NW	SSW
Sunday	10	45	72,5	30,11	30,10	W	W
Monday	11	46	73	30,10	30,07	W	SE
Tuesday	12	46,5	72,	30,06	30,02	SE	ESE
Wednesday	13	47,5	71,	30,02	29,93	SE	SSE
Thursday	14	47	74	29,80	29,64	SE	S
Friday	15	56	65 5	29,50	29,06	WSW	W
Saturday	16	46	64	29,74	29,26	W	W
Sunday	17	51,5	61	29,80	29,72	W	W
Monday	18	49	56	29,50	29,52	N	W
Tuesday	19	39	55,5	29,68	29,85	W	W
Wednesday	20	33	54	29.80	29,32	SSW	S
Thursday	21	39	60	29,25	29,25	W	NW
Friday	22	41	59	29,42	29,70	W	W
Saturday	23	47	68	29,78	29,76	SW	SW
Sunday	24	57	67	29,50	29,54	SW	WSW
Monday	25	44	61	29,46	29,46	W	SW
Tuesday	26	40	53	29,67	29,90	WbS	WNW
Wednesday	27	34	56	29,90	29,88	W	W
Thursday	28	41	61	29,88	29,80	W	WbN
Friday	29	44	57	28,89	30,00	NW	NW
Saturday	30	35	60,5	30,00	29,80	NW	S

For October, 1820.

		Thermometer		Barometer		Wind	
		Low	High	Morn.	Eve.	Morn.	Eve.
Sunday	1	47	59	29,93	30,10	W	W
Monday	2	42,5	60	30,17	30,28	W	W
Tuesday	3	44	59	30,37	30,40	NW	NNE
Wednesday	4	31	56	30,40	30,34	NE	NE
Thursday	5	45	56	30,27	30,17	NE	E
Friday	6	48	58	30,12	30,08	ENE	ENE
Saturday	7	46,5	58 5	30,08	30,00	NE	ENE
Sunday	8	41	56	30,03	30,03	E	NE
Monday	9	44	50	30,10	30,08	NE	NE
Tuesday	10	43	51	30,00	29,91	N	NE
Wednesday	11	42	51,5	29,85	29,85	NNE	NE
Thursday	12	40	53	29,89	29.89	N	NW
Friday	13	33	52	29,85	29,80	W	W
Saturday	14	42,5	56	29,60	29,15	SW	ESE
Sunday	15	43	62	28,80	28,85	SE	SW
Monday	16	42	54	28,93	28,87	SW	SW
Tuesday	17	45	55	28,70	28,70	WbS	WSW
Wednesday	18	38,5	54	28,78	28,90	W	SW
Thursday	19	40	51	29,10	28,98	W	SW
Friday	20	40	50	28,70	28,80	SW	W
Saturday	21	36	53	29,13	29,37	W	W
Sunday	22	38	52,5	28,71	28,60	SbW	W
Monday	23	42	52	28,80	28,94	W	W
Tuesday	24	39	52,5	28,70	28,61	W	SbW
Wednesday	25	41	50	28,79	29,10	W	WbS
Thursday	26	37,5	48,5	29,12	28,74	WbS	SW
Friday	27	40	50,5	28,77	29,09	WbS	W
Saturday	28	39	51	29,31	29.50	W	WbN
Sunday	29	38	46	29,30	29,03	SE	SE
Monday	30	35	50	29,30	29,44	SW	SW
Tuesday	31	33	47	29,31	29,17	SE	ESE

For November, 1820.

		Thermometer		Barometer		Wind	
		Low	High	Morn.	Eve.	Morn.	Eve.
Wednesday	1	42	46	29,20	29,32	WbS	WbS
Thursday	2	34	48	29,50	29,60	W	W
Friday	3	29	44	29,67	29,69	W	WNW
Saturday	4	25	43	29,70	29,79	WNW	NE
Sunday	5	29	47	29,71	29,48	SSE	S
Monday	6	30	52	29,50	29,51	W	W
Tuesday	7	46	55	29,52	29,60	SW	SE
Wednesday	8	49	54	29,64	29,71	E	E
Thursday	9	42	47	29,79	29,82	E	E
Friday	10	41	48	29,82	29,88	ENE	N
Saturday	11	39	46	29,92	30,09	N	N
Sunday	12	33	41	29,89	29,65	W	NW
Monday	13	36	40	29,50	29,50	NW	NE
Tuesday	14	34	38	29,68	29,68	NE	NE
Wednesday	15	33,	41,5	29,73	29,81	N	N
Thursday	16	34	40	29,80	29,70	N	NbW
Friday	17	29	42	29,50	29,42	SE	W
Saturday	18	29	39,5	29.58	29,72	W	WbS
Sunday	19	29 5	44	29,72	29,75	W	W
Monday	20	35	48	29,73	29,70	S	SE
Tuesday	21	48	51	29,65	29,60	SSW	S
Wednesday	22	46	48	29,58	29,50	SSE	S
Thursday	23	38	47	29,40	29,40	SbE	WSW
Friday	24	31	43	29,48	29,50	W	E
Saturday	25	42	50	29,41	29,46	E	E
Sunday	26	43	49	29,60	29,70	SE	SE
Monday	27	39	47	29,73	29,80	E	E
Tuesday	28	37	39,5	29,94	30,00	EbN	EbN
Wednesday	29	38	42	30,12	30,12	NE	E
Thursday	30	39	41,5	30,07	30,03	SW	W

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INDEX.

Erratum.—In the Table of Contents of Number XIX. Article V. for *Agriculture* read *Horticulture*.

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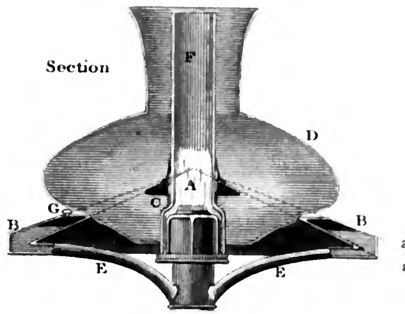
Fig 3.

Fig 2.



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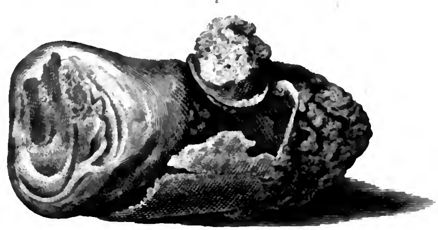

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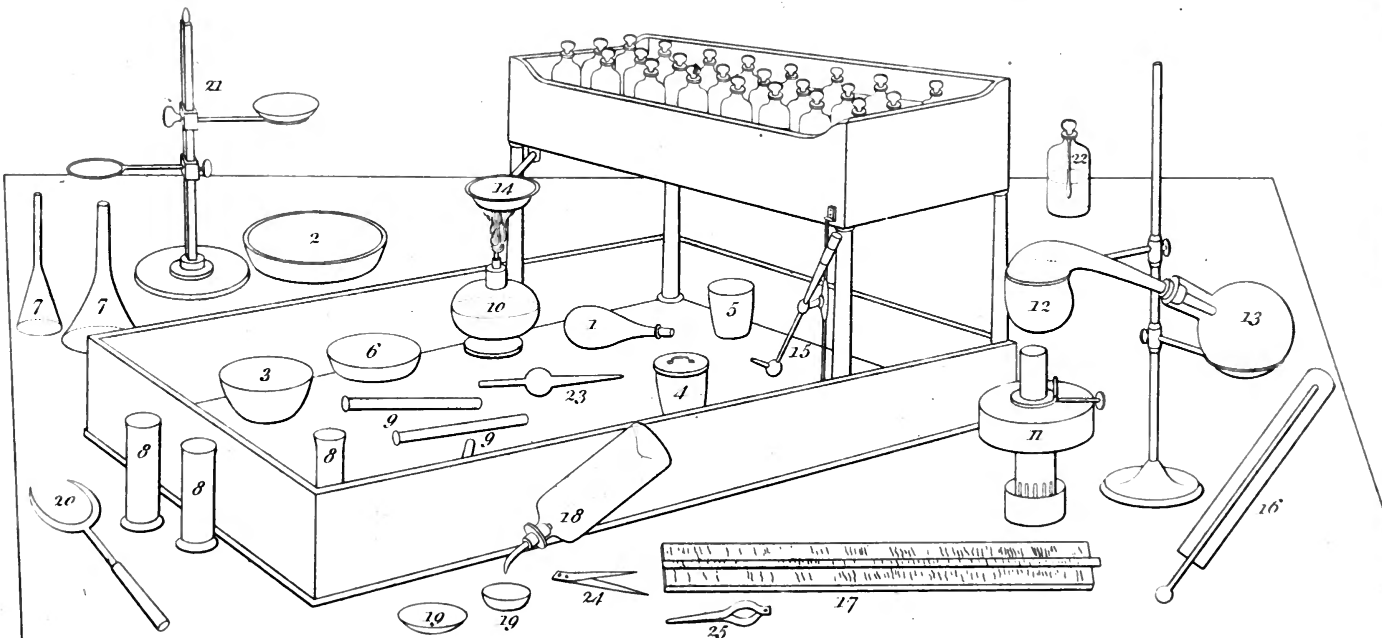

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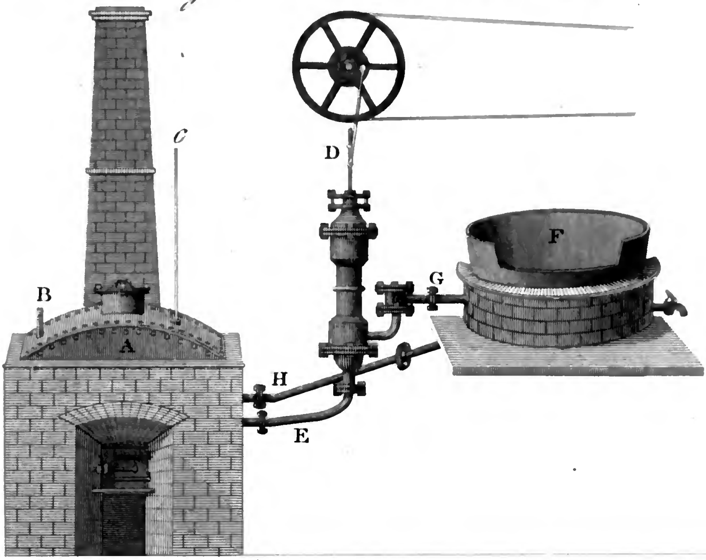
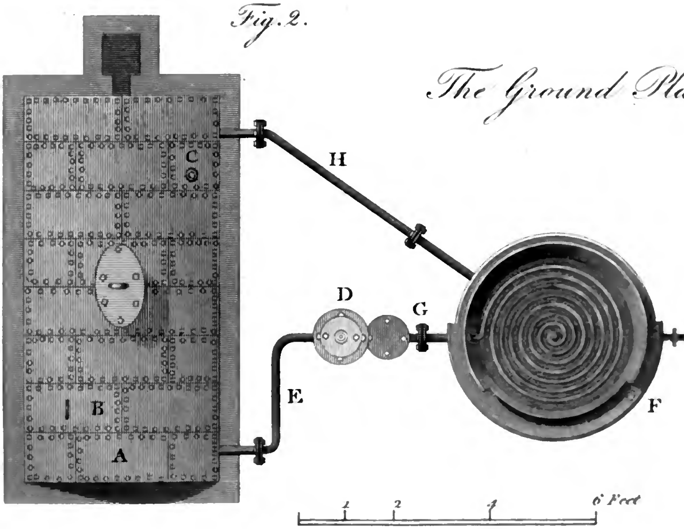


Fig. 2.

The Ground Plan.



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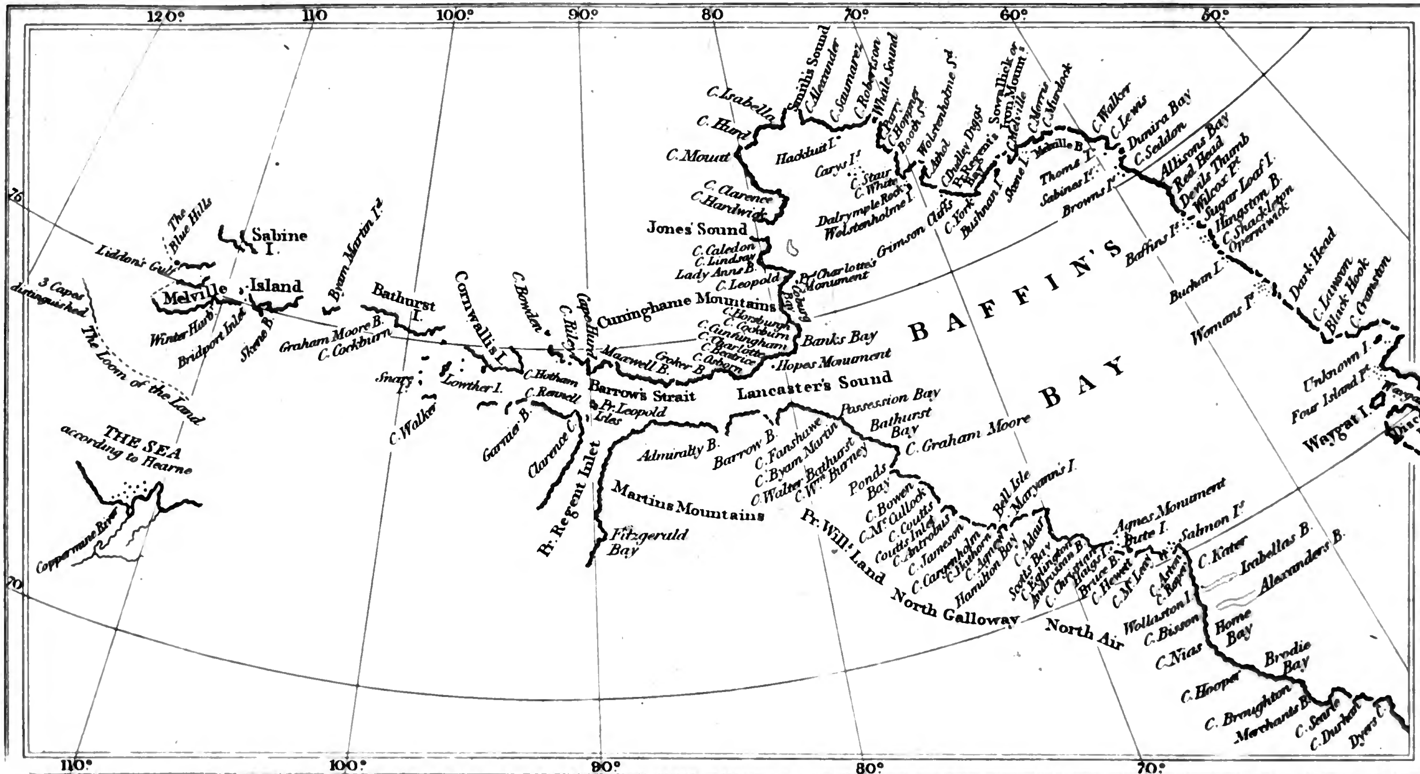
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