THE

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OF

SCIENCE,

LITERATURE, AND THE ARTS.

VOLUME XII.

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OF

THE QUARTERLY JOURNAL,

No. XXIV.

I. MECHANICAL SCIENCE.

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TO CORRESPONDENTS.

M. R. I. is informed that his suggestion is premature, as he will see in our next Number.

The project of a "*Constant Reader*" exceeds our means.

Mr. Langham's communication came too late for insertion in this Number, and we apprehend that its subject is of too temporary a nature to stand over till April; if, therefore, he will favour us with his address the MS. shall be returned.

The OLEAD has reached us, but we have no *niche* for poetry.

The information desired by "*An Old F.R.S.*" may be obtained at Somerset House, but we cannot satisfy his misgivings respecting the ROYAL SOCIETY *of Literature;* upon that subject we coincide in opinion with him, and shall not hesitate to avail ourselves of his hints.

The letter from a Member of the London Institution, came too late to be answered: his project, we believe has already been entertained without success.

The insertion of B.'s paper is a sufficient reply to the contents of its envelope: there were three errors in his cipher.

Mr. Macgrigor's pamphlet reached us too late.

Mr. J. T. Todd will see that we have availed ourselves of his communications: we hope again to hear from him in the course of his journey.

Under the signature of POMPHOLIX we recognise an old correspondent; but, *ex nihilo nihil fit.*

TO CORRESPONDENTS.

The change suggested by Typographus has long been in contemplation. As to the *gross errors* he talks of, they amount to the printing of *a* instead of *x*, and of + instead of ×. Upon the whole, we doubt whether any work of similar miscellaneous contents is more accurately printed than the late Numbers of this Journal.

We are much obliged by the ingenious suggestions of our Correspondent at Ashburton. We request that he will again attentively peruse the paper in our Journal to which he adverts, and the more extended work there quoted: we think he will then find the principal part of his plan anticipated.

We have in vain endeavoured to decipher the "Observations on Defensive Fortification."

We cannot reprint extracts from other Journals of an old date, to the amount required in the papers with which we have been favoured from Wexford; but shall be happy to receive any new information, or practical details upon the subject of the Rev. Mr. Rice's letter.

Crito Edinensis has our thanks, and shall be attended to, but we cannot submit to the *motto* which he proposes.

Τι δει μαθειν, χρη γαρ παθειν, is not an apt quotation for the case in question. Does Σ pretend to say there is no *science* in Medicine? or what is his object?

S. evinces much spirit of observation. The insect to which he alludes, is probably the *Staphylinus riparius* of Linnæus. There are at least 300 species of the same Linnæan genus, that fold up their wings in a similar manner under short *Elytra*, or wing cases. We observe the same thing in the *Forficula*, or earwig, of which S. will find an interesting account in Adams *on the Microscope.* Our correspondent should persevere in his observations on the Œconomy of Insects; he has before him an inexhaustible field of useful amusement.

TO CORRESPONDENTS.

Dr. Walker's Natural History of the neighbourhood of Huddersfield reached us too late for insertion.

Dr. Fitton's Paper on the History of English Geology, has been withdrawn by the Author, with a view to its appearance in a separate publication.

We regret that want of room has obliged us to exclude Mr. Slight's Report relative to the Portsmouth Philosophical Society.

ROYAL INSTITUTION,

21, ALBEMARLE-STREET.

The Members and Subscribers are informed, that the Lectures will commence on Saturday the 2d of February at two o'Clock, and that the following Arrangements have been made for the Season.

On *Experimental Chemistry*, and on the *Principles* of some of the *Chemical Arts.* By WILLIAM THOMAS BRANDE, Esq., Sec. R.S. Lond. and F.R.S. Edin., Professor of Chemistry to the Royal Institution.—To commence on Saturday the 2d of February, at Two o'Clock, and to be regularly continued on each succeeding Saturday at the same hour till further notice.

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 to the Royal Institution.—To commence on Thursday the 7th of February, at Two o'Clock, and to be regularly continued on each succeeding Thursday, at the same hour, till further notice.

On *Comparative Physiology*, comprising Philosophical Views of the Functions of Animal Life, as connected with Organization. By P. M. ROGET, M.D., F.R.S.—To commence on Tuesday the 12th of February, at Two o'Clock, and to be regularly continued on each succeeding Tuesday at the same hour, till further notice.

On *Botany.* By Sir JAMES EDWARD SMITH, M.D., F.R.S., Pres. Linn. Soc., Professor of Botany to the Royal Institution.—To commence after Easter.

On the *Engraved Hieroglyphics* of *Canaan, Egypt,* and *Chaldea.* By JOHN LANDSEER, Esq., F.A.S., A.R.A., Engraver to the King. To commence after Easter.

On *Music.* By W. CROTCH, Mus. D. Professor of Music in the University of Oxford.—To commence after Easter.

The Spring Course of the Lectures and Demonstrations on *Chemistry* delivered by Mr. BRANDE, in the Laboratory of this Institution, will commence on Tuesday the 12th of February, 1822, at Nine in the morning precisely, and will be continued every Tuesday, Thursday, and Saturday at the same hour, until the end of May.

THE

QUARTERLY JOURNAL,

October, 1821.

ART. I.—*On an ancient Monument of Sculpture in Stone, representing the Theban Sphinx; which was recently discovered at Colchester, in Essex. By* E. W. A. HAY, Esq., A.B., F.A.S.*

THERE is not perhaps any single object of ancient art, that has been ever found in this country, which offers so much interest, not merely to the antiquary, but to the artist and to the historian, as this very extraordinary and beautiful sculpture lately discovered.

The engravings (Fig. 1, 2, 3, and 4†,) are after drawings made, as nearly as the perspective would admit, according to a scale of one quarter the size of the original. They represent the four principal views of the Sphinx, which the sculptor has exhibited as having already slain some victim of her wiles; her bloodthirsty passion would seem to be already satiated; she sits as it were satisfied with her prowess, and in perfect serenity, over the mangled remains of her unsuccessful opponent.

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* In a letter which the writer of this article published lately at Colchester, upon the same subject, he announced his intention of transmitting the four drawings, here engraved, to the Society of Antiquaries of London, for the purpose of insertion in their Archæological publication. The drawings have been exhibited to the society, and excited considerable interest with that learned body; but, as the forthcoming volume of the Archæologia was already completed, and in the press, and there was not any probability of part of another being published within a twelvemonth from the present time, it was thought desirable to convey to the public, some knowledge of this curious discovery, through the circulation of this Quarterly Journal.

† The figures 1 and 2 present the right and left views of the monument: the figures 3 and 4, shewing the front and rear views, will be given in the following Number of this *Journal.*

This singular figure, which, from its beauty, might naturally be expected to be of the finest marble, has been sculptured in freestone. The material appears, from its good quality, to have been brought from the Isle of Portland; since it is not known that any stone of the same kind has been found in a native state in the neighbourhood of Colchester. The Romans brought, no doubt, from Portland, much of the materials for their finer works at that place where they planted their first colony* in Britain, and which long remained one of their principal military stations.

This Sphinx was found in the midst of a great number of fragments of another species of stone, equally unknown at Colchester as a native product, and vulgarly called *Swanage,* from the place where it is dug in the Isle of Purbeck, which is in the immediate vicinity of Portland.

The general dimensions of our antique are as follows:

Length of the base, ...	25½ Inches.
Medium breadth of the base, ...	10
Height from base to top of Sphinx's head, .	25
The face of the Sphinx, measuring from under the chin to the crown of the head, . .	5

The stone was found at the depth of about two feet from the surface of the soil, in trenching the ground around the General Hospital lately erected at Colchester. It was discovered in an almost perfect state, there being only a few marks of injury, and those slight; excepting upon the left side of the work that lay uppermost. The fracture of the nose of the Sphinx, as well as a blow upon the man's forehead, were, with some other less material bruises, the almost unavoidable effects of the labour upon discovery.

But the circumstances of the spot where this relic has been found, as well as the great beauty and the peculiar character of the work, have stimulated inquiry, and merit some discussion.

Notwithstanding all that has been written upon the early history of Colchester, it may be useful, as a preliminary step to our consideration of the probable history of the monument in question, to refer those who retain any hesitation upon affixing

* Tacitus *Annal.*, L. 14, and Camden upon Essex.

the *Colonia Camulodunum* of the Romans at Colchester, to the Archæologia. Mr. Walford, adducing several authorities upon whose judgment and learning the most scrupulous may be willing to rely, has, in the 16th volume of that publication, concluded an interesting dissertation upon the situation of Camulodunum in these terms: "I hope the preceding observations, which from my own personal inspection I have found very accurate, will be the means of preventing any future controversies or difference in opinion upon the subject; and that all will agree with Bishop Stillingfleet, Dr. Stukeley, Dr. Mason, Mr. Morant, Mr. Gough, and the Rev. Mr. Leman, that Colchester, and not Malden, was the *Colonia Camulodunum* of Richard of Cirencester*."

Having premised thus much, I proceed to present such hypotheses upon the probable age, authors, and history of the monument under review, as may be fairly deduced from a consideration of its own character, of various remains of antiquity found near it, and of the historical records immediately applicable to the place of discovery.

The adjustment of the hair of the Sphinx, is the same as that of the younger empress Faustina; yet it resembles almost as closely the dress that we have frequent occasion to notice upon Roman monuments of an earlier time. But when we consider the evidence of the sculpture itself, whose chaste, yet cultivated character is as far distant from the rude operations of the earliest British workmen, as it is from the meretricious style of any middle time in this country; we need not hesitate to ascribe our Sphinx to the chisel of some eminent Roman sculptor, and of the best age.

The coarse nature of the material militates against the probability of this fine piece of work having been imported from Italy. We may therefore be allowed to conclude, that it was wrought in this island; for it is acknowledged that the Romans brought artists hither with them, and they may also have been invited over by the British Princes †.

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* See Pegge *on the Coins of Cunobelin*, and Ruding's *Annals of the Coinage*.

† See Pegge *on the Coins of Cunobelin*, p. 54, *&c.*

Of the Roman origin of this monument, we have irresistible proof in the great mass, found in and around the very spot where the Sphinx was discovered, of antique remains: such as bricks, tiles, pottery, fragments of bronze, and other relics, that are decidedly of Roman fabric; and which, as well as the Sphinx, lay concealed in a soil that apparently had not for centuries been disturbed beyond the depth of the plough-share. But my attention has been particularly attracted by the portion of a sepulchral inscription, to the memory of one or more legionary Roman soldiers: this was dug up a few days sooner than the Sphinx, and at the distance of no more than about twenty-five paces from it. The following cut is a faithful copy, although the engraver has formed the letters with less sharpness and care, and represented them as being more mutilated, than the state of the original will justify.

This fragment is moreover of itself extremely curious, as making a distinct mention of the *Third* Imperial Legion, (LEGionis III. AVgustæ) a portion of the Roman forces, of whose appearance in Britain I cannot find any trace by all the references I have been able to make. The stone has 10½ inches by 8 of surface, and is 1⅜ of an inch thick. Let it be noticed, that this fragment is of the stone brought from Swanage; and it is further an extraordinary relic, as, excepting one other fragment of a sepulchral inscription, which is of the same material, but cut with greater delicacy, and found at Colchester about twelve years ago*, no Roman remains of a similar kind are known to have been discovered either at Colchester, or even within the county of Essex. This, from the long residence of the Romans in that quarter of the island, is indeed almost unaccountable; but may be judged of by the void it has occasioned in Horsley's *Britannia Romana*†.

We have now to offer an unexpected testimony in favour, not only of the Roman origin of our stone Sphinx, but also of the peculiar reverence in which, perhaps, that very same beautiful figure of sacred legend, or at least the more common and less complicate mystic symbol, was held at Camulodunum; by the fortunate finding of a bronze Sphinx, of which the following engraving presents a view, the same in size as its original‡.

The bronze is perfect, excepting the loss of the wings, which, from the appearance of the back, have evidently been torn off. This, which I deem to have been one of the *lares* of an inhabitant, was dug up last summer within a few yards of the spot,

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* Now in my possession.

† Since this article went to the press, John Disney, Esq., of the Hyde, near Ingatestone in Essex, has been so obliging as to make known to me a small sepulchral monument in his possession, which was found at Colchester in 1713, and bears the following inscription. It is a tablet of marble, eight inches long, by four and three-quarters high, having a border of foliage and other work in an inferior style:

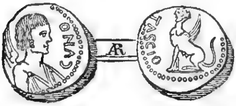
CONSIDIA·VENERIA·FILIA·V·A·III·D·XXX·CONSIDIA·NATALIS·MATER·V·A·XXXV.

‡ In the possession of a lady at Colchester.

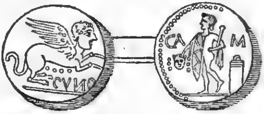
where our stone Sphinx was discovered. The little image exhibits, in its present state, no further compound than of the lion and the virgin; and, from the arrangement of the hair resembling that of Julia Mæsa, or of her daughter Julia Sœmias (mother of the Emperor Heliogabalus), as well as from its inferiority of style and execution, it is doubtless of a later time than the large Sphinx; not appearing to claim any earlier date than about the beginning of the third century of our era.

But, although I do not feel myself at liberty to refer the age of our stone monument to an earlier time than that of the first Emperor Claudius, as will be hereafter shewn; yet I consider that we are fully authorized in tracing the respect for the Sphinx symbol, that would appear to have been entertained by the inhabitants of *Colonia Camulodunum*, as well by the indigenous Briton, as by the Roman colonist, to a higher origin, even to the time of Augustus. We learn from Pliny, from Suetonius, and from Dion Cassius, that the image of a sphinx was, during a certain period of the life of Augustus, employed by that emperor as his seal. We find also the figure of the sphinx upon the Roman coins of * his time: but what comes more directly

No. 1.



No. 2.



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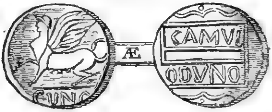
No. 3.



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* Upon the passage referred to in Suetonius, Lævinus Torrentius has the following comment: "(De Sphinge) et eodem fortassis referendum quod T. Carisii, qui sub Augusto III. vir monetalis fuit, numismata sphingem præferunt." See also Morelli and Illustrations by Agostini in *Reg. Imp. Rom. Numism.* No. 17. tab. 12.

† No. 1. "Obv. CVNO. The bust of a winged figure, possibly a Victory. Rev. TASCIO. A Sphinx, from a coin of Augustus.—No. 2. Obv. CVNO. A Sphinx. Rev. CAM. Qu. A British warrior with the head of an enemy in his right hand?" See British Coins, plate 4 and 5, Nos. 8 and 25 in Ruding's *Annals.*—No. 3. Obv. CVNO. A Sphinx. Rev. CAMVLODVNO.

in aid of our argument is, that we meet with the same figure repeatedly upon the coins of Cunobelin.

This illustrious British prince * had, according to Dion Cassius, his royal seat at Camulodunum; and more of his coins are found at and near to Colchester, than in any other part of the island. Of the three here given, Nos. 1 and 2 have been already published; No. 3 is now, I believe, for the first time, presented to the public: the original is in the possession of Mr. William Keymer, of Colchester, where it was found about twenty-five years ago. I possess also a small brass coin of Cunobelin, found at the same place in 1819: it is in a bad state of preservation, but bears upon both sides a very striking resemblance to No. 3; yet mine appears to have had the wheel under the right paw of the sphinx, as we find exhibited in Gem, No. 206 of Agostini, in No. 111 of Tassie, and elsewhere. This addition No. 3, from the action of its sphinx, may have also had, but it seems to have suffered corrosion upon that part.

Cunobelin is said to have cherished a friendship for the Romans, to have attended Augustus in his wars, and so well to have behaved himself, that he grew into particular favour with that emperor, and accompanied him to Rome, where he was saluted by the title of Friend to the Republic: moreover, that during his residence there, Tenuant (his father, and immediate predecessor on the Trinobantine throne,) paid no tribute †.

The attachment for the Roman emperor, at least the politic respect which our great British chief ‡ might have entertained, or merely put forth, in regard to his powerful protector, may be in some measure estimated by the appearance of the head of Augustus impressed upon the coins of Cunobelin §; who, as Mr. Pegge writes, "omitted no opportunity of making his court to that emperor." We find also some notices in Suetonius, of the continued friendship that appears to have subsisted between

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* Dion L. 60.

† See *History of Colchester* by Morant; who in this matter, does in common with other respectable writers, give credit to Geoffrey of Monmouth.

‡ Or as Baxter styles him, the *Pendragon. Gloss. Vo. Cunobelinus.*

§ Pegge on his *Fifth Class*, No. 1. Also Ruding's *Annals*, v. 1, p. 268, an v. 5, pp. 13—17.

the Iceni and Trinobantes and the Romans; even from the time of Julius Cæsar to that of the first Claudius.

Morant thus epitomizes from Tacitus, from Dion Cassius, and others, the victorious invasion by the latter emperor, and the events which immediately followed: "Claudius took Camulodunum, where he placed a colony of veterans. In honour of this victory he was divers times saluted *Imperator*, contrary to the Roman custom, which permitted it but once in one expedition. After this he ordered the Britons to be disarmed; but to those that yielded, he remitted the confiscation of their goods, which so endeared him to them, *that they erected a temple and an altar to him at Camulodunum,* and honoured him as a god."

This is the only Roman temple of which we have any account as having been erected at Colchester; and that building I apprehend to have stood upon the very spot where the stone Sphinx was discovered. The situation is peculiarly striking, as it accords better than any other that can be chosen in or around the town, with the probable position of that sacred edifice. This will readily be acceded, when its elevated situation be taken into view, with its neighbourhood to the grand military way, and the presentation under such an aspect of its hallowed fane to all those in intercourse between the great camp at Lexden * and the capital of the Trinobantes. Since some may be unwilling to give credit to my position as taken in reference to the Roman way, objecting that such roads were not constructed so early as the reign of Claudius; I beg to observe, that, although these roads may not have been extended generally throughout Britain so soon as the first century of our æra, yet there must, without doubt, have been, from the first establishment of the Roman conquest, a grand public street leading from so principal a camp as that of Lexden (where such extensive vestiges still remain) to the great Roman colony established at the Trinobantine metropolis of Camulodunum. To others again who may remark, that the spot where I would fix the temple, is without the ancient walls, I beg to recall the words of Tacitus †, by

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* Baxter, *Vo. Camulodunum.* † *Annals*, L. 14, ch. 31.

whom it is shewn, that no walls existed around the town at the time of this temple. Indeed, had there been such a circuit of defence, we should bear in mind, that it was by no means uncommon with the Romans to construct temples without their towns; and they seem, with all other nations of antiquity, to have preferred high places for such holy purpose.

My conjecture is singularly supported by the very great mass of building materials, that have been thrown up during the late foundation of the General Hospital. It must have been evident to all those who witnessed, as I did, the recent disturbance of the soil upon the identical spot under consideration, that some very large building had stood on the same ground; but, at an early period, of which no local tradition now remains.

I have already pointed at the Roman character, of the antiquities discovered upon the spot: to which I might add the coins of that people, which are found there from the time of Julius Cæsar to that of the Constantines.

Considering the pains that have been taken at Colchester, for many successive centuries, to remove from their foundations all remains of Roman edifices convenient in the erection of the buildings, which succeeded at every period; the quantity of stones and Roman bricks dug out from the present hospital garden did appear extraordinary. I observed not only red bricks and tiles of undoubted Roman manufacture, but have also remarked, at the same place, several specimens of a costlier species of white tile, evidently the fabric of that people. Amidst a large quantity of unhewn stone, which has been lately thrown out, and was probably used in foundation, and other works equally removed from the eye, there have been discovered many and well hewn fragments of Swanage; much of which stone is observable also in the castle, and among other Roman materials in the walls of the town, in the churches, and in other ancient buildings at Colchester.

While the workmen were digging last year for the foundation of the hospital, I remarked continually the bones of oxen, deer, pigs *, and fowls, amongst the Roman remains: and, from

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* These were evidently of the wild species.

similar observations that have occurred upon the sites of other Roman temples, it is far from being unreasonable to consider these bones as relics of sacrifice.

We know that it was a custom with the Romans, as with the Greeks and the Egyptians, to place images of the Sphinx in the *pronaos* of their temples. It is precisely to such a purpose, that I suspect our beautiful sculpture in stone to have been dedicated, in the vestibule of the temple of Claudius *.

But it may be asked, Why, in such a situation, should a mutilated victim be exhibited under the fangs of the Theban Sphinx †? I cannot certainly venture in an unhesitating tone to attempt an explanation of this peculiarity. Yet, if we adopt the views of Hoffmann, this exhibition of the utter destruction of the victim may be fairly accounted for, and may be shewn to have been perfectly adapted to the supposed situation of this Sphinx, as a warning emblem for all who presumed to pass the holy precincts, to enter even the porch of the temple. Hoffmann ‡ considers the fable of the Sphinx, destroying such as did not understand her mysteries, to intimate that those who observed not the precepts of the gods, were abandoned to her, as to the infernal minister of that divine wrath, which would not fail to consign the disobedient to torments and to death §.

In reply to any hesitation that may be felt in granting the claims advanced for the Roman origin of our antique stone, upon the ground of the victim being thus introduced; I beg

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* In the same temple, it is supposed that the worship of *Camulus* was also maintained. This may have been a native appellation of the God Mars, thus latinized, as it was in many other parts of Europe; where the worship of the Deity under that designation, would, from various and authentic records, appear to have been observed.

† See Pausanias, Bœot. c. 26. The base of our figure appears to have been left in a rude state; this may be allusive to the rock, upon which the oracular monster was said to have her residence; and whence she rushed to destroy those who could not solve the riddles she propounded.

‡ *Lexicon*, Vo. Sphinx.

§ Pausanias, describing the celebrated temple and statue of the Olympian Jupiter, says—"Before his feet the Theban youth are seen forced away by Sphinxes; and under the Sphinxes, Apollo and Diana are piercing with their arrows the children of Niobe. B. 5, ch. ii.—Mr. Taylor's translation.

leave to observe, that it is only requisite to turn to Montfaucon [*], to Hoffmann [†], to Raspe [‡], or to Millin [§], among the mass of antiquarian authority to which we might refer, for the exhibition of a victim conjoined with the image of the Grecian Sphinx. Yet, in the careful attachment, which is sometimes perhaps too exclusively retained, for precedent; we may be called upon to give an example from antiquity, of the destruction of a victim of the Sphinx, being shewn as already completed. The following view of an Amethyst Gem, copied from the *Traité des Pierres Gravées* of Mariette [||], is for this purpose very satisfactory.

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[*] *Antiq. Expliq.* T. 2, part. II. c. xvii.

[†] *Lexicon*, Vo. Sphinx.

[‡] *Descriptive Catalogue of Tassie's Gems*, No. 8596, &c.

[§] *Galerie Mytholog.* Pl. 142, No. 502.

[||] I here beg leave to express my thanks to the learned medallist in the British Museum, Taylor Combe, Esq.; for his obliging courtesy in having pointed out this Gem, as well as several coins of Cunobelin, which were before unknown to me.

We have here Œdipus expounding the enigma of the Sphinx *, who is seated on a rock; whence she would seem to have thrown, as if in defiance, under the hero's eye, the bones of some one who had unhappily preceded him. Again, we find at No. 106 of *Tassie's Gems*, a fine engraving upon carnelian †, which Mr. Raspe has classed amongst the Egyptian antiques, having a Sphinx winged and sitting, with *a death's head at her feet*. No. 8,601 of the same collection, is taken from an ancient engraving upon onyx ‡, which represents "Œdipus explaining the enigma of the Sphinx, who is sitting upon the top of a rock," Here also may, as I think, be discerned *a human scull at the bottom of the rock* §.

But what shall we say upon that very extraordinary reverse of the preceding coin, No. 2 of Cunobelin; representing, as Mr. Ruding questions doubtfully, " a British warrior, with the head of an enemy in his right hand?" and upon the obverse is a Sphinx!

I have elsewhere observed, that the head of the man upon the Colchester stone has many portrait-like peculiarities; that the hair is moreover short, appearing as if artificially curled; that although it would represent a middle-aged man, yet that it is beardless: all which circumstances combined in suggesting my first idea, that it had not been modelled after the head of any individual of an ordinary class. The closely-shaven beard, as well as the carefully-curled hair, were fashions, not only with the Romans of high rank, but, I remark, that upon all the coins of Cunobelin, as well as upon those of an earlier British stamp, in every instance, excepting where a divinity seems to have been personified, the heads are dressed with short hair; where the state of the art could attain the expression, it appears

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* According to the vulgar tale of the Sphinx's riddle, as proposed to her by Œdipus, whose solution of it, is said to have been the cause of her destruction; it was: What animal is it, that in the morning walks on four legs, at noon on two, and at night on three? To which the Hero answered—" Man."

† *Cab. Flor. Gori Mus. Flor.* II. No. 94, 2.

‡ Stosch MSS. Cat. 41, 7.

§ The sulphur impression that I have of No. 8,601 is not very distinct.

curled, and the faces are always beardless. Mr. Pegge has also noticed, that although, according to Cæsar [*], the Britons wore their hair long, but shaved it on every part of the body, except the head and upper lip, yet that there are no signs of the flowing hair upon the coins: the reason for which he apprehends to have been, that their princes were exempt from this general rule.

I am inclined to differ from Mr. Ruding, in his description of the above coin, No. 2, and to consider that, from the costume of the human figure, it is meant for the image of some Roman divinity: this idea receives support from the altar near to it, as well as from the reverse of another coin of Cunobelin, given by Mr. Ruding in his Appendix [†]. Scarcely can the reverse of the coin, No. 2, be thought to represent the effigy of a Roman emperor; much less a priest, or any one at sacrifice, the back being turned to the altar.

But, before any attempt at a conclusion, I refer again to the British coins, engraved in Mr. Ruding's work. Plate ii. No. 22. Rev. "Probably a Briton driving his chariot over a falling enemy." Nos. 23, 24, 26, and 53, following, appear to represent the same thing; and in Nos. 28 and 29, the hand underneath the (horse or other) four-footed animal with a human head, and raised, as it were, in a supplicating posture, seems to be a similar indication. In all these examples, (as well as in some other coins of the same class), the quadruped has the head of a man, or rather of a woman; and in Nos. 23, 24, and 53, it is distinctly winged. Further, upon the head of many of the same monsters, we may perceive a cap or coif, resembling that upon the head of the Sphinx, in the coin No. 3, here engraved.

Thus, in the absence of every chance for obtaining from history, any lights by which to penetrate this dark matter; we can only attempt to guide ourselves by such interpretation, as may not be thought too extravagant in an apposition of those monuments of antiquity, that are supposed to be remains of nearly the same time in this country.

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[*] *De Bell. Gall.* L. 5.

[†] Append. to the Annals. Pl. 29, No. 6.

In a former publication upon the Colchester Sphinx, I had hazarded a supposition, that the head of the mutilated victim might have been designed, in conjunction with the more ancient emblem of Grecian allegory, to allude to the united wisdom and strength of the Roman army, having in co-operation with a native Trinobantine force, succeeded in the destruction of the power of some great chief of another district of the island.

Yet, further than mere conjecture, I dare not adventure: here therefore I dismiss this abstruse subject, with these rude sketches of theory perhaps presumptuous. I may have already incurred the charge, of wildly expatiating upon what time has placed, as I fear, far beyond the ken of the most learned inquirer. Indeed could we see through all the darksome distance of its history, the amplest results might prove, like many of the fruits of antiquarian labour, to have been scarcely worthy our ungrateful toil.

There now remains little more, for our immediate purpose, than to point out the probable circumstances that may have preserved unto our time, this Stone Sphinx; which I apprehend to have been a principal decoration of the temple erected at Camulodunum, in the time of the first Claudius.

This Sphinx had probably been placed upon a pedestal, and perhaps companion to another; as we see in Montfaucon, that a Sphinx was erected upon either side the entrance to the temple of Diana Pergæa. It has evidently been designed, to be seen at an elevation somewhat above the level of the eye; for the only parts of the work, which have not been highly finished, excepting around the base, are the upper edges of the wings at their contact.

The temple at Camulodunum is recorded by Tacitus to have been destroyed by the natives, irritated with the tyrannous sway of the Romans at that station*. The historian says expressly, that this temple which had been erected in honour of the deified Claudius, was looked upon by the inhabitants as a fortress, built for the purpose of their eternal bondage; and

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* A.D. 62.

that the priests, appointed, as in appearance, for objects only of religious service, wasted the substance of the people. The soldiery, having no proper citadel at the time of the British irruption into their settlement, fled for shelter within the walls of the temple; there they endeavoured, but in vain, to defend themselves for two days, when it was taken by storm. The temple would appear to have been destroyed with the rest of the colonial buildings, which were burnt or razed to the ground by the exasperated Britons. The Sphinx fell, as we may suppose, in the undiscriminating ruin; and although the statues of Claudius, or of the Roman Mars himself might be objects so hateful, as to merit the more particular enmity of Boadicea and her people; yet the less prominent magnitude of our relic might escape the observation of the wild destroyers; and might have lain concealed for centuries, and would probably have still remained so for as many ages more, had not the most unexpected accident brought it to light, from amidst the confused variety of ruin whence I so lately assisted in disinterring it.

The engravings here presented, are sufficiently satisfactory for a general view of the sculpture; and, as a contemplation of the original can alone enable one to form a correct idea of its beauties in detail, I shall subjoin only a few words, with respect to the chief characteristics of the Colchester Sphinx.

It comprises every component part of the allegoric monster of classic fable. The stone presents us with the bust of a young maiden of graceful delicacy, conjoined in a gradual transition, that is admitted by the intermediate form of the body of a bitch, with the ponderous yet evidently active powers of a lion's hinder quarter. This portion of the symbolic figure, is completed by that most expressive emblem the serpent, which forms the tail; and it is introduced with considerable skill in tortuous foldings, that keep it, without undue constraint, within the narrow limits, that the confined size of the material has imposed upon the artist. The back of the Sphinx is furnished with the wings of an eagle, beautifully raised, though not expanded. Thus, our various emblem, which may perhaps, without offence to history, be styled a "Gentile Cherub," in the language of a late writer upon Pagan Idolatry *; or again, a Symbol of the Great Universal Mother of Mythology; or in other terms, (to adhere to expressions of more common use), this Pantheic Sign combines in a striking manner the principal attributes apportioned to their host of gods, by the fancies of the heathen: we will not wait here to inquire, whether these several images were the offspring of grateful love, or of the more effective impulse of fear, in the original simplicity of very early times; whether they were altogether, or in part, a corrupted copying, or, as some deem them, a perverse depravation, of sacred record; or otherwise the mere result of hieroglyphic painting and poetic fancy. These mystic signs, thus arbitrarily adapted and conjoined, are now brought before us into one view, by the first antique copy of Grecian art of this occult character, that has ever been discovered in Britain†.

With respect to the origin of the *Theban Sphinx*, of which so many varied tales have been promulgated, and of whose name so many etymons have been advanced; it may be sufficient here to observe, that the Grecian Sphinx is, no doubt, immediately derived from that of Egypt. The Egyptian Sphinx has hitherto been more commonly acknowledged the prototype of all the others; as supposed to be derived from a simple emblematic reference to the annual inundations of the Egyptian valley, by the waters that descend from the hills of Abyssinia, and overflow the banks of the Nile, while the sun is passing through the signs of *Leo* and of *Virgo* ‡. I have not however

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* See *the Origin of Pagan Idolatry*, by G. S. Faber, B.D., for much curious speculation on this subject.

† I should not omit noticing that, under the very roughly-hewn base of the Sphinx, is clearly engraven a large and well-formed Roman S, of somewhat more than five inches in height. It appears from its situation (removed altogether from the eye of the spectator) to indicate, not so much the artist's name, or any circumstance intended to be recorded; as simply the intention of the inferior workman, who having been employed to prepare the block for the sculptor, thus marked it as a particular stone selected and set apart by the latter, for the representation of the Sphinx.

‡ Such an application of the Sphinx figure by the Egyptians, would

yet found that any one has combated with sufficient evidence, an hypothesis that I have recently proffered, of this fabulous form having been taken by the Egyptians, from the Chaldaic or Assyrian Sphinxes. The Mithraic figures of ancient Persia, that call forth the wonder of modern travellers, among the wreck of Persepolis, are doubtless cognate with, or directly copied from, those of Babylon, which have from time to time been discovered amid the ruins of that *most ancient city;* and are in some instances *found there of precisely the same form as the Egyptian Sphinx**.

But wandering amid the mazes of conjecture, vainly endeavouring to obtain a clear idea of this antique matter, the reader may be looking anxiously forward to the end of our almost hopeless inquiry; and may well be disposed to exclaim with the outwearied patience of *Milphio* in the play:

> Si nequeo facere, ut abeas, egomet abiero:
> Nam isti quidem hercle orationi Oedipo
> Conjectore opus est, qui Sphyngi interpres fuit.
>
> *Plautus—Pœnulus,* Act. 1.

ART. II. *Some additional Facts relating to the Division of the Eighth Pair of Nerves, communicated in a Letter to the Editor of the Journal of the Royal Institution, by* A. P. W. PHILIP, M.D., F.R.S. Ed., *&c.*

I CANNOT, in any way, refer to the division of the eighth pair of nerves, without taking the opportunity to acknowledge the very candid and liberal manner in which Mr. Broughton

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appear still more likely; if the etymon of its name be allowed (according to the Abbé Pluche) to be found in the Hebrew, signifying nothing more than *Superabundance.* "*Sphang*, redundantia, Job xxii. 11. and 4 Reg. ix. 17. and Paraph. Chaldaic. in Proverb. iii, 10. 'Vino torcularia redundabant.'"—*History of the Heavens*, v. 1. b. 1. c. 1.

* I allude more particularly to a very ancient Gem in fine preservation, which, with several others of extreme curiosity, has been found within these few years, upon the Site of Babylon, by Captain Lockett, who brought them to this country, and with a sight of which I have very recently been favoured by Mr. Landseer, to whom they are intrusted.—See *Archæologia*, Vol. xviii., Art. 45.

has, in the last Number of the above Journal, stated the results of the experiments lately performed at the Royal Institution.

It appears, from the observations of that gentleman, that it is admitted by all who witnessed these experiments, that after the eighth pair of nerves are divided in the neck, and the lower portion folded back, little, if any, progress is made in the digestive process; whereas, if the divided ends are not displaced, its progress is still considerable.

An interesting question here arises, whether in the latter case the nervous influence is still conveyed in consequence of the divided ends of the nerves remaining in actual contact, or in consequence of its passing through moisture or other intervening bodies.

In order to determine this question, the following experiment was made, in which Mr. Cutler, Assistant Surgeon to the second regiment of Life Guards, had the goodness to assist me.

A rabbit was kept without food for twenty-four hours, and then allowed to eat as much parsley as it chose; as soon as it ceased to eat, the eighth pair of nerves were divided in the neck, without displacing them. It was evident, both to Mr. Cutler and myself, that at the moment the division of the nerves was made, the ends so retracted as to cause them to separate from each other to about the distance of the sixth part of an inch. The animal was allowed no food after the operation. It was found dead at the end of eight hours.

On the stomach being opened, it appeared that digestion of the new food had made considerable progress. The neck was examined, and the divided ends of the nerves were found to have remained at the above-mentioned distance from each other.

This experiment was repeated in all respects in the same manner. The ends of the nerves now retracted to the distance of a quarter of an inch from each other on both sides. The animal lived about six hours, and the digestion of the new food was far advanced.

Mr. Brodie examined the state of the food and the position of the divided nerves in this rabbit, and both he and Mr. Cutler, allow me to state, that they are satisfied that the nervous influence had, in the foregoing experiments, passed from the upper to the lower portions of the divided nerves.

The following facts, relating to the division of the eighth pair of nerves and the power of galvanism in obviating its effects, appear now to be admitted on all hands,—

That after these nerves are divided in the neck, and the lower portions folded back, little, if any, progress is made in the digestive process.

That if the lower portions, thus folded back, be connected with the positive end of a voltaic battery of a certain power, the other end of the battery being connected with the skin over the stomach, the efforts to vomit which follow the division of the nerves do not take place, and digestion goes on as perfectly, as far as can be judged from the appearance of the contents of the stomach, as in the healthy animal.

That when, instead of the lower portions being folded back, they, as well as the upper portions, are allowed as much as possible to remain in the natural position; digestion makes great progress, notwithstanding the divided ends so retract as to be separated from each other by the distance of a quarter of an inch.

That the difficulty of breathing occasioned by the division of the eighth pair of nerves in the neck is prevented by the influence of the voltaic battery applied in the way above pointed out.

The reader will judge how far these facts tend to establish the identity of the nervous influence and galvanism.

But in judging of this question, it is necessary in addition to the foregoing facts, to keep in view those which seem to prove that the nervous influence is altogether distinct from, and has nothing in common with, the sensorial and vital powers, and possesses no properties but those of a chemical agent; and that the influence of the voltaic battery is not only the best of all stimuli to the muscular fibre, and capable of passing along the nerves in either direction, but is also capable of raising the temperature of living arterious blood, while it can produce no such effect either on venous blood, namely, that which has already undergone the effects of the secreting power, or on arterious blood, which has lost the vital principle. I need not here refer to the well-known facts of electricity being evidently conveyed by the nerves and under the power of the will in certain animals, and the newly dead brain forming, with other parts of animals, a galvanic apparatus.

It has been said that the restored digestion and free breathing produced by the influence of the voltaic battery after the interruption of the nervous influence, may arise from the former acting as a stimulus to the secreting surfaces; but it will appear, I think, that this explanation which is derived from the laws of the muscular system, is founded on a false analogy. It is proved, by the most simple experiments, that after the nerves of a muscle have been divided, the application of a stimulus still calls it into action; the cause of which is explained by those experiments which prove that the muscular power is independent of the nervous system, and only affected by its influence in the same way as by other stimuli. But with respect to the secreting power, all the experiments on the subject combine to prove that it so immediately depends on the nervous system, that it ceases to exist as soon as the influence of that system is withdrawn.

It may be proper to add, that the experiment which proves that the division of the nerves only destroys the secreting power when one portion of them is folded back, is an *experimentum crucis* respecting the subject of my late discussion with Dr. Alison. It appears from it, that it is not the injury done to the nerve, but the interruption of the nervous influence, which destroys the secreting power.

Mr. Field found that an opening made in the trachea of a horse, large enough freely to admit of the introduction of two fingers, had no effect in relieving the dyspnœa occasioned by division of the eighth pair of nerves.

ART. III. *On Secret Writing, in reply to* Mr. Chenevix's *Challenge. By the* Rev. Edward Hincks, A.M., *and formerly Fellow of Trinity College, Dublin.*

THE expedients which have been adopted by persons who have occasion to carry on a correspondence necessary to be concealed, and yet liable to be intercepted, have been very numerous. More than one might be pointed out, by which absolute inscrutability is attained with a very trifling expenditure of trouble: but as our regularly-bred physicians refuse to administer the nostrum of a quack, even in diseases where its healing efficacy is acknowledged by every one but themselves; so professional cipherers have always exhibited an unwillingness to have secrecy obtained by any other means than a *literal* cipher. I employ this term to distinguish from *syllabic* and *verbal* ciphers those in which the *letters* of the sentence to be concealed are separately expressed by characters, either simple or complex, in the cipher. I am not, of course, sufficiently acquainted with the diplomacy of the present day, to be able to state whether secretaries for foreign affairs, and their agents, are so precise in their ideas as to decline to be secret, unless they can be so *secundum artem;* but if they be, they are certainly more scrupulous than their predecessors of the 17th century were; and yet I much doubt if they can better baffle those whom violence or treachery might put in possession of their despatches.

My intention, in the present paper, is to expose the futility of *literal* ciphers; among the rest those of Mr. Blair, (REES's *Cyclopædia*, Article CIPHER,) and of Mr. Chenevix, (*Journal of Science*, &c., No. XIX.) I do not mean to assert, that no literal cipher can be contrived so as to be sufficiently secret; but that this cannot be effected without a key, so extensive and complicated that it might be applied with equal facility to syllables, as to letters. And if so, a syllabic cipher is unquestionably preferable; inasmuch as, being equally secret, it is written and read with about one-third part of the labour.

Of literal ciphers there are two kinds; one, in which the signification of the symbols is varied, the other, in which it is permanent. In the former, the number of symbols is generally small, and the cipherer depends for secrecy on the intricacy of the law according to which their signification is changed, or on the length of the period of the change. Under any circumstances this method is extremely troublesome; but, if the law of variation be not of the very simplest nature, the labour of both the reader and the writer of the cipher will be prodigious; and if the law be simple, it is plain that the period of the cipher can contain no more *loci,* (as Mr. C. calls them,) than the cipher contains characters; and this, I think, insufficient (with the usual number of characters, at least,) to defy the powers of a decipherer. I consider, in fact, this kind of literal cipher so obviously inferior to the other, that I should scarcely have noticed it, but for the purpose of explaining Mr. C.'s first specimens, (Nos. 1—12,) in which he has employed it.

The germ of this method is to be found in the dial-cipher of the Encyclopædia Britannica; and, in fact, the key to these specimens may be most commodiously exhibited in the form of a dial. Let there be constructed a circle of paste-board, moveable on a pivot in its centre, and attached to a larger circle concentric with it; let the interval between the two peripheries, and the rim of the inner pasteboard circle, be each divided into thirty equal compartments, and in the former let the alphabet, with Mr. C.'s four additional symbols, be arranged in the following order; *viz.*, a b c d e 4 f g h i j k 8 l m n o p q r s t ϕ u v w x y z φ; on the rim of the inner circle let the following letters be arranged, *viz.*, e e e a a i i n n o o s s t t r r u u b c d f g h k l m p y. Set the indicator, or capital letter prefixed to the cipher, to correspond with the first of the three e's, and in this position of the dial the signification of each letter in the outer circle will be the corresponding letter in the inner circle. This position of the dial will explain the first *division* of the cipher; for the following division the inner circle must be moved forward or backward, (as the position of Mr. C.'s arrow determines,) through one compartment; and so for the other division; always moving the inner circle a compartment forward or backward at the end of each division. If a capital letter occur in the middle of the sentence, as in Nos. 2 and 3, set the dial for that indicator, and the key of the first division following it will be given.—Then proceed as before. In Nos. 1 and 6 the divisions correspond with words, in Nos. 9 and 10 with letters; in fact, they are perfectly arbitrary in their lengths.

By help of a key, constructed as I have described, it will be easy to read the concealed sentence, Nos. 6, *&c*., which is this "Monuments of learning are durable;" and to correct the key, sentence, line 2, word 3, which should be "many" and not "five." Other errata, proceeding probably from the carelessness of the printer, are to be found in several of the ciphers.

It will be remarked, that in this cipher (and it is the case in all Mr. C.'s,) j must be represented by i, and v and w by u; but the letters q, x and z, are also omitted in the present key; and as none of them occurs in either of the sentences given, it is impossible to tell what contrivance Mr. C. uses to represent them; the letter marked k in the key will probably suffice for them all. But see Mr. C.'s character of a *complete* cipher, (p. 92). How can this be applied to any of those that he has proposed?

It would also appear from Mr. C.'s being able to cipher "Europe in 200 ways, Emancipation in 1280," (p. 93,) that he had *five* symbols for e in this key; there appear, however, to be only *three*. Has Mr. C. any contrivances to represent this letter, not exhibited in any of his 12 specimens? or are we to understand the passage in p. 93 as applying not to this variable key, but to the permanent one of No. 18?

I pass now to the second kind of literal ciphers, or those in which the symbols retain each a permanent signification. Secrecy is in such ciphers to be looked for, by having a considerable number of characters, especially to represent those letters which are of most frequent occurrence. It is evident that a sufficient supply of symbols for the most extensive key of this sort that could be required, would be furnished by the series of natural numbers, commencing suppose with unity, and continued *ad libitum;* a point, or comma, being interposed between every two for distinction. The letters of the Hebrew alphabet, accompanied by the several vowel points that may belong to them, would also afford an amply sufficient number of characters; but the difficulty of dictation by the person holding the key to his amanuensis, is no small objection to the use of these. A friend of mine has composed a cipher consisting of nine radical characters, those composing the well-known figure #,) which, with one, two, three, or more points at pleasure, above, below, or in the body of the character, compose a sufficient variety of symbols for any purpose; but the last-mentioned objection applies with even greater force to this system. Besides, the use of any of these sets of symbols, and especially of the two last, requires very complex characters to be frequently written; while, at the same time, it affords none of that *additional* secrecy, which the employment of professedly complex characters will often confer on a cipher. I shall return to the consideration of this additional secrecy. I now proceed to describe some of the principal ways in which cipherers have sought to procure a multiplicity of symbols, by combining together characters of a few different kinds. The varieties of the complex symbol will always be in number that power of the number of varieties in each part, whose exponent is the number of parts. In Lord Bacon's cipher there were five parts, and two varieties only in each part. This gave for the number of symbols 32, the fifth power of 2. In the line writing, (Plate 2, Cyclopædia, Art. Cipher,) there are three positions of the line, and three lines to a symbol, which gives 27 symbols. In the dot writing, (plate 3,) *four* dots composing a symbol, we have 81 symbols. Symbols composed of two letters are in number 676; if with the letters be mingled a point, as Mr. Blair has done, there will be formed 729 different symbols, the square of 27; and so in other cases. To commence with Lord Bacon's cipher. From the smallness of the number of possible symbols, 32, (of which, if my memory do not fail me, but 26, or 24, were in use,) it is plain that his lordship looked to elude the vigilance, rather than to defy the skill, of a decipherer. His professed object, if I recollect rightly, (but I have no access at present to his work,) was to express *omnia per omnia*, that is, to write any given passage in such a manner as to convey any given secret meaning; and this in such a manner as to excite no suspicion in the mind of any casual inspector of the writing that such secret meaning was involved in it. It is necessary in his Lordship's method that the secret meaning should be much shorter than the outward visible contents of the letter; not exceeding one-fifth of it. For the conveyance of very laconic messages this method is, I think, unrivalled; though an improvement would be required in the formation of the two alphabets, which, as exhibited by Lord B., are neither sufficiently distinct, nor sufficiently void of suspicion. Perhaps it might be better to compose the characters of five words, than of five letters, and to have the variation consisting in the correctness or incorrectness of their orthography. A mis-spelt word, or one improperly made to commence with a capital, or abbreviated, might supply the place of one of his Lordship's crooked letters; and if the outward epistle was that of a servant, or mechanic, or other ignorant person, this would create no suspicion; while the multitude of expletives used by such persons in their correspondence, would much facilitate the composition of the cipher. The employment of the names and style of such persons as I have alluded to, although in a somewhat different manner, has been, it would seem, a common trick in diplomacy; and, unless Privy-Councils and Secretaries of State were egregiously mistaken in their suspicions, the endeavours of an exiled monarch to recover possession of his throne, have been often shadowed under those of a turned-off servant to get back to his place, or of an ale-house-keeper to have his license restored.

I have spoken at greater length than I otherwise should on Lord Bacon's method, as it has been, I think, unfairly represented in Mr. C.'s paper. The difficulty of writing it is much exaggerated, and the real source of its secrecy is lost sight of.

I now proceed to Mr. Blair's different methods of secret writing. Of the dot or line writing with 27 characters, it is needless to say any thing, as it can have no pretensions to inscrutability. That with 81 has not yet received a fair trial, as Mr. B. accompanied his specimen with a key and interpretation, and the latter served to unravel whatever intricacy was attached to the former. I believe that, with a different and unknown key, it would be *possible* to decipher a long specimen of this sort of writing, but certainly not one of a few sentences only, or which concealed only the important words of the writing. I must however own, that I think still greater secrecy would be attained if, of the 81 characters, only about 40 were used to express the *letters* of the alphabet, and the remaining half applied to the *syllables* and *short words* of most frequent recurrence. The cipher of 729 characters has certainly no need of any additional contrivances to increase its secrecy, as with a new key, even arranged on a similar plan to the present one, which is by no means necessary, detection is entirely out of the question. But wherefore use above 700 literal characters, when 100 is sufficient to produce absolute secrecy; and even half that number, if syllabic characters be interspersed? The cipher is, beyond a doubt, sufficiently copious, without in the smallest degree diminishing its secrecy, to express by separate characters all the particles, pronouns, auxiliary verbs, and terminations, as well those that belong to the grammar, as those that make up the great body of the dictionary, and will have a number yet to spare for the proper names, and other words, that may be beforehand selected, as specially likely to occur in the correspondence. But to employ a cipher, possessing such means as this, in the manner Mr. B. has done, to denote *merely* the letters of the alphabet, appears to me a waste of time and trouble, to as little purpose as if he had turned the force of a steam-engine to draw a cork, or to crack a nut. What I have objected to the dot writing of 81 characters, will apply with apply with equal force to the figure-cipher which admits 100 characters, each composed of two figures. But, in his application of this method, Mr. B. has adopted a very happy contrivance, the description of which leads me back to a subject I promised to return to—the additional secrecy produced by the employment of complex characters, above what the same number of simple ones would create. This additional secrecy consists in this, that a person unacquainted with the key can never be sure before he commences the task of deciphering, how many of the simple characters he is to consider as coalescing into a single complex one. The difficulty thus occasioned is not eluded as easily as might at first be supposed, namely, by trying first one number, then another, till the right one be ascertained; for, of the elementary characters, some may compose perfect characters by themselves, while others are always found as ingredients in complex ones. This is the case in Mr. B.'s figure-cipher in the Cyclopædia. To accommodate it to the same key by which the other ciphers are read, it was necessary to reduce the number of complex characters from 100 to 81. This he accomplished by selecting one of the ten figures for a perfect character, while the other nine were always used in combination with each other. This 82nd character was used by Mr. B. as a null, making the intervals between the different words. By doubling it, and combining it with another complex character of the same nature, the different *stops* in a sentence might, with very little trouble, be expressed; but, if I recollect right, this has not been attempted by Mr. B. The existence of a single character of this sort may be detected by observing what elementary character is never found with an *odd* number of characters between its consecutive appearances; this rule however fails, if more than one such character be found in the system. Other contrivances increasing the confusion produced by complex characters to those ignorant of the key, but affording no additional trouble to him that possesses it, whether for writing or reading, have at different times occurred to me; and I have formed out of them a system, which I think possesses as much practical utility as any literal cipher hitherto published, the key being extremely simple, and yet of such a nature as even if it were intercepted, or partially discovered, not to make known the *entire* meaning to a person ignorant of a particular secret connected with it, which may be readily understood and remembered, but which could scarcely be guessed. This may be useful in case it should be necessary to commence a secret correspondence with a person abroad, to whom there is no opportunity of safely sending a copious syllabic key; for, if this could be done, I am not so partial to my own method as to think that this would not be preferable. Though I do not fear that any specimens I should give would be *correctly* and *fully* read, yet I shall abstain from making the trial at present, as I do not feel myself called upon to offer any *reward* for the discovery, and without one, few would probably choose to make the attempt.

It will now be expected that I should say something of Mr. Chenevix's ciphers 13—18. In these he has connected with the use of permanent characters a contrivance hitherto, I agree with him, never adopted. I own, however, I suspect the reason *why* it was never adopted, to be, not that it was never thought of, but that when thought of, it was always rejected. My objections to it are very great on many accounts, but as I do not choose to describe the novel contrivance, I must decline stating them to the public. The want of secrecy, which, when Mr. C. reads the remainder of this paper he must be convinced it possesses, is alone a sufficient objection to it. On this subject it is very plain that he has greatly deceived himself. His opinions that "the security is at least one hundred times as great with a double as with a single key," and that either of them exceeds in power "the limit $(m-1)^n$," (he should have said m^n-1), are both very erroneous, and show that he has very little acquaintance with the principles of the art of deciphering. Incorrectly as the specimens 17 and 18 are printed, especially the former, he will here see that *they have not escaped detection,* any more than those where a known sentence, ciphered by the same key, was given along with them. Perhaps Mr. C. may be surprised to learn that I deciphered No. 17 in considerably less time than No. 18. The frequency of small and common words it would therefore seem is of more service to a decipherer, and of course of more dis-service to the employers of the method, than the trifling difference of one or two keys being used in the different parts of the sentence. I am not very sanguine in my expectations of the reward offered for deciphering these specimens, and yet I think the following sentences comply with all the conditions. I first give the explanation of

Nos. 15 and 16. I have already given that of Nos. 6 and 12. I then give the *translations* No. 17, into the cipher used in No. 18, and *vice versâ;* thus showing that I can not only *read* but *write* in Mr. C.'s most secret ciphers. As to the composition of these, or similar keys, I am not aware that it is very difficult, when the method is so completely discovered as it has been by me. This Journal, at any rate, would not be the proper place for publishing the specimen of such a key.

Sentence ciphered in Nos. 15 and 16.

Should this sentence also escape detection, the security of the cipher will receive additional confirmation.

Substance of No. 17, in cipher of No. 18:

ljy a84naf cxkzoi rbq4i ϕgamtuy az imktz koop yammmfa 8gii vcca faayφ srdxfaϕ tzxlmpj qb4yxd mkmoiicvuw lddd 8rppϕqaussφ8d ii mdfzttrφdϕd faaffa mmm lsaxn kldbϕ wuyzkfmg.

Substance of No. 18, in cipher of No. 17.

iφjlbd8rrt pxcdϕφ ijmhismmpn tmcj zx isodzmp nmmmoix ndϕopp efgmtt stllxbprr xφxw4eee nfacrl oxahhaa wwoccoϕ dlxcmadvd cdoφ8pplf hisgdv llxcfcw mmrpp facimponn axy cfmldϕee jripmgxfs tt hhkmp ll plll8s glgamx wxlsfeaa4m gxsupϕϕ dfnmhgalm pw8fn tt cfcdqtt qklamk tzxt pgkx pccctq.

gh ddeaa zymijhe bdyφl ukifyyyy oyl lt8φoj lm.

The last sentence contains my address in the cipher of Nos. 13 and 16.

Art. IV. *Contributions towards the Chemical Knowledge of Mineral Substances. By the late* Martin Henry Klaproth.

[Continued from Page 280, of Vol. XI.]

Analysis of the Ferro-arsenical Sulphuret of Copper, or Fahlerz, from Freiberg.

The usual difficulties of ascertaining the relative quantity of arsenic where sulphur also is present, occurred in the examination of this ore, and being unable to obtain satisfactory results by any humid process, I proceeded as follows :—

A.

a. 200 grains of finely pulverized fahlerz were heated to redness in a small glass retort; the neck of the retort contained after this operation 17 grains of red *sulphuret of arsenic.*

b. The residuary ore had fused into a lead-gray mass ; it was pulverized, mixed with half its weight of charcoal powder and submitted to a second sublimation, the retort being kept at a bright red heat for one hour; it thus afforded a crystallized sublimate of pure *arsenic,* weighing 22.5 grains.

c. The residue was now taken out of the retort and exposed upon a red-hot plate so as to burn away the charcoal, during which the fumes of arsenic were also perceived to escape. After this operation the residuary ore weighed 146 grains, so that it had sustained a loss of 54 grains, which, however, cannot be regarded as correctly indicating the loss of arsenic and sulphur, since it is probable that during the combustion of the charcoal a portion of oxygen was absorbed so as to increase the weight of the residue.

d. The roasted ore was now digested in nitric acid diluted with its weight of water, and the blue solution passed through a filter, upon which there remained 7 grains of a mixture of charcoal and red oxide of iron, which being separated by muriatic acid, 3 grains of charcoal remained.

e. On adding the muriatic solution of iron to the preceding nitric solution, a portion of muriate of silver was thrown down, which being carefully collected and reduced, afforded 0.80 grains of *silver.*

f. The solution was now mixed with sulphuric acid and evaporated to dryness; the residue was perfectly soluble in water, so that it contained no lead. Excess of ammonia was therefore added, which separated *oxide of iron,* weighing after having been dried, mixed with oil and ignited, 45 grains; and being perfectly attractable by the magnet.

g. The dark-blue ammoniacal solution, supersaturated with sulphuric acid and precipitated by zinc, gave 82 grains of *copper*.

B.

The above processes afforded no indications of antimony, which, if present, probably escaped with the arsenic: 100 grains of the ore, therefore, were roasted without the addition of charcoal, and then digested in muriatic acid and dissolved by the aid of nitric acid, added drop by drop. The filtered solution, which was of a green colour, was concentrated by evaporation, supersaturated with potassa, boiled, diluted, and filtered. The filtered alcaline liquor after having been neutralized with sulphuric acid, and mixed with carbonate of potassa, was scarcely rendered perceptibly turbid.

C.

To endeavour to ascertain the relative proportion of sulphur to that of arsenic, 200 grains of the pulverized ore, mixed with half their weight of charcoal powder, were submitted to sublimation. A few drops of moisture and some fœtid gas was evolved, and the sublimate in the neck of the retort was opaque, reddish brown, and of a metallic lustre; but that in the dome of the retort consisted of pure *arsenic*. The sublimate weighed 35 grains: it was pulverized and digested in weak solution of potassa in a gentle heat; the liquid acquired a brown colour, and left undissolved 23 grains of metallic arsenic in the form of a heavy black powder; it afterwards deposited 1 grain more of arsenic, and the solution became colourless; the addition of acids separated the sulphur in the form of egg-yellow flocks. The colour of the sulphur, and the colourless appearance of its alcaline solution, indicated the existence of a portion of remaining arsenic.

Amidst these obstacles to accurate results*, I am induced to estimate the proportion of sulphur at 10 per cent., and to assume the following as the components of 100 parts of this ore:—

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* To the above Klaproth has added two other analyses of Fahlerz, but as they are nearly similar to that given in the text, and equally open to fallacy as far as concerns the determination of the weights of sulphur and arsenic, we have omitted them.

Copper	A *g*	41
Silver	*e*	.40
Arsenic		24.10
Iron	*f*	22.50
Sulphur		10
	Loss	2
		100

Analysis of an Antimonial Sulphuret of Copper, (Graugültigerz,) from Kapnik.

a. 300 grains of this ore in picked and clean crystals were reduced to powder and digested in a mixture of three ounces of nitric acid, (sp. gr. 1.230,) and an ounce and a half of water. There was some evolution of nitrous gas, which was increased by heat, and when the action ceased the liquid was poured off, and the residue again digested in two ounces of nitric acid diluted with one of water; the insoluble portion was then collected and washed upon a filter.

b. To the pale blue nitric solution concentrated by evaporation muriatic acid was added, which produced a slight turbidness, and the mixture being heated, afforded only half a grain of a precipitate, which did not behave like pure muriate of silver.

c. The solution was next divided into three equal portions.

1. One-third was evaporated nearly to dryness, and the green saline mass moistened with sulphuric acid which presently formed a clear solution, indicative of the absence of lead; the solution was supersaturated with caustic ammonia, which left an insoluble portion of precipitate, which being collected and ignited weighed 3.5 grains. It was dissolved in muriatic acid, and the solution decomposed by prussiate of potassa. After the separation of the blue precipitate, carbonate of potassa occasioned a slight brown deposit which weighed 0.25 grains, and examined by the blow-pipe appeared to be *oxide of manganese*, so that the proportion of *iron* may be assumed at about 3.25 grains. The dark-blue ammoniacal liquor was now supersaturated with sulphuric acid, and the *copper* being separated by the immersion of a plate of clean iron weighed 37.5 grains.

2. Another third of the nitric solution was mixed with excess

of caustic potassa and boiled; when cold, the precipitated oxide was separated by filtration, and the filtered liquor being neutralized with muriatic acid was mixed whilst boiling hot with carbonate of potassa, a white precipitate was thus formed, which after ignition weighed 6.5 grains. This precipitate became yellow when heated, and again white when cold, which seemed like oxide of zinc; it readily formed a colourless solution with dilute sulphuric acid, and afforded crystals of sulphat of zinc. These 6.5 grains of oxide I consider as equivalent to 5 grains of *zinc*.

3. Since the long digestion in nitric acid rendered it probable that a portion of the sulphur of the ore had become acidified, the remaining third of the solution was devoted to the determination of its quantity; it was therefore mixed with muriate of baryta, and the precipitate when collected, washed, dried, and ignited, weighed 66 grains, which I consider as equal to 9.25 grains of sulphur.

d. The yellowish-white insoluble matter, remaining after the action of nitric acid, was digested in muriatic acid; the solution was of a straw-yellow colour, and the sulphur separated in yellowish flocculi; it was collected, washed with very dilute muriatic acid, and dried; its weight was then 57.25 grains; burned upon a hot plate it left 1 grain of residue, which being fused upon charcoal with borax, yielded a globule of *copper* weighing 0.75 grains.

e. The muriatic solution, concentrated by gentle evaporation, gave no trace of muriate of lead; it was divided into 3 equal parts.

1. One of these third parts was diluted with ten parts of water, which rendered it turbid, muriatic acid was therefore added so as just to restore transparency; hydro-sulphuret of ammonia was then dropped in, which gave a precipitate of a pure orange-colour.

2. The second third of the muriatic solution was largely diluted with water, and the white precipitate being collected and moderately heated weighed 30 grains. The remaining liquid saturated with carbonate of potassa afforded a greenish-gray

precipitate which consisted of oxide of antimony and copper, but which, when dry, scarcely weighed $\frac{1}{8}$ of a grain.

3. The remaining third of the muriatic solution was diluted with 6 parts of water, and rendered again transparent by an adequate addition of muriatic acid; a plate of zinc was then immersed, which effected the separation of 22 grains of metallic *antimony*.

The components, therefore, of 100 parts of the ore under examination are,

Copper	(*c* 1.)	37.50	37.75
	(*d* 1.)	0.25	
Antimony	(*e* 3.)		22
Zinc	(*c* 2.)		5
Iron	(*c* 1.)		3.25
Sulphur	(*c* 3.)	9.25	28
	(*d* 3.)	18.75	
Silver	(*b*)		together about 0.25
Oxide of manganese	(*c* 1.)		
Loss			3.75
			100

(Here follow five other analyses of varieties of the same ore, upon which the author observes, that copper, antimony, iron, and sulphur, are alone to be regarded as the essential components; silver, mercury, and zinc, being merely accidental additions.)

Analysis of a Cupreous Sulphuret of Lead and Antimony, from Clausthal in the Hartz.

a. It was ascertained by previous trials that the quartzose matrix of the purest parts of this ore bore the proportion of about 13 to 100.

226 grains, therefore, of the ore were heated to redness in a glass retort, but nothing sublimed, and the ore appeared merely to have undergone imperfect fusion.

b. It was therefore reduced to powder, and digested in 4 ounces of a mixture of equal parts of nitric acid and water.

The mixture was then further diluted with four ounces of water, and the digestion continued at a higher temperature till the residue acquired an uniform gray colour; the whole was then poured on a filter.

c. The nitric solution was concentrated by evaporation and mixed with muriatic acid, which produced no further change than that the original sky-blue colour became green; the addition of sulphuric acid, however, caused an abundant deposition of sulphate of lead, which being separated, the remaining solution was supersaturated with caustic ammonia, by which the brown oxide of iron was obtained, and after mixture with oil and ignition, it afforded 10 grains of magnetic *oxide of iron.*

d. The ammoniacal solution was mixed with sulphuric acid in excess, and a plate of zinc, then immersed, gave 23.5 grains of *copper.*

e. The insoluble residue of the ore was boiled in muriatic acid with the addition of a little nitric acid, and this operation was repeated with fresh acid so long as it acted; the remainder was placed upon a filter and washed, first with very dilute muriatic acid, and then with water. It weighed, when dry, 64 grains; its sulphur was burned off at a gentle heat, and left 28 grains of incombustible residue; so that the *sulphur* consumed amounted to 36 grains.

f. The remainder, boiled again in muriatic acid, was found to consist entirely of siliceous sand, and weighed 26 grains.

g. The several muriatic solutions were concentrated by slow evaporation and afforded abundant acicular crystals of muriate of lead; these were carefully collected, and the evaporation continued as long as they could be obtained; they were then washed with alcohol acidulated by muriatic acid, dissolved in hot water, and converted by the addition of sulphuric acid into sulphate of lead, the quantity of which, including that separated in process *c*, amounted to 120.5 grains, equivalent to about 85 grains of *lead.*

h. The remaining muriatic solution, now free from lead, and which by the test of hydro-sulphuret of ammonia appeared to contain nothing but antimony, was decomposed by a sufficient addition of water, and the precipitate collected, dried, and heated, weighed 51.5 grains, which by collateral experiments was found equivalent to 39.5 grains of *antimony*.

So that 100 grains of the cupreous sulphuret of lead and antimony consist of

Lead	(*g*)	42.50
Antimony	(*h*)	19.75
Copper	(*d*)	11.75
Iron	(*c*)	5
Sulphur	(*e*)	18
	Loss	3
		100

Analysis of the Sulphuret of Bismuth and Copper.

This is a massive ore of a steel-gray colour, soft, and giving a black streak.

A.

As a preliminary trial, 100 grains of the powdered ore were digested in a moderate heat with nitric acid ; sulphur and fine siliceous sand remained : the solution was filtered, somewhat diluted, and tested by muriatic and sulphuric acids, neither of which caused any turbidness, so that neither silver nor lead were present. Water in larger quantity being added, a white oxide of bismuth separated, and a plate of iron, immersed in the residuary solution, threw down the copper.

B.

a. To determine the relative proportions of the ingredients of this ore, two hundred grains were boiled in muriatic acid, and nitric acid added, drop by drop, as long as it produced any action. The insoluble portion was carefully collected, washed and dried, and being duly heated the sulphur burned away to the amount of 17.5 grains. The residue was again digested in nitro-muriatic acid, and the portion which resisted its action was properly washed, dried, and heated so as to burn off the remaining sulphur, which amounted to 3 grains, leaving a siliceous residue weighing 37 grains.

b. The solution was evaporated till it acquired the appearance of a grass-green saline mass, which was redissolved and largely diluted, by which a milky mixture was obtained; this was set aside in a warm place, where it deposited a white precipitate, which was collected, washed and dried by heat: it weighed 94 grains.

c. To determine the proportion of metallic bismuth contained in this precipitated oxide, 100 grains of pure bismuth were dijested in muriatic acid, nitric acid being gradually added till the metal was dissolved; this solution was then evaporated in a sand heat to a saline mass, which was put into a large quantity of water, and the precipitate washed and dried as before; it weighed 122 grains. The remaining acid liquor was neutralized with carbonate of potassa, by which a further portion of oxide of bismuth, amounting, however, only to ¼ grain, was obtained. So that it appears from this experiment that the 94 grains of precipitate obtained in the analysis from 200 grains of the ore, are equivalent to 77 grains of *bismuth.*

d. The filtered liquor remaining after the separation of the oxide (*b*), was of a blue colour; it was saturated with potassa to separate the copper, which, though at first thrown down in the form of a blue precipitate, became, after having been some time gently heated, of a brown colour; it was collected, edulcorated, dried, and ignited, and was then found to weigh 70.75 grains, equivalent to 56.5 grains of *copper.*

Hence, 200 grains of this ore have yielded

Bismuth	77 grains
Copper	56.5
Sulphur	20.5
Silica	87
	191

But as the silica is to be considered as derived from the quartzose matrix of the ore, the components afforded by 100 parts of the pure ore will stand thus—

Bismuth	47.24
Copper	34.66
Sulphur	12.58
	94.48

[Klaproth attributes the loss, amounting to 5.52 per cent., to oxygen, and supposes that the bismuth is not contained in the ore entirely in the metallic state.]

Analysis of the Blue Iron Ore of Eckartsberg.

This ore occurs in kidney-shaped masses of an indigo-blue colour; a tint which it derives from exposure to air, for when recently taken from the strata in which it is found it is nearly white. It is soft and easily friable.

A.

a. 100 grains gradually heated red-hot in a small retort lost 20 grains, and acquired a brown colour. The loss consisted of pure water.

b. Exposed in a crucible to a more intense heat, the ore melted into a steel-gray slag, of a metallic lustre, and slightly magnetic.

B.

a. 100 grains of the ore were mixed with a solution of caustic soda, and evaporated: the dry mass being softened with water, there remained brown oxide of iron, which was collected, edulcorated, dried, triturated with a little oil, and ignited in a close crucible. It gave 47.50 grains of *black oxide of iron.*

b. The alcaline liquor was neutralized with nitric acid, and tested by ammonia, which afforded no precipitate: the ammonia being again supersaturated with nitric acid, acetate of lead was added, which caused a precipitate of phosphate of lead, weighing, after having been washed, dried, and moderately heated, 142 grains, equivalent to 32 grains of phosphoric acid.

100 parts, therefore, of this reniform blue iron ore, consists of

Black oxide of iron	(B *a*)	47.50
Phosphoric acid .	(B *b*)	32
Water	(A *a*)	20
		99.50

Analysis of the granular Chromic-Iron Ore, from Steiermark.

The specific gravity of this ore, freed from the talcose matrix in which it is embedded, is = 4.500.

a. 100 grains lost by moderate ignition 2 grains, and acquired a perfect metallic lustre : it was levigated, mixed with a lixivium containing 500 grains of caustic potassa, put into a polished iron crucible, evaporated to dryness, and moderately ignited for an hour; the mass at first frothed up, but afterwards entered into quiet fusion, and when cold was of a sap-green colour; it was readily soluble in warm water, depositing a reddish-brown precipitate, which was collected upon a filter, washed, dried, and digested in boiling muriatic acid. 23 grains remained insoluble, which were fused with potassa, and treated as before, by which the insoluble portion was reduced to four grains, and was ultimately rendered entirely soluble by another repetition of fusion and solution.

b. The muriatic solutions were mixed with caustic ammonia, by which a brown precipitate was formed, weighing, when duly washed, dried, and ignited, 35 grains. It was again dissolved in muriatic acid, when it left two grains of *silica,* so that the weight of the oxide of iron was 33 grains.

c. The green alcaline liquors were carefully neutralized by nitric acid, during which *alumina* separated, amounting, after having been dried at a red heat, to 6 grains.

d. The neutralized liquor, after the separation of the alumina, appeared of a fine orange colour. A cold nitric solution of mercury was added to it, till no further precipitation ensued, and till the supernatant liquid appeared colourless and transparent. The precipitate, which was of the colour of fine vermilion, weighed, when edulcorated and dried, 369 grains. The mercury was driven off by moderate ignition in a platinum crucible, and there remained pure *oxide of chrome* of a dark grass-green colour, weighing 55.5 grains. 100 parts, therefore, of the above ore, consist of

Oxide of chrome	(*d*)	55.50
Oxide of iron	(*b*)	33
Alumina	(*c*)	6
Silica	(*b*)	2
Loss by ignition	(*a*)	2
		98.50

ART. V. *A Letter respecting the Construction of a Balance, from* Capt. HENRY KATER*.

London, Aug. 8th, 1821.

DEAR SIR,

I OBSERVE in the last number of the *Quarterly Journal*, edited at the Royal Institution, the description of a Balance from a drawing by Mr. Children. I should not have troubled you merely to claim this as my own arrangement, but to notice an error in the description where the beam is said to be "of platinum;" it should have been "of bell-metal," which combines the essential desideratum *lightness* with the requisite degree of strength.

This beam had its origin in a wish to render the hydrostatic balance less expensive without diminishing its accuracy and sensibility, and it has in every respect answered my expectations.

Dr. Wollaston has contrived the following additional apparatus, which he finds particularly convenient. The annexed wood-cut represents the bottom of the case supporting the balance, A and B the scale pans. Four slips of wood, or brass, are connected by pins forming centres of motion at *a*, *b*, and *c*, the centre *c*, being fixed to the case. Four brass pins, *e*, *f*, *g*, *h*, of a sufficient length, project perpendicularly from the side pieces. These

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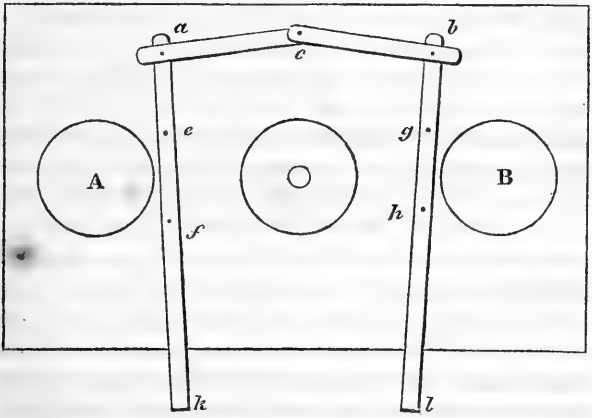
* We need scarcely observe that we were not aware to whom Mr. Robinson was indebted for the plan of the very useful balance of which we have given an account in our last Number. We merely saw the instrument in the possession of Mr. Children, who was kind enough to furnish the sketch from which our engraving was made.

pins are to be brought into lateral contact with the scale pans by means of the ends *k*, *l*, when the beam is elevated above its support. The knife edge being then lowered upon the agate planes, and the pins withdrawn, any want of equilibrium becomes instantly perceptible.



I am, dear Sir,
Yours very faithfully,
Henry Kater.

W. T. Brande, Esq.

Art. VI. *On Pitchstone.* *By* J. Mac Culloch, M.D. F.R.S., *&c.*

The information to be procured respecting this rock is so scanty, from its limited occurrence and the imperfection of the observations hitherto recorded, that I cannot pretend to place its geological history in a very luminous point of view. There is reason to suspect that some of the remarks on its position have been warped by theoretic views of the same nature as those which have influenced the corresponding observations on the trap rocks; while it is also apparent, that the peculiar jaspers occurring in beds under trap, have been occasionally confounded with it, as I lately remarked in a communication on the subject of jasper. Under these doubts, I can only pretend to give its history as far as it can be deduced from the facts visible in Scotland. Hereafter, when it has been an object of more accurate research, its true history will, probably, be more completely elucidated.

In its geological connexions, as well as in some of its mineral characters, it approaches very nearly to many members of the trap family; although it presents, at the same time, some very remarkable differences. It is also important to remark, as it appears to strengthen the connexion between this rock and that family, that it is found in those districts or countries where trap exists.

It has been said to occur in the form of strata in many parts of the European continent; but no example of this disposition has yet been observed in Britain. Those masses which have been mistaken for strata in the island of Arran, are no more entitled to that name than the masses of trap which are so often found similarly placed; they are merely veins holding a parallel or conformable direction to the rocks by which they are included, as will be more fully pointed out immediately.

It will also be found that the stratified pitchstones of Europe, so often described, and so well known, do not appertain to this species of rock, and that the whole of the observations on these rocks are founded on a mineralogical error; that they have arisen from confounding the resinous looking jaspers, described under that head in a preceding article, with true pitchstone. These substances are, indeed, stratified; but they differ from pitchstone, not only in their several characters and chemical composition, but in every essential circumstance of their geological origin and position. This, at least, I may affirm of every specimen of pitchstone from these strata which has hitherto fallen under my inspection; and I have little doubt that a fuller and an unprejudiced re-examination of these imaginary strata, will confirm these views; by proving that they are truly jaspers, and that whenever real pitchstone occurs in masses parallel to stratified rocks, these are conformable veins, like those of Arran.

In every situation except one, where this rock occurs in Scotland, it forms veins, generally so decided in their characters, and intersecting in so marked a manner the accompanying strata at angles, as to permit no doubt respecting their real nature. In a few instances these veins are parallel to the beds in which they lie; but, in this respect, they differ in no way from the intruding masses of trap that so often occupy similar positions. These veins are rarely continued for a long space, more commonly terminating abruptly among the surrounding rocks. The ramification of these veins are so rare that I have hitherto observed it only in one instance; one of the pitchstone veins of Egg being divided round a mass of chert.

The only mass of pitchstone in Scotland of which the true position appears at first sight doubtful, is that which forms the Scuir of Egg, which is rather, however, a substance intermediate between that rock and basalt, and which is at the same time porphyritic. It stands insulated in the form of a narrow irregular wall on the surface of a mass of trap. So far it resembles a bed as little as it does a vein. But that it is not a bed deposited on the trap, nor a stratified rock, is evident from this; that the same mass of trap lies above the latest of the secondary strata containing coal, into which it also intrudes in the usual manner. If this pitchstone mass were, therefore, a stratified rock, it must be considered not only as later than the latest stratified rocks of which we have any knowledge in Scotland, but posterior also to the trap which succeeds these at a distant interval of time. Under any view, therefore, it is no part of the secondary strata. It is more probably the remains of a vein which has once existed in that trap on which it seems now to stand. The degradation of that rock may easily be imagined to have left the more durable pitchstone in the position it now occupies: and of this degradation, the surrounding rock, which is of a loose amygdaloidal character, gives ample evidence. The columnar structure is no objection to this view, as that structure exists frequently in trap veins. Neither is it an objection that this structure is parallel to the sides; since trap veins occasionally exhibit the same disposition of their columnar parts.

As there is so striking an analogy in many particulars between pitchstone and basalt, it is very conceivable that, if not found in regular strata, it may at least exist in the form of large overlying masses, like the trap rocks; and probably some of the foreign examples may be referred to this variety of position.

If the preceding remarks are not judged sufficient ground for excluding pitchstone from the regularly stratified rocks, the following argument will, perhaps, be considered valid. It is found in granite, in red sandstone, in the more recent sandstones, and in the latest trap rocks. It thus occupies a variety of discordant positions which no rock has yet been known to do except the intruding substances of the trap family. In granite, or trap, it is obvious that it could not be stratified; nor are we acquainted in nature with any rock which is found in the form of veins, and in that of regular and true strata also. Even where it occurs apparently inter-stratified among the sandstones, it must therefore be considered as a parallel vein.

Pitchstone presents some other remarkable analogies to trap. It is often porphyritic, or contains imbedded crystals, or irregular grains of feldspar, and occasionally of quartz also. This is an interesting circumstance in its history, as it is found only in the unstratified rocks, (with the exception of granitic gneiss,) or in those which intrude among the truly stratified substances in the form of veins. The porphyritic structure is, indeed, so common in the pitchstones of Scotland, that the instances of it far exceed those of the simple rock; particularly in the island of Arran. It is, perhaps, rare to find any mass which, in some place or other, does not contain imbedded crystals of feldspar, or, at least, rounded and irregular grains.

The amygdaloidal structure, which is so common in the rocks of the trap family, is, however, so rare in the pitchstones, that only one instance of it has yet occurred to me in the various specimens which I have examined. This specimen was from Baffin's Bay, and it contained zeolites, that is, mesotype or nadelstein; resembling, in every respect but its base, the well-known basaltic amygdaloids.

The entanglement of fragments of the including strata in trap veins, has always been esteemed a proof of the intrusion of these veins among the stratified rocks, and as further indicating a degree of violence, simultaneous with or preceding the intrusion of the vein. This very remarkable circumstance is sufficiently common in trap veins to have fallen under the observation of every geologist conversant in districts where these rocks abound; but in pitchstone veins it is so rare, that as yet I have only observed one example of it. This is in the well-known vein at the end of Brodick Wood in Arran. The including strata consist of the red sandstone, and the fragments of the same sandstone are insulated in the middle of the vein. If this occurrence is as yet solitary, it must be recollected that it does not happen, perhaps, in one trap vein of a thousand; so that it may be merely a question of chances, depending on the much greater rarity of veins of pitchstone. The fact, even if solitary, is valuable, as establishing, in another important particular, the resemblance between pitchstone and the trap rocks.

Admitting, or presuming, with most modern geologists, the igneous origin of the trap rocks, there is found in some pitchstones a remarkable circumstance, which, together with the analogies already pointed out to these, seem strongly to indicate an igneous origin for them also. Nothing indeed of this peculiar nature has occurred in any of the porphyritic traps that have come under my examination. In those pitch-stones which are of a porphyritic character, the crystals, or rather grains of feldspar, are rounded but irregular. When broken, it will be found that the feldspar retains its usual plated structure in the centre of the grain, but that it gradually becomes confused, or loses its character; while the outer surface is a coating of white enamel such as is produced by the fusion of feldspar. This feldspar is in some cases the glassy variety, in others the common; and, the change on the crystal is precisely that which may be induced by the regulated action of the blow-pipe. The smaller crystals, it is also worthy of notice, are entirely converted into enamel. These singular varieties occur in the hill of Glamich in Sky, as well as in Arran; and, it is interesting to remark further, the connexion which they possess with pearl-stone.

In tracing the progress of common pitch-stone to pearl-stone, it will sometimes be seen that a spheroidal concretionary, but indistinct, structure exists in some of these, which, by a gradually increasing distinctness of the concretions, causes them to pass into the latter substance. In others it may be observed, that the enamel grains just mentioned, are the centres of a spheroidal concretionary tendency; and that such varieties are in fact imperfect pearl-stones. In the pearl-stones of decided character, it will also be found that the grains or spherules contain a central enamel, and these varieties have been called pearl-stone porphyry. It is, moreover, interesting to observe, that in other cases the enamel and the investing pitch-stone spherule seem to have entered into combination; the result being a pearl-stone of a distinct and peculiar character, but in which the enamel grains may still in some part or other be discovered.

I may, lastly, remark, while on the subject of the porphyritic structure, that in some rare instances, of which Arran affords an example, the feldspar crystals consist of successive concentric prisms, a layer of pitch-stone being interposed between each, so as to produce a compound crystal.

There is no stronger proof of resemblance between pitch-stone and the trap rocks, than the transition from that substance to basalt, a change which is far from being uncommon. The rock which forms the Scuir of Egg already mentioned, holds a place pretty nearly intermediate between the two. Nor are similar transitions rare in basaltic veins in many other cases; particularly where one such vein traverses another of a larger size. In many places, and remarkably at Loch Scavig in Sky, the basaltic ramifying veins that traverse the hypersthene rock, become gradually finer in texture as they divide, until the minute filaments are converted into perfect pitch-stone.

In the same island, a vein of basalt of a considerable size is found in the Cuchullin hills, the outer laminæ of which are pitch-stone, graduating into the common matter of the vein. But Lamlash presents the most striking instance of this nature: here a vein of basalt passes through a vein of greenstone; and the outer parts or walls, to the depth of about half an inch, are formed of a glossy black pitch-stone. In some parts of its course this outer lamina is very decided and distinct, but in others it graduates into the basalt of the vein by an intermediate substance similar to that which forms the Scuir of Egg. A confirmation of this peculiarity, though of a less marked nature, is found in one of the well-known veins of Egg, where the outer part of the vein is a remarkably perfect and brittle pitch-stone, while the inner is formed of the intermediate substance just described.

From all these circumstances it may be concluded that pitch-stone possesses a strong affinity to the trap rocks, both in its geological relations and in its origin, being among the latest of the intruding rocks.

In one respect it presents an important character of difference. While in all cases veins of trap are very persistent, and can in many places be traced to a fundamental mass, those of pitch-stone are remarkably limited in extent, and have never yet been found connected with any principal body of the rock.

Veins of pitch-stone vary considerably in breadth, ranging from many yards to a few inches. In the case above alluded to, where basalt veins become pitch-stone during the progress of ramification, they descend almost to the dimensions of a thread.

In composition they are not always and invariably the same throughout. Independently of the rare cases in which veins composed of basalt in the middle are formed of pitch-stone at the sides, it sometimes happens that pitch-stone of many different qualities will be found in the same vein. In these cases the most perfect or vitreous-like parts are placed as in the former instances, at the outer sides of the vein; and, in the same cases, they present differences of colour as well as of texture and aspect.

Occasionally, these veins like those of trap present a laminar disposition, the lamina being parallel to the vein; and, in such instances, it sometimes happens that there is a change of substance where each lamina terminates; the pitch-stone passing into chalcedony and into chert. In a few instances they present a prismatic tendency at right angles to the course of the vein; and if the Scuir of Egg be also a vein, they must further be conceived capable of dividing into regular columns parallel to its sides.

In respect to the internal disposition of pitch-stone as a rock, it is various, presenting many remarkable peculiarities of structure, both on the great and small scale. In Arran, it is found imperfectly prismatic on a very large scale; the prisms being at right angles to the plane of the vein. In Egg, the prisms are smaller, but capable of being easily detached, in many cases, and often of very perfect forms; being at the same time placed in various intricate directions with regard to the plane of the vein, and in such a manner, that the ends of the several prisms are extenuated, by being compressed between the middle parts of those adjoining. The laminar disposition of the veins noticed in a preceding paragraph, sometimes also occurs on a scale so large as to divide the vein into two or three separate portions, which are most frequently marked by some corresponding change of character in the different laminæ.

The smaller varieties of structure are numerous, and often very remarkable. Among these, a laminar concretionary structure is not unfrequent; and the lamellæ which vary much in size, are either straight or curved. In some cases they are as thin as paper; in others they are thick, and further separable by joints at right angles to the planes of the lamellæ. Thus they become divisible so as sometimes to present a minute prismatic disposition.

Occasionally these prisms present also some curved surfaces together with the straight, so as to form columns on a very small scale, which are further jointed, in some very rare instances, by surfaces alternately concave and convex; and, when such joints are very near, the parts separate into irregular spheroidal forms. In some rare cases of the jointed columnar structure, a central atom of feldspar or of enamel, is found in each joint. This structure presents a considerable resemblance to that of pearl-stone, here considered as a variety of pitch-stone, and the specimens of this nature actually pass into that substance.

Analogous to this is a spheroidal concretionary structure more or less perfect, and varying in size, but commonly minute. By a transition from this, or by the greater perfection of the spheroidal structure, pitch-stone becomes at length an aggregate of irregular grains; and it thus passes, as in the former case, into pearl-stone, which, from this and the preceding fact, can only be considered as a variety of pitch-stone, and is accordingly ranked here as a variety. The spherules or grains of pearl-stone frequently also contain a central atom of feldspar or enamel as already noticed, like the variety mentioned in the preceding paragraph; a circumstance which confirms this view of its nature, and which is perhaps very intimately connected with the peculiarity of its structure.

The last obvious variety of structure to be noticed in pitchstone, is the porphyritic, already mentioned in pointing out its analogy to the trap rocks. This varies much in the magnitude, perfection, or number of the included crystals, producing corresponding differences of aspect. It was already observed, that the larger crystals are rounded, and converted into a white or grey enamel on the exterior, while in lieu of the smaller there are only to be found spheroidal grains formed entirely of the same enamel.

Independently of these obvious varieties of structure, pitchstone sometimes gives indications of an internal arrangement, which, as in the case of the trap rocks, is only detected on wethering. Many of the varieties become of a pure white on the surface after exposure to air, scaling off in successive crusts that resemble white enamel, as they still retain the vitreous aspect and fracture. At the same time the exposed surfaces are covered with undulating lines, resembling certain varieties of marbled paper, and evidently resulting from some corresponding difference of laminar structure; the lamina most sensible to atmospheric action being thus detected, although invisible in the fresh rock. In a further progress to decomposition, pitch-stone is at length converted into a fine clay, forming in water a very tenacious paste; and it appears to be very readily acted on by the water, as may be witnessed in Arran. It does not always weather to a white enamel, becoming occasionally brown, and more rarely turning into a black powder. Occasionally it exhibits the kind of bloom seen on a plum, or on certain kinds of bottle-glass.

It must here also be remarked, that notwithstanding its vitreous aspect, pitch-stone often contains a great deal of loose water, which is easily separated by drying, as in many other rocks. In these cases it is tender, but becomes brittle after the water has evaporated.

The lustre of pitch-stone varies exceedingly, according to its several states or varieties; passing from the most perfectly vitreous to one scarcely more glossy than that of the finest basalt.

The colours are various, greyish white, pale ochre-yellow, brown, olive brown, olive green, dark reddish brown, dark bottle-blue, and black. In Scotland the darker colours, and the olive greens in particular, are predominant.

The ordinary transitions of pitch-stone are into chalcedony, chert, and semi-opal: it appears also to pass into a substance not easy distinguishable from the conchoidal shining jasper found among the clay strata that are entangled in trap or volcanic rocks, with which, as I formerly remarked, it appears to have been occasionally confounded. The transitions into basalt have been already noticed.

Such is a general sketch of its geological and mineral characters, as far as my limited knowledge extends. The more minute particulars will be found in the synopsis of varieties.

It is scarcely necessary to define it, further than to say, that in a general way, it is distinguished from all other rocks by its vitreous or by its resinous aspect; and that it differs from obsidian, with which it possesses, in fact, no affinity or transition, by its inferior hardness. For a fuller definition I may refer to the various systems of mineralogy which are in the hands of every one.

SYNOPSIS OF PITCHSTONE.

FIRST DIVISION. SIMPLE.

A. Amorphous, massive.

a. With a simple conchoidal fracture.

b. With a flat conchoidal fracture.

c. With a splintery conchoidal fracture.

d. With a splintery granular fracture, and of various aspects, according to the size of the parts.

e. With a mixed fracture, the conchoidal being also granular.

f. With a mixed fracture, the large conchoidal presenting a minute additionally conchoidal surface.

g. The conchoidal fracture covered with minute scales, so as to give, on a superficial view, a porphyritic aspect.

B. Concretionary.

a. Flat, lamellar; the lamellæ thick or thin; it even becomes schistose or papery, or fine scaly.

b. Curved lamellar.

c. Prismatic; the prisms simple or jointed; with plain or with curved surfaces.

d. Columnar; similarly, either simple or jointed: the concretion is also sometimes partly columnar, and partly prismatic; or has plane and curved sides.

e. Spheroidal; large. This is generally combined with the columnar. The small spheroidal concretionary, is arranged with the pearl-stones.

f. Very imperfectly spheroidal, concretionary; also passes into the porphyritic.

§. Sometimes different structures are intermixed in the same rock, as the columnar and prismatic with the lamellar. The lamellar pitch-stone sometimes contains lamellæ of chert, or of cherty chalcedony, into which it also passes.

SECOND DIVISION. Porphyritic.

A. Pitch-stone porphyry of mineralogists.

a. With distinct crystals of glassy feldspar.

§. These are sometimes so transparent as to be nearly invisible on a fresh fracture.

b. With distinct crystals of common feldspar.

c. With rounded, or shapeless particles of the same.

d. With imbedded spherules, consisting of a grain of feldspar surrounded by a grey enamel, or of the grey enamel alone.

§. This enamel is sometimes blended with the surrounding rock. When the spherules are numerous, this porphyry passes into pearl-stone.

e. With imbedded crystals or grains of quartz, or of quartz and feldspar both.

f. With grains of quartz surrounded by chert, and blending with the surrounding rock; or with grains of chert alone. This variety is analogous to *d.*

g. Porphyritic; but the grains, which are chiefly of quartz and chert, are further condensed in lamellæ, which alternate with a slightly porphyritic pitchstone.

THIRD DIVISION. Concretionary-spheroidal: Pearl-stone.

A. Consisting of simple pitch-stone; the grains irregular, and compressing each other in the manner of coccolite.

B. Some, or all of the grains, containing a central substance of another kind; pearl-stone porphyry.

a. With grains of feldspar, or of feldspar and enamel, or of enamel only.

b. With quartz, or quartz and chert.

c. With both quartz and feldspar.

d. With a central atom of clay.

FOURTH DIVISION. Amygdaloidal. *Containing imbedded nodules of another Mineral.*

A. Pitch-stone containing imbedded zeolites.

§. As yet this variety has occurred only in Baffin's Bay.

§. The colours of pitchstone are various. They are principally dull white, pale ochre, pink, pale green, greenish grey, ochre yellow, ochre red, yellow brown, fawn colour, red brown, greenish brown, olives of various hues down to blackish green, dark blue, and black. These colours are sometimes also intermixed; and thus, pale and dark green, or pale and dark grey, are interlaminated, producing a striped surface. The colours of pearl-stone are much more limited, possibly because the substance is more rare. Those of the porphyritic varieties are numerous, as this rock is more frequently porphyritic than simple.

§. As the porphyritic varieties may possess any of the modifications of the simple rock as a base, many additional aspects result from this cause.

§. Besides the passage into chert and cherty chalcedony already noticed, pitchstone passes into basalt, and, as is supposed, into opal and semi-opal. In the passage to chert, it is sometimes found to contain minute grains or particles of chalcedony, discernible only by the lens. This variety often possesses a remarkable spheroidal concretionary structure, which was mentioned in a preceding part of this communication.

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

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

Continued from Vol. XI. p. 271.

Essay XI.

Air is rendered heavier by the separation of its lighter parts.

In discussing the third method by which air becomes heavier, namely, by the separation of its lighter parts, I begin with this incontrovertible truth, that if from any thing its lighter parts be taken away, the remainder will be heavier; I do not say that it will be heavier than the whole original quantity, but than a portion of it equal in bulk to the remainder. Separate the silver which that knave of a goldsmith mixed with the gold in King Hiero's crown, the remaining gold will be heavier than an equal portion of the entire crown; what in this case you do by art, nature effects by her industry, having no other tool but heat, which serves her worthily in the work. Observe the preparers of salt, who receive the sea water into their pans, through canals; they know that the sun's heat subtilizing the water, will sublime it into air, leaving them the salt, the heavier part, at the bottom. The alchymist, true ape of nature, desirous to imitate her, places his infusion of rhubarb over a chafing dish, that the liquor exhaling the extract may remain behind. But if he want that part, which being subtilized, flies off, he stops it by the way, (sly fellow as he is) *(Cauteleux qu'il est,)* by means of the heat which he applies to his alembic. By this scheme he obtains the brandy, which is lighter than the wine, from which it rises, and the wine lighter than the lees remaining after the whole distillation. In this manner heat acts on all sorts of liquors, rarefying some parts, thickening others, and always separating them by the difference it occasions in their respective weights, *(par peser plus ou moins.)* It produces the same effects on air; to observe which, turn your face, I beg, towards that plain over which the sun has all the day darted his rays. You think, I wager, that the air which is in immediate contact with it, is lighter than it was in the morning? just the reverse; it is much thicker, and consequently heavier; for how can the heat have subtilized the air, without having caused it to ascend, and how have caused it to ascend without the descent of a heavier? Nothing rises by its own power, it is the falling down of another body which pushes it up. Doubtless it has separated the most subtle, and raised it on high, leaving the heavier below, as happens in the distillation of liquors. But if this argument do not convince you of the thickening and increased weight of this heated air, at least believe the evidence of your own senses. I undertake to make you feel that it is so by the touch of your hand, and see it with your eyes. Now, whilst it is mid-day, do you not feel the air warmer to the touch, than it was a quarter of an hour after sunrise? It is not however that the sun has imparted a higher degree of heat to it, since that never varies, and it disperses it at a constant rate within the sphere of its activity; for in like manner as its rays transpierced the air without resistance, and communicated all its light to it in a moment, so did it communicate its heat, which has not itself increased, though its action is greater, from the greater density of the matter it acts on; for the more subtle parts being gradually raised up, the others remain below in greater number, and more united in the self-same place; and from this greater union comes this greater action. This may be made clearer by considering the nature of elementary fire, which although hot in the utmost degree, nevertheless does not burn, in consequence of its extreme rarity, but red-hot iron burns violently, not that it is hotter (for how can its heat exceed the utmost degree) but because it is denser, containing more parts in an equal space. So much for the touch, let us proceed to the other sense. When the sun began this morning to send its rays on the horizon, the air, by its subtlety, was wholly invisible; but now do you not see how it trembles over the furrows? It is because it is thicker, and has acquired more body that it is in some measure visible to you. Thus, I think, I have sufficiently acquitted myself of my promise; but it is fit that I should go a step further, and add, that if the simple heat of the sun thus thicken our lower air, and make it heavier, driving upwards its more subtle parts, and keeping the denser and more solid *(drues)* below, what will not the vehement and long-continued heat from the mouth of a red-hot furnace effect? A quantity of brandy placed over it in a vessel will disappear, common water, and all sorts of liquors, will exhale in a few hours. The air nevertheless will remain there, (there being no other body to fill its place) but it will be an air of the utmost possible density and weight; an air, as I may say, no longer air, but that has lost its nature, having changed its subtle fluidity for a viscid grossness, *(grossièreté visqueuse.)* For the violence of the fire, subtilizing all the air that comes near it, will drive an immense quantity of it to a distance, leaving around itself, of this immense quantity, only a kind of dregs, which from its glutinous weight cannot fly off.

Essay XII.

Fire, by its heat, can thicken homogeneous Bodies.

I know not what fatal calamity has seized the sciences, that when an error has arisen in them, and they are, as it were, inured to it by time, their professors will not endure its abatement. The doctrine of the preceding essay has already been taken in dudgeon, *(formalisé)* and it has been objected to me, that since fire thickens heterogeneous bodies, by separating their more subtle parts, as being of a different nature, it cannot do the same with homogeneous bodies, inasmuch as it acts uniformly on all their parts, and has no other action than that of distending and dilating them equally; so that, on this account, air cannot be thickened and made heavy by the force of heat. I recognise this doctrine, (thus opposed to my belief,) as derived from the school of philosophers, whom I honour for their great insight into nature; but I frankly avow, that I have never sworn by the words of any of them. If truth be with them, I receive it;—if not, I seek it elsewhere. Let us see if they have met with it in this matter. Homogeneous bodies, they say, are those whose parts are all of the same nature, or which have the same name and definition as the whole. I certainly agree that fire acting on such bodies, does, of its own accord and nature, *(de soy et par sa nature,)* dilate them; but reason teaches me, and experience confirms it, that, by accident, as we say, and in consequence of the subtilization and separation of some parts, the others remain thick and heavy. If that be denied me, and the aforesaid doctrine must be rigorously investigated, as if fire, neither by itself nor by accident, could thicken homogeneous bodies, I challenge it as false, and could bring forward a cloud of instances to the encounter; but the courteous reader, for whom I labour, will be contented with a few.

Vitriol is a homogeneous body, for its parts have the same name and definition as the whole. Now, if this be put in a retort, fire so acts upon it, that it shews us separately its phlegm, its oil, and its colcothar, parts which differ in thickness and weight. Turpentine is a homogeneous body, the smallest part being no less turpentine than the whole mass; if this be put in an alembic, fire, by its action, dilates some of its parts, and thickens others, separating its water, spirit, oil, and colophony, whose difference in respect of weight and subtlety is notorious. I have already spoken of wine, (also a homogeneous body,) which fire, displaying its powers on it in distillation, extends and dilates till it extracts its brandy and weak water *(eau petite,)* as it is called; and the residuum is thicker in proportion to the quantity of this water or phlegm that has been drawn off. But why do I take the trouble to bring forward these examples, since it is evident, that from all such bodies, salt, sulphur, and mercury, parts which sensibly differ in tenuity and weight are extracted by the help of fire; it is not true, therefore, that fire dilates all their parts equally. I well foresee that some will endeavour to evade this, by saying that the instances I propose are of compound bodies, and that it would be otherwise with those that are simple. Yet, forsooth, have I proved the falsity of this maxim, taken, as they lay it down, in its general sense, and extended to all homogeneous bodies. But let us see if it be more consistent with truth when applied to simple bodies.

ESSAY XIII.

Fire can thicken Water.

Water, unquestionably, is a simple body, and it is equally certain, that fire acting on it, dilates some of its parts, and thickens others; albeit, as I have said already, the first action is proper and natural to it, and the second accidental. Pour a barrel of water into an alembic, heat it according to the rules of art, and draw off first a gallon *(pot*)* of it. It is certain that the water of this gallon will be more subtile than that which you put into the alembic. If any one stimulated by the desire of contradiction deny it, let him be refuted by the chemists; who not being able to make their extracts conveniently with common water, are accustomed to use distilled water or dew, (which is nothing else than water passed through the great alembic of nature,) for such water being more subtle, better penetrates the substance of the simples, and more readily draws out their virtues and tinctures. Moreover, its greater diuretic effects, and its less weight, (the inseparable companion of a less degree of thickness), give ample proof of the truth of my assertion. But if the water of this gallon be more subtle than the water put into the alembic, that which remains must be thicker, inasmuch as a thickening is a necessary consequence of the separation of the subtle part. This will be more evident if you continue the distillation; for, drawing off gallon after gallon, till no more remains, the last will be sensibly heavier and thicker than the first; and this sensible difference will ensue by small, but imperceptible, degrees, from the first to the second, from the second to the third, and so on consecutively to the last. Nor will this difference be only from gallon to gallon, but from glass to glass, indeed, from drop to drop; for since the two extreme drops must manifestly differ in thick-

* A measure nearly equal to a gallon.

ness and weight, this difference must continually be increasing from the beginning to the end, by the increase of the number of drops arising from the distillation. From this it appears, that, as in heterogeneous bodies, fire separates the parts that are of different natures, so in homogeneous bodies, it disjoins the parts which differ in tenuity, and then weight assumes the office of giving them rank, and of assigning to each its place, especially in fluids, the heaviest portions of which, always gain the bottom, making their way through those that are less so, and necessarily falling down in them. So that if all the water that could distil from the above-mentioned pipe, fell in order into a tube of sufficient length, and of the size of a quill, it is credible, that the second drop would sink in the first, and the third in these two, and so consecutively to the last, which being the heaviest, would pass through all that preceded it, occupying the lowest place; so that the drop which sought the first, would, at the end, find itself in the highest place. Now, although this continual traversing occasions some degree of mixture to the parts, yet would it not be such, but that the distinction in weight of the high and low portions would always be very discernible. But as we cannot see this sinking of the drops by the eye, if any one call it in question, let him apply and dexterously join the mouth of one phial full of water to the mouth of a similar phial full of claret wine, and he will behold a similar thing; for the water being the heavier, will descend into the lower phial through the wine, manifestly forcing it to mount into the upper. Does not the wine itself arrange its more subtle parts at the upper part of the barrel, and its grosser at the bottom, by means of the greater weight of one than of the other?

The common people think also, and not without reason, that the first glass we pour out of the gallon is more subtle and vaporous than the following. This difference observed in so small a vessel might lead some to imagine, that if we were to make a tube only an inch wide, and several toises in length, fill it with wine and leave it for some time at rest, the upper portion, if not absolute brandy, would come very near it in tenuity and efficacy. A pretty invention, certainly, to draw off brandy without fire, if the fact be so, (*si la chose allait ainsi*;) and the difficulty of making the instrument did not prevent the use of it. All these observations serve me as a bridge to pass to this general assertion, that in all fluids, as well compound as simple or elementary, the upper parts always differ in subtlety and weight from the lower; and that this difference is distinguished by as many degrees as their matter is divisible by their height into distinct parts: so that if we conceive a line drawn from the lowest part of one of the fluid elements (as air) to its highest surface, just so many degrees of weight and subtlety will there be in that element, as the line can be divided into different parts, (I mean materially to avoid sophistry,) and the upper part of all will always be thinner and lighter than the second, the second than the third, and so on to the end. For to attribute to all the parts of each element the same body, (*corpulence*) is to belie our sense, which compels us to consider air (for instance) more subtle at the summit of a mountain, than in the plain at its foot. And, in like manner, when the heat of the sun, or of our fire, subtilizes it here below, it mounts on high, unquestionably, till it meets with its like, according to the degree of subtlety it has acquired. Besides, if this equality prevailed through the whole elements, there would be no reason why one portion of it should be below, and one above, when the air is calm. For to commit that to chance and hazard, would be to shock the incomparable wisdom of the Author of nature, who has made nothing in it without number, weight, and measure; and has established such order throughout, that nothing happens fortuitously and without cause. I conclude, therefore, that this arrangement is derived from weight, and no otherwise. But to conclude this essay, I say that every one may now see that fire acting on the simple body, water, does not equally distend all its parts, but by dilating some it separates them, whence follows the thickening of others. Thus the maxim in dispute is not true. But it will be said this must be shewn of air, on which the pivot of the controversy turns. That is their last refuge, and I proceed to deprive them of that.

ESSAY XIV.

Fire can thicken Air.

THE reasons discussed in the 11th Essay, are sufficient to satisfy an unprejudiced mind that fire heating air subtilizes and separates some of its parts, and that this separation is necessarily followed by the thickening and increase of weight of the others. But since this truth is obstinately contested, in order to exhibit it more clearly, I demand that a laboratory be prepared for me in the region of elementary fire, adjoining that of air, and in it I will shew them ocularly what they are unwilling to believe. For as the vessels which we here call empty are nevertheless full of air, so will they be there full of fire. And since when we pour water into an alembic, the previously-enclosed air is expelled, so will fire give place to the air which in that region shall be poured into the alembic; and when put upon the furnace will distil over drop by drop into the receiver, and the first measure of it that is collected will be more subtle than the second, the second than the third, and so on to the end. What is more, the difference in subtlety and weight between the first and last measure will be as perceptible as in those of distilled water. Now if any one laugh at my demand, let him know that the great Archimedes required, in a like case, that a spot should be given him in the region of air where to place his engine, and he then promised to lift up the whole earth. (*Δος πȣστω, ϰαι την γην ϰινησω.*) Not that he supposed that what he demanded could be done, (for in the estimation of the wisest men he was neither madman nor fool,) but he made it, confident in the certainty of his demonstrations, and for clearer evidence of the truth of his assertions. My demand too has no other object. Whoever would see a thing approaching to this, without having recourse to an impossibility, let him place an alembic of extraordinary dimensions on a furnace, and having fixed a bladder, emptied of its air, to the highest part of the head by a small pipe, begin to heat it; the air of the alembic will then begin to dilate, and being no longer retainable in its original space, will rush out and fill the bladder. Let another similar bladder be filled and fitted in like manner, and so on to the end. I say that the last bladder will be heavier than the first; whoever doubts it, let him make the experiment, proceeding exactly as directed. By this train of thought my mind elevates itself to greater things, which, however, I shall not mention now, as they are irrelevant to this subject, and difficult not only to practise but even to understand. I come to another demonstration, by which the truth that I defend will be more than evident (*plus que visible.*) Plant a cannon directly on its breech, its muzzle pointing upwards, and throw into it a red-hot ball of the size of its bore. It is certain that the air contained in the cavity of the gun is so thin in substance and small in quantity, that the ball, in passing, will impress on it its full degree of heat; notwithstanding this, if you put your hand into the muzzle, you will easily keep it there at first, but in a short time you will be unable to do so; not that the air has increased in heat, for it will rather have decreased, as well as the ball which gradually cools; but, because being thickened by the separation of the more subtle parts of a large quantity of air which will rush out quickly, (*d'un abondance d'air, qui s'y portera grand erre*) it will act more powerfully, as I have said elsewhere. In the second place, the air which will be seen trembling above the muzzle of the gun, (which does not take place at first,) compels us to confess, that it is thickened; for, it cannot be said that these are vapours or exhalations rising from the gun, for every thing about it is too dry and solid to suffer the escape of any of its own substance. Thirdly, if the air were not thickened above the muzzle it would not render the objects beyond it, seen through it, confused. Nor can my opponents excuse their disbelief on account of the tremulous motion (*brandillement*) of the air, since I distinctly see that lady's beauties through the air, which she agitates with her fan, and also all sorts of objects through an atmosphere violently disturbed by the north wind, when it blows and whistles boisterously. Lastly, if a flock of wool much extended (*esparpillé*) be placed over the muzzle of the gun, it will not descend; and, if it be pushed some distance into the mouth, it will suddenly rise up again, which would assuredly not happen if the air were not thicker than on (*à l'escart*) one side of the cannon where the flock of wool descends readily. These reasons though not gross, are nevertheless so palpable that they must convince every one that heat has thickened the air just above the muzzle of the gun. But if it has thickened it before the mouth, what, I pray you, must it have done at the bottom of the bore next the ball? Unquestionably when that comes out after it is cold, you will see it more inclining to a whitish hue than before it was heated red, as if the thickened and adhesive air gave it that colour, which in time tarnishes and wears off, especially in a damp place, since the surrounding air, moistening that which adheres to the ball, reduces it to its original state. By way of dessert I will serve up one remark to the reader, which perhaps will be to his taste. They who worthily practise medicine are sometimes called in to visit an asthmatic patient, who panting in bed in a hot chamber, fetches his breath with great difficulty, which the physicians observing, they have the window thrown open, conduct the patient to it, and make him inhale the outer air, to his great relief. If you ask these gentlemen whence the sick man receives so immediate comfort, they will tell you, it is because the air of the chamber being too hot cannot furnish that necessary refreshment to the heart, which the external air by its coolness affords. But gentlemen, my honoured colleagues, having undeceived myself on this point by the preceding meditations, suffer me, I pray you, to undeceive you also. It is not the heat of the air of the chamber that occasions the panting, as not being capable of sufficiently refreshing the heart, but rather it is its thickness, which retards its course across the *obstruction* (*literal*) of the lungs, so that it cannot furnish the heart regularly (*à temps*) with sufficient matter for the generation of the vital spirits, which fresh air being more subtle, can better effect. Now, that you may not fancy I advance this without reason, observe the feverish patient who lies in the same chamber, whom the confined air sufficiently refreshes, although he has greater need of it; and, if fever were to come on to the asthmatic patient (which you wish for his advantage,) and dissipate the matter that closes the vessels of the lungs, would not the same air then sufficiently refresh the sick man, now that the need of it is increased? Does not the same thing happen if this clearing of the lungs be affected by the diasulphur, which, as you well know, Galen compounds of sulphur, pepper, and mustard-seed in given quantities. Heat then must have thickened the air of the chamber, by driving off the more subtle parts which some proclaim (*chante*) as so impossible. I already perceive that to elude the force of so many reasons and experiments, I shall be told that the instances I have produced, may indeed be proved in our gross and impure air, but that it would be otherwise in pure air if such can be found in nature: and, certainly I desire nothing better to induce me to cry victory. For what? do they believe that I imagine the Sieur Brun and the rest, who have found the increase of weight in question, have obtained a purer air by bills of exchange from beyond the confines of nature?

(To be continued)

ART. VIII. *A further Inquiry into the Nature of the Products of the slow Combustion of Ether.* *By* J. F. Daniell, F.R.S. *and* M.R.I.

SHORTLY after the publication of my first paper* upon the acid discovered by Sir Humphry Davy during his researches into the nature of flame, it was suggested to me by my friend Mr. Richard Phillips, that most of the properties of the salts described by me, as well as the equivalent number of the acid derived from their analysis, agreed very closely with those of the acetic acid. Although I must own that at the time I thought the evidence sufficiently conclusive against their identity, yet with the promise of his assistance I willingly undertook a further series of experiments upon the subject. The results

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* Journal, Vol. VI. p. 318.

I shall now as concisely as possible describe, as being in themselves interesting, and as furnishing rather a curious anecdote of the progress of the investigation.

I prepared a large quantity of the acid from sulphuric ether in the manner described in my former paper, making use of a lamp with three burners, which greatly accelerated the operation. The first product I rectified by drawing off from it at a low heat one-third of the quantity. This I did, to get rid of any ether with which it might have been contaminated, to the influence of which Mr. Phillips seemed to think some of the singular results which I had before obtained, might have been owing.

The liquor condensed from this distillation had not the slightest smell of sulphuric ether. It was inflammable, and burned with a pale blue flame, leaving but a small tasteless fluid residue. The *spirit* had a pungent, suffocating smell, and its taste was hot and rather acrid. It did not change the colour of litmus. It evaporated without leaving any residue, with a very gentle heat. Neither water, alcohol, nor ether produced any change in its appearance. When dropped into the solutions of muriate of gold and nitrate of silver, and warmed, it produced no precipitation of those metals.

The *acid* thus purified was intensely sour, of a peculiar pungent smell, which when received into the lungs, produced great irritation. Some pure acetic acid was procured, with which a small quantity of sulphuric ether was mixed, and with the two the following comparative experiments were made, with the assistance of Mr. Phillips in his laboratory:

Exp. 1.—Some solution of the muriate of gold was put into a test-tube, with a small quantity of the acid: upon the application of a very gentle heat the mixture became turbid, and appeared green by transmitted light. The gold was instantaneously reduced, and completely coated the inside of the glass. Some of the mixture of acetic acid and ether was likewise dropped into muriate of gold, but no change ensued even when strongly heated.

Exp. 2.—A little of the acid mixed with some solution of nitrate of silver produced an instantaneous turbidness. The mixture appeared blue by transmitted light, and the tube in which the experiment was made, was shortly lined with metallic silver. The mixture of acetic acid and ether produced no effect upon some of the same solution.

Exp. 3.—Some of the acid was dropped into a solution of muriate of platinum. When heated no change ensued. Upon examining the muriate of platinum it was found to be very acid. The excess was carefully neutralized with soda, when upon treating it as before a violent action took place, some of the liquid was thrown out of the tube, and the whole turned intensely black. Upon standing at rest for a few minutes the tube became coated with metallic particles of platinum, interspersed with a black powder not of a metallic appearance. The acetic acid and ether produced no effect upon the neutral solution.

Exp. 4.—Some of the acid was neutralized with carbonate of barytes: the solution was set to evaporate with a very gentle heat, but it underwent a certain degree of decomposition as it became very brown and pasty, and no crystals were obtained from it. For the sake of preserving the acid, the barytes was very carefully precipitated by dilute sulphuric acid. The sulphate of barytes was separated by the filter. After this process, the recovered acid still retained its property of reducing the metallic salts.

Exp. 5.—The acid was saturated with potash, and furnished, by very careful evaporation, long prismatic transparent crystals. The solution of this salt reduced the metallic solution with as much ease as the pure acid. No such effect was produced by acetate of potash.

Exp. 6.—Some peroxide of mercury, prepared from nitric acid, was put into a glass with some of the acid, and gently warmed. A very bulky white salt was instantly formed, which appeared to be nearly, if not wholly, insoluble in water. An equal quantity of the peroxide was treated in the same way with the mixture of acetic acid and ether, and a perfectly transparent colourless solution resulted, which was not at all altered by heat.

Exp. 7.—A quantity of peracetate of mercury was formed by dissolving peroxide of mercury, prepared from nitric acid, in acetic acid. Into a portion of this solution a small quantity of the acid was dropped, and, upon heating the mixture, an instantaneous precipitation of the bulky white salt obtained in Exp. 6 took place, in such a quantity that the whole mixture became solid, and the glass tube in which the experiment was made was upset without spilling a drop. It is hardly necessary to mention, that no such effect was produced by the mixture of acetic acid and ether.

After these experiments which were often repeated and varied in ways which it is unnecessary to detail, we thought that the distinct and peculiar nature of the acid formed during the slow combustion of ether was sufficiently established, and Mr. Phillips kindly requests me to state that he had no longer a doubt existing in his mind.

By a curious chance, I had no sooner satisfied others with regard to this subject, than I had reason to entertain doubts myself upon this very point. Being engaged in following up another branch of the subject in my own laboratory, (the object of our joint inquiry being considered accomplished,) I had occasion to prepare some peracetate of mercury. For this purpose I took some peroxide of mercury *per se*, and put it into acetic acid in a gentle heat. To my great surprise instead of dissolving entirely as the *nitrous* red oxide had always done, a large quantity of white insoluble salt was formed, exactly resembling that obtained in former experiments by the acid from ether.

Something of the true explanation of these apparently contradictory results soon occurred to me, and I immediately set about the following experiment to elucidate the matter.

Exp. 8.—Some protoxide of mercury was put into acetic acid and warmed, a spongy white salt of a silvery micaceous appearance was speedily formed, which was nearly insoluble in cold water. Some of the same protoxide was treated with the acid from ether, and an exactly similar salt resulted. Both products were carefully dried upon filtering paper, and set apart. At the end of a few days they were examined: the first was wholly unaltered in its appearance, but the second had assumed a grey hue, and was found upon examination with a lens, to be full of minute globules of revived mercury.

From hence it would appear that the acid formed during the slow combustion of ether is in fact the acetic, but combined with some substance of a highly disoxygenising nature, different from ether. In this manner we can account for the rapid reduction of the metallic oxides in the former experiments, and also for the instantaneous change of the soluble peracetate of mercury into the insoluble proto-acetate. In the latter case it is clear that the disoxygenating substance (whatever its nature may be,) took from the oxide of mercury in the peracetate one proportion of its oxygen, and precipitated the salt in the form of proto-acetate.

Upon this view of the subject it would seem to follow that the peroxide of mercury formed *per se* differed from the peroxide formed from nitric acid, in containing either combined or mixed a portion of protoxide. To bring this idea to the proof, I proceeded as follows.

Exp. 9.—Fifty grains of the *nitric* peroxide of mercury (red precipitate,) were put into pure muriatic acid. The whole dissolved readily, except a slight cloudiness. Fifty grains of the peroxide *per se* were treated in the same manner. A considerable quantity of grey insoluble matter remained, which being separated weighed 0.7 grains. It consisted chiefly of metallic mercury in a state of minute division, mixed with a very small portion of calomel. The quantity of this latter indicated, contrary to my expectation, but a very minute proportion of protoxide, but it is obvious that the metallic mercury during the process of solution in the acetic acid, combined with one proportion of the oxygen of the peroxide, forming thereby protoxide both by composition and decomposition. Why this should happen during the action of the acetic, and not during the action of the muriatic acid, is not perhaps at the first view of the subject quite clear, but I think upon consideration may be satisfactorily explained. In the former, a predisposing affinity exists between all the ingredients obviously tending to such an arrangement, resulting from the combined attraction of the mercury for oxygen, and of the acetic acid for the protoxide, but in the latter no such predisposing force can exist. When muriatic acid is put to the mixture of mercury and peroxide, a double decomposition takes place; the two proportions of oxygen in the oxide combine with two proportions of hydrogen derived from the acid, and water and a bichloride are formed, the metallic mercury remaining untouched: for, although we can readily enough conceive in the former instance that the play of affinities should produce the permanent arrangement of the proto-acetate of mercury, it is impossible to imagine that any predisposing affinity should determine the composition of protoxide of mercury, and at the same time its decomposition, as must have been the case if protochloride of mercury had been formed from the mixture of metal and peroxide; that is to say, protoxide of mercury must have been formed by the action of the metallic mercury upon the peroxide, merely for the purpose of being again decomposed by the agency of the acid, the whole arrangement being determined by the affinity of two substances which in point of fact did not exist.

I believe that it is the general opinion that the red precipitate is a more impure preparation of the peroxide of mercury than the oxide *per se;* but these experiments would seem to prove the contrary. Certain it is, that in medicine the former, which is of very low price, is not considered to answer the same purposes as the latter, which is a costly drug. Might not a due mixture of metallic mercury with the former assimilate the properties of the two? It is right to mention that the oxides which I employed were obtained from Messrs. Allen and Co. Plough-court, the accuracy of whose preparations are well known.

But to return to the immediate object of this paper. I endeavoured in various ways to separate the acetic acid, formed during the slow combustion of sulphuric ether, from the substance with which it appeared to be combined, so as to produce the latter in an isolated state, but without success. It appeared to undergo such rapid decomposition when exposed to but a moderate heat, that, combined at the same time with the speedy decomposition of most of the acetates, I could not effect my purpose. The salts which I obtained in a crystallized form held this substance in combination with them, so that their solution produced the same effects as the acid of which they were composed. It is owing to this circumstance that most of the salts, as I described in my former paper, burned with flame, and afterwards glowed like a live coal, and the results of the analysis which I made of the acid by means of the salt of barytes and chlorate of potash are explained, and the source of the excess of hydrogen clearly demonstrated. Thus the very same causes seemed to fix distinctive characters upon the lampic acid as contributed so long to separate the pyroligneous, the formic, and other modifications of the acetic acid.

Notwithstanding the failure of my endeavours to separate the substance of which I was in search, from its combinations I am enabled to throw considerable light upon its nature and formation by the discovery of an analogous compound, which is easily obtained in an isolated form. This compound is produced by the slow combustion of nitric ether.

Exp. 10.—When nitric ether is substituted for sulphuric in the aphlogistic lamp, the platinum wire glows as readily, but the fumes of the former are of a denser nature than those of the latter. They possess a more suffocating and more disagreeable smell, and are more inflammable, so that greater caution is requisite in adjusting the wire to prevent their breaking out into flame. After the process has been carried on a short time with the apparatus described in my former paper, an incrustation may be observed forming about the wire, a considerable quantity of liquid is condensed in the receiver; and by continuing the process, around the mouth, and upon the top of the alembic-head, is deposited a resinous-looking substance of a bright yellow colour. These products were collected and examined as follows:—

Exp. 11.—The liquid was purified from any spirituous or etherial mixture by careful distillation. The fluid condensed from this rectification was colourless, inflammable, and of a pungent very disagreeable smell. It was not affected by mixture with water, or alcohol, and did not affect the colour of litmus. The residual liquor was of a bright yellow colour, of a hot, acrid, and nauseous taste, and turned litmus paper red. Its smell was suffocating, and very unpleasant. It effected the reduction of the metals from their solutions in the same way as the lampic acid, but even more speedily. It instantly converted per-acetate of mercury into proto-acetate.

Exp. 12.—The resinous substance was highly inflammable. It burned rapidly with a hissing noise and scintillation, leaving a very bulky coal, such as is formed by the decomposition of a quill or other animal matter. It was soluble both in water and alcohol, but very sparingly in ether. Its taste was bitter, astringent, and very unpleasant.

Exp. 13.—The solutions of this matter produced the same effect upon the metallic salts as the distilled liquid; and when the acid of the latter was neutralised by an alkali, much of the resinous substance was obtained by gentle evaporation.

Exp. 14.—Its vapour, when heated, smelled very strongly of ammonia. A small piece of it was placed in a glass tube with a piece of turmeric paper, and upon the application of heat, the colour of the paper was turned to red.

Exp. 15.—It did not burn out of the contact of oxygen.

Exp. 16.—With the assistance of Mr. Faraday, I attempted to analyse this curious and interesting substance by means of per-oxide of copper. A quantity, weighing 1.8 grains, was taken, which was at first rather tough and tenacious. Being exposed to a very gentle heat, it lost in weight 0.2 grains, and became more hard and brittle. It was then triturated in a mortar, and well mixed with a large quantity of per-oxide of copper, and exposed to a red heat in an iron tube. The products of the decomposition were received over mercury. The quantity of gaseous matter, with all corrections made, amounted to 7.37 cubic inches. Upon examining the oxide of copper after the operation, it appeared that some of the carbon had escaped decomposition. It was, therefore, again triturated and mixed in a mortar, and returned into the tube. A further quantity of gas was thus obtained, amounting to 0.32 cubic inch. This added to the former produce, made the whole 7.69 cubic inches. This gaseous matter could not be fired with hydrogen, and produced no change in nitric oxide. Lime water was copiously precipitated by it. After these preliminary trials it was found that 6.23 cubic inches were absorbed by potash; and the residue, 1.46 cubic inch, possessed all the negative properties of azote. Thus the calculated results of the analysis stand

Grs.
6.23 inches carbonic acid = 0.79 carbon
1.46 ditto azote . . . = 0.43 azote
Loss . . . = 0.38 hydrogen
1.60

Now, the nearest definite proportions to these would be

Grs.	
0.75	carbon
0.43	azote
0.34	hydrogen
1.52	

Or upon the scale of equivalents,

4	proportions carbon	3.00
1	ditto azote	1.75
11	hydrogen	1.37

Which are further equivalent to

4	proportions of sub-carburetted hydrogen,	4.00
1	proportion of ammonia	2.12

To this singular compound, possessed of such distinctive characters, it will, no doubt, be deemed right to give a name, and I will venture to suggest that *Hydro-carburet of Azote,* will not inappropriately express its composition.

Exp. 17.—Some solution of muriate of platinum was taken, in which all excess of acid was neutralized carefully by soda. When the solution of hydro-carburet of azote was added to it, and gently warmed, a violent action, almost amounting to an explosion, took place; much of the mixture was thrown about, and a very black precipitate was formed. This consisted of metallic platinum, mixed with a large quantity of a deep black powder. The latter was separated on a filter, and gently warmed, to dry it. It had scarce parted with its moisture when it exploded with flame and noise, and nothing was left but reduced platinum.

Exp. 18.—A small lamp trimmed with nitric ether with the wire glowing, was placed under a bell-glass full of atmospheric air, and surrounded with water. A great absorption took place, amounting to one-fourth, when the lamp was extinguished. Upon examining the residual air and the water, the former was found to contain a large proportion of nitric oxide, and the latter of nitric acid. When nitric ether alone was placed in the same situation, an absorption, after some time, took place; and the water contained nitric acid, but no nitric oxide was formed. These experiments were repeated, with the same results, after having put some pieces of potash into the ether.

It was with a view of throwing some light upon the changes produced in the air by the slow combustion effected by the aphlogistic lamp, that the last experiments were instituted; but as this inquiry has led me into a wider field than I had at first anticipated, and in which I am still engaged, I shall reserve this further discussion for a future communication, and shall conclude this paper with a brief recapitulation of the principal facts established.

It appears, then, that the acid formed during the slow combustion of ether is the acetic, but combined with some compound of carbon and hydrogen, differing from ether or alcohol. In the product from sulphuric ether, it is less volatile, and more energetic in its action upon oxygenated bodies than ether. Owing to its easy decomposition, its point of volatilization, and ignorance of its peculiar affinities, it has not yet been separated from its concomitants, and exhibited in a distinct form.

In the products from nitric ether, this compound proves to be a triple combination of carbon, hydrogen, and azote, hitherto unknown. Its effects upon metallic oxides appear to be quite analogous to those of the compound produced from sulphuric ether, but it is easily separable from the other products. It forms a fulminating combination with platinum.

ART. IX.—*On some new Electro-Magnetical Motions, and on the Theory of Magnetism. By* M. Faraday, *Chemical Assistant in the Royal Institution.*

In making an experiment the beginning of last week, to ascertain the position of the magnetic needle to the connecting wire of a voltaic apparatus, I was led into a series which appear to me to give some new views of electro-magnetic action, and of magnetism altogether; and to render more distinct and clear those already taken. After the great men who have already experimented on the subject, I should have felt doubtful that any thing I could do could be new or possess an interest, but that the experiments seem to me to reconcile considerably the opposite opinions that are entertained on it. I am induced in consequence to publish this account of them, in the hope they will assist in making this important branch of knowledge more perfect.

The apparatus used was that invented by Dr. Hare of Philadelphia, and called by him a calorimotor; it is in fact a single pair of large plates, each having its power heightened by the induction of others. Consequently all the positions and motions of the needles, poles, *&c.*, are opposite to those produced by an apparatus of several plates; for, if a current be supposed to exist in the connecting wire of a battery from the zinc to the copper, it will be in each connected pair of plates from the copper to the zinc; and the wire I have used is that connection between the two plates of one pair. In the diagrams I may have occasion to subjoin the ends of the connecting wire, marked Z and C, are connected with the zinc and copper-plates respectively; the sections are all horizontal and seen from above, and the arrow-heads have been used sometimes to mark the pole of a needle or magnet which points to the north, and sometimes to mark the direction of motion; no difficulty can occur in ascertaining to which of those uses any particular head is applied.

On placing the wire perpendicularly, and bringing a needle towards it to ascertain the attractive and repulsive positions with regard to the wire; instead of finding these to be four, one attractive and one repulsive, for each pole, I found them to be eight, two attractive and two repulsive for each pole: thus allowing the needle to take its natural position across the wire, which is exactly opposite to that pointed out by Oersted for the reason before-mentioned, and then drawing the support away from the wire slowly, so as to bring the north pole, for instance, nearer to it, there is attraction, as is to be expected; but on continuing to make the end of the needle come nearer to the wire, repulsion takes place, though the wire still be on the same side of the needle. If the wire be on the other side of the same pole of the needle, it will repel it when opposite to most parts between the centre of motion and the end; but there is a small portion at the end where it attracts it. Fig. 1, plate iii, shews the positions of attraction for the north and south poles, fig. 2, the positions of repulsion.

If the wire be made to approach perpendicularly towards one pole of the needle, the pole will pass off on one side, in that direction which the attraction and repulsion at the extreme point of the pole would give; but, if the wire be continually made to approach the centre of motion, by either the one or other side of the needle, the tendency to move in the former direction diminishes; it then becomes null, and the needle is quite indifferent to the wire; and ultimately the motion is reversed, and the needle powerfully endeavours to pass the opposite way.

It is evident from this that the centre of the active portion of either limb of the needle, or the true pole, as it may be called, is not at the extremity of the needle, but may be represented by a point generally in the axis of the needle, at some little distance from the end. It was evident, also, that this point had a tendency to revolve round the wire, and necessarily, therefore, the wire round this point; and as the same effects in the opposite direction took place with the other pole, it was evident that each pole had the power of acting on the wire by itself, and not as any part of the needle, or as connected with the opposite pole.

By attending to fig. 3, which represents sections of the wire in its different positions to the needle, all this will be plain; the active poles are represented by two dots, and the arrow-heads shew the tendency of the wire in its positions to go round these poles.

Several important conclusions flow from these facts; such as that there is no attraction between the wire and either pole of a magnet; that the wire ought to revolve round a magnetic pole, and a magnetic pole round the wire; that both attraction and repulsion of connecting wires, and probably magnets, are compound actions; that true magnetic poles are centres of action induced by the whole bar, *&c. &c.* Such of these as I have been able to confirm by experiment, shall be stated, with their proofs.

The revolution of the wire and the pole round each other being the first important thing required to prove the nature of the force mutually exerted by them, various means were tried to succeed in producing it. The difficulty consisted in making a suspension of part of the wire sufficiently delicate for the motion, and yet affording sufficient mass of matter for contact. This was overcome in the following manner:—A piece of brass wire had a small button of silver soldered on to its end, a little cup was hollowed in the silver, and the metal being amalgamated, it would then retain a drop of mercury in it, though placed upside down for an upper centre of motion; for a lower centre, a similar cup was made of copper, into which a little mercury was put; this was placed in a jar of water under the former centre. A piece of copper wire was then bent into the form of a crank, its ends amalgamated, and the distances being arranged, they were placed in the cups. To prevent too much friction from the weight of the wire on the lower cup, it had been passed through a cork duly adjusted in size, and that being pushed down on the wire till immersed in the water, the friction became very little, and the wire very mobile yet with good contacts. The plates being then connected with the two cups, the apparatus was completed. In this state, a magnetic pole being brought to the centre of motion of the crank, the wire immediately made an effort to revolve until it struck the magnet, and that being rapidly brought round to the other side, the wire again made a revolution, giving evidence that it would have gone round continually but for the extension of the magnet on the outside. To do away with this impediment, the wire and lower metal cup were removed, and a deep basin of mercury placed beneath; at the bottom of this was a piece of wax, and a small round bar magnet was stuck upright in it, so that one pole was about half or three-fourths of an inch above the surface of the mercury, and directly under the silver cup. A straight piece of copper wire, long enough to reach from the cup, and dip about half an inch into the mercury, had its ends amalgamated, and a small round piece of cork fixed on to one of them to make it more buoyant; this being dipped in the mercury close beside the magnet, and the other end placed under the little cup, the wire remained upright, for the adhesion of the cork to the magnet was sufficient for that purpose, and yet at its lower end had freedom of motion round the pole. The connection being now made from the plates to the upper cup, and to the mercury below, the wire immediately began to revolve round the pole of the magnet, and continued to do so as long as the connexion was continued.

When it was wished to give a large diameter to the circle described by the wire, the cork was moved from the magnet, and a little loop of platinum passed round the magnet and wire, to prevent them from separating too far. Revolution again took place on making the connexion, but more slowly as the distance increased.

The direction in which the wire moved was according to the way in which the connexions were made, and to the magnetic pole brought into action. When the upper part of the wire was connected with the zinc, and the lower with the copper plate, the motion round the north and south poles of a magnet were as in fig. 4 and 5, looking from above; when the connexions were reversed, the motions were in the opposite direction.

On bringing the magnetic pole from the centre of motion to the side of the wire, there was neither attraction nor repulsion; but the wire endeavoured to pass off in a circle, still having the pole for its centre, and that either to the one side or the other, according to the above law.

When the pole was on the outside the wire, the wire moved in a direction directly contrary to that taken when the pole was in the inside; but it did not move far, the endeavour was still to go round the pole as a centre, and it only moved till that power and the power which retained it in a circle about its own axis were equipoised.

The next object was to make the magnet revolve round the wire. This was done by so loading one pole of the small magnet with platinum that the magnet would float upright in a basin of mercury, with the other pole above its surface; then connecting the mercury with one plate, and bringing a wire from the other perpendicularly into it in another part near the floating magnet: the upper pole immediately began to revolve round the wire, whilst the lower pole being removed away caused no interference or counteracting effect.

The motions were again according to the pole and the connexions. When the upper part of the wire was in contact with the zinc plate, and the lower with the copper, the direction of the curve described by the north and south poles were as in fig. 6 and 7. When the connexions were reversed, the motions were in the opposite directions.

Having succeeded thus far, I endeavoured to make a wire and a magnet revolve on their own axis by preventing the rotation in a circle round them, but have not been able to get the slightest indications that such can be the case; nor does it, on consideration, appear probable. The motions evidently belong to the current, or whatever else it be, that is passing through the wire, and not to the wire itself, except as the vehicle of the current. When that current is made a curve by the form of the wire, it is easy to conceive how, in revolving, it should take the wire with it; but when the wire is straight, the current may revolve without any motion being communicated to the wire through which it passes.

M. Ampere has shewn that two similar connecting wires, by which is meant, having currents in the same direction through them, attract each other; and that two wires having currents in opposite directions through them, repel each other; the attraction and repulsion taking place in right lines between them. From the attraction of the north pole of a needle on one side the wire and of the south on the other, and the repulsion of the poles on the opposite sides, Dr. Wollaston called this magnetism vertiginous, and conceived that the phænomena might 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, and exhibiting north and south powers on the opposite sides. It is, indeed, an ascertained fact, that the connecting wire has different powers at its opposite sides; or rather each power continues all round the wire, the direction being the same; and hence it is evident that the attractions and repulsions of M. Ampere's wires are not simple, but complicated results.

A simple case which may be taken of magnetic motion, is the circle described by the wire or the pole round each other. If a wire be made into a helix, as M. Ampere describes, the arrangement is such that all the vertiginous magnetism, as Dr. Wollaston has named it, of the one kind, or one side of the wire, is concentrated in the axis of the helix, whilst the contrary kind is very much diffused, *i. e.*, the power exerted by a great length of wire to make a pole pass one way round it, all tends to carry that pole to a particular spot, whilst the opposite power is diffused and much weakened in its action on any one pole. Hence the power on one side of the wire is very much concentrated, and its particular effects brought out strongly, whilst that on the other is rendered insensible. A means is thus obtained of separating, as it were, the one power from the other: but when this is done, and we examine the end of the helix, it is found very much to resemble a magnetic pole; the power is concentrated at the extremity of the helix; it attracts or repels one pole in all directions; and I find that it causes the revolution of the connecting wire round it, just as a magnetic pole does. Hence it may, for the present, be considered identical with a magnetic pole; and I think that the experimental evidence of the ensuing pages will much strengthen that opinion.

Assuming, then, that the pole of a magnetic needle presents us with the properties of one side of the wire, the phenomena it presents with the wire itself, offers us a means of analysis which, probably, if well pursued, will give us a much more intimate knowledge of the state of the powers active in magnets. When it is placed near the wire, always assuming the latter to be connected with the battery, it is made to revolve round it, passing towards that side by which it is attracted, and from that side by which it is repelled, *i. e.*, the pole is at once attracted and repelled by equal powers, and therefore neither recedes or approaches; but the powers being from opposite sides of the wire, the pole in its double effort to recede from one side and approach the other revolves in the circle, that circle being evidently decided by the particular pole and state of the wire, and deducible from the law before mentioned.

The phenomena presented by the approximation of one pole to two or more wires, or two poles to one or more wires, offer many illustrations of this double action, and will lead to more correct views of the magnet. These experiments are easily made by loading a needle with platinum at one pole, that the other may float above mercury, or by almost floating a small magnetic needle by cork in a basin of water, at the bottom of which is some mercury with which to connect the wires. In describing them I shall refrain from entering into all their variations, or pursuing them to such conclusions as are not directly important.

Two similar wires, Ampere has shewn, attract each other; and Sir H. Davy has shewn that the filings adhering to them attract from one to another on the same side. They are in that position in which the north and south influence of the different wires attract each other. They seem also to neutralize each other in the parts that face, for the magnetic pole is quite inactive between them, but if put close together, it moves round the outside of both, circulating round them as round one wire, and their influences being in the same direction, the greatest effect is found to be at the farther outside surfaces of the wires. If several similar wires be put together, side by side like a ribbon, the result is the same, and the needle revolves round them all; the internal wires appear to lose part of their force, which is carried on towards the extreme wire in opposite directions, so that the floating pole is accelerated in its motion as it passes by the edges that they form. If, in place of a ribbon of parallel wires, a slip of metal be used, the effect is the same, and the edges act as if they contained in a concentrated state the power that belonged to the inner portion of the slip. In this way we procure the means of removing, as it were, in that direction, the two sides of the wire from each other.

If two wires in opposite states be arranged parallel to each other, and the pole be brought near them, it will circulate round either of them in obedience to the law laid down; but as the wires have opposite currents, it moves in opposite directions round the two, so that when equidistant from them, the pole is propelled in a right line perpendicular to the line which joins them, either receding or approaching; and if it approaches, passing between and then receding: hence it exhibits the curious appearance of being first attracted by the two wires, and afterwards repelled. (Fig. 8.) If the connexion with both wires be inverted, or if the pole be changed, the line it describes is in the opposite direction. If these two opposite currents be made by bending a piece of silked wire parallel to itself, fig. 9, it, when connected with the apparatus, becomes a curious magnet; with the north pole, for instance, it attracts powerfully on one side at the line between the two currents, but repels strongly to the right or left; whilst on the other side the line repels the north pole, but attracts it strongly to the right or left. With the south pole the attractions and repulsions are reversed.

When both poles of the needle were allowed to come into action on the wire or wires, the effects were in accordance with those described. When a magnetic needle was floated on water, and the perpendicular wire brought towards it, the needle turned round more or less, until it took a direction perpendicular to, and across the wire, the poles being in such positions that either of them alone would revolve round the wire in a circle proceeding by the side to which it had gone, according to the law before stated. The needle then approaches to the wire, its centre (not either pole) going in a direct line towards it. If the wire be then lifted up and put down on the other side the needle, the needle passes on in the same line receding from the wire, so that the wire seems here to be both attractive and repulsive of the needle. This effect will be readily understood from fig. 10, where the poles and direction of the wire is not marked, because they are the same as before. If either be reversed, the others reverse themselves. The experiment is analogous to the one described above; there the pole passed between two dissimilar wires, here the wire between two dissimilar poles.

If two dissimilar wires be used, and the magnet have both poles active, it is repelled, turned round, or is attracted in various ways, until it settles across between the two wires; all its motions being easily reducible to those impressed on the poles by the wires, both wires and both poles being active in giving that position. Then if it happens not to be midway between the two, or they are not of equal power, it goes slowly towards one of them, and acts with it just as with the single wire of the last paragraph.

Fig. 11 and 12 exhibit more distinctly the direction of the forces which influence the poles in passing between two dissimilar wires: fig. 11, when the pole draws up between the wires; fig. 12, the pole thrown out from between them. The poles and state of the wire are not marked, because the diagrams illustrate the attraction and repulsion of both poles: for any particular pole, the connexion of the wires must be accordingly.

If one of the poles be brought purposely near either wire in the position in which it appears to attract most strongly, still if freedom of motion be given by a little tapping, the needle will slip along till it stands midway across the wire.

A beautiful little apparatus has been made by M. de la Rive, to whom I am indebted for one of them, consisting of a small voltaic combination floating by a cork; the ends of the little zinc and copper slips come through the cork, and are connected above by a piece of silked wire which has been wrapped four or five times round a cylinder, and the wires tied together with a silk thread so as to form a close helix about one inch in diameter. When placed on acidulated water it is very obedient to the magnet, and serves admirably to transform, as it were, the experiments with straight wires that have been mentioned, to the similar ones made with helices. Thus, if a magnet be brought near it and level with its axis, the apparatus will recede or turn round until that side of the curve next to the nearest pole is the side attracted by it. It will then approach the pole, pass it, recede from it until it gains the middle of the magnet, where it will rest like an equator round it, its motions and position being still the same as those before pointed out. (Fig. 13.) If brought near either pole it will still return to the centre; and if purposely placed in the opposite direction at the centre of the magnet, it will pass off by either pole to which it happens to be nearest, being apparently first attracted by the pole and afterwards repelled, as is actually the case; will, if any circumstance disturbs its perpendicularity to the magnet, turn half way round; and will then pass on to the magnet again, into the position first described. If, instead of passing the magnet through the curve, it be held over it; it stands in a plane perpendicular to the magnet, but in an opposite direction to the former one. So that a magnet, both within and without this curve, causes it to direct.

When the poles of the magnet are brought over this floating curve, there are some movements and positions which at first appears anomalous, but are by a little attention easily reducible to the circular movement of the wire about the pole. I do not think it necessary to state them particularly.

The attractive and repulsive positions of this curve may be seen by fig. 13, the curve in the two dotted positions is attracted by the poles near them. If the positions be reversed, repulsion takes place.

From the central situation of the magnet in these experiments, it may be concluded that a strong and powerful curve or helix would suspend a powerful needle in its centre. By making a needle almost float on water and putting the helix over a glass tube, this result has in part been obtained.

In all these magnetic movements between wires and poles, those which resemble attraction and repulsion, that is to say, those which took place in right lines, required at least either two poles and a wire, or two wires and a pole; for such as appear to exist between the wire and either pole of the battery, are deceptive and may be resolved into the circular motion. It has been allowed, I believe, by all who have experimented on these phenomena, that the similar powers repel and the dissimilar powers attract each other; and that, whether they exist in the poles of magnets or in the opposite sides of conducting wires. This being admitted, the simplest cases of magnetic action will be those exerted by the poles of helices, for, as they offer the magnetic states of the opposite sides of the wire independent, or nearly so, one of the other, we are enabled by them to bring into action two of those powers only, to the exclusion of the rest; and, from experiment it appears that when the powers are similar, repulsion takes place, and when dissimilar, attraction; so that two cases of repulsion and one of attraction are produced by the combination of these magnetic powers *.

The next cases of magnetic motion, in the order of simplicity, are those where three powers are concerned or those produced by a pole and a wire. These are the circular motions described in the early part of this paper. They resolve themselves into two, a north pole and the wire round each other, and a south pole, and the wire round each other. The law which governs these motions has been stated.

Then follow the actions between two wires, these when similarly electrified attract as M. Ampere has shewn; for then the opposite sides are towards each other, and the four powers all combine to draw the currents together forming a double attraction; but, when the wires are dissimilar they repel, because, then on both sides the wire the same powers are opposed, and cause a double repulsion.

The motions that result from the action of two dissimilar poles and a wire next follow: the wire endeavours to describe opposite circles round the poles; consequently, it is carried in a line passing through the central part of the needle in which they are situated. If the wire is on the side on which the circles close together, it is attracted; if on the opposite side, from whence the circles open, it is repelled, fig. 10.

The motions of a pole with two wires are almost the same as the last; when the wires are dissimilar, the pole endeavours to form two opposite circles about the wires; when it is on that side the wires on which the circles meet, it is attracted; when on the side on which they open, it is repelled, figs. 8, 11, 12.

Finally, the motion between two poles and two dissimilar wires, is an instance where several powers combine to produce an effect.

M. Ampere, whilst reasoning on the discovery of M. Oersted, was led to the adoption of a theory, by which he endeavoured to account for the properties of magnets, by the existence of

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* This is perhaps not strictly true, because, though the opposite powers are weakened, they still remain in action.

concentric currents of electricity in them, arranged round the axis of the magnet. In support of this theory, he first formed the spiral or helix wire, in which currents could be made to pass nearly perpendicular to, and round the axis of a cylinder. The ends of such helices were found, when connected with the voltaic apparatus to be in opposite magnetic states, and to present the appearance of poles. Whilst pursuing the mutual action of poles and wires, and tracing out the circular movements, it seemed to me that much information respecting the competency of this theory might be gained from an attempt to trace the action of the helix, and compare it with that of the magnet more rigorously than had yet been done; and to form artificial electro-magnets, and analyze natural ones. In doing this, I think I have so far succeeded as to trace the action of an electro-magnetic pole, either in attracting or repelling, to the circulating motion before described.

If three inches of connecting wire be taken, and a magnetic pole be allowed to circulate round the middle of it, describing a circle of a little less than one inch in diameter, it will be moved with equal force in all parts of the circle, fig. 14; bend then the wire into a circle, leaving that part round which the pole revolves perpendicularly, undisturbed, as seen by the dotted lines, and make it a condition that the pole be restrained from moving out of the circle by a radius. It will immediately be evident that the wire now acts very differently on the pole in the different parts of the circle it describes. Every part of it will be active at the same time on the pole, to make it move through the centre of the wire ring, whilst as it passes away from that position the powers diverge from it, and it is either removed from their action or submitted to opposing ones, until on its arriving at the opposite part of the circle it is urged by a very small portion indeed of those which moved it before. As it continues to go round, its motion is accelerated, the forces rapidly gather together on it, until it again reaches the centre of the wire ring where they are at their highest, and afterwards diminish as before. Thus the pole is perpetually urged in a circle, but with powers constantly changing. If the wire ring be conceived to be occupied by a plane, then the centre of that plane is the spot where the powers are most active on the pole, and move it with most force. Now this spot is actually the pole of this magnetic apparatus. It seems to have powers over the circulating pole, making it approach or attracting it on the one side, and making it recede or repelling it on the other, with powers varying as the distance; but its powers are only apparent, for the force is in the ring, and this spot is merely the place where they are most accumulated: and though it seems to have opposite powers, namely, those of attracting and repelling; yet this is merely a consequence of its situation in the circle, the motion being uniform in its direction, and really and truly impressed on the pole by its motor, the wire.

At page 81, it was shewn that two or more similar wires put together in a line, acted as one; the power being, as it were, accumulated towards the extreme wires, by a species of induction taking place among them all: and at the same time was noticed the similar case of a plate of metal connecting the ends of the apparatus, its powers being apparently strongest at the edges. If, then, a series of concentric rings be placed one inside the other, they having the electric current sent through them in the same direction; or if, which is the same thing, a flat spiral of silked wire passing from the centre to the circumference be formed, and its ends be in connexion with the battery, fig. 15, then the circle of revolution would still be as in fig. 14, passing through the centre of the rings or spiral, but the power would be very much increased. Such a spiral, when made, beautifully illustrates this fact; it takes up an enormous quantity of iron filings, which approach to the form of cones, so strong is the action at the centre; and its action on the needle by the different sides, is eminently powerful.

If in place of putting ring within ring, they be placed side by side, so as to form a cylinder, or if a helix be made, then the same kind of neutralization takes place in the intermediate wires, and accumulated effect in the extreme ones, as before. The line which the pole would now travel, supposing the inner end of the radius to move over the inner and outer surface of the cylinder, would be through the axis of the cylinder round the edge to one side, back up that side, and round to the axis, down which it would go, as before. In this case the force would probably be greatest at the two extremes of the axis of the cylinder, and least at the middle distance on the outside.

Now consider the internal space of the cylinder filled up by rings or spirals, all having the currents in the same direction: the direction and kind of force would be the same, but very much strengthened: it would exist in the strongest degree down the axis of the mass, because of the circular form, and it would have the two sides of the point in the centre of the simple ring, which *seemed* to possess attractive and repulsive powers on the pole, removed to the ends of the cylinder; giving rise to two points, apparently distinct in their action, one being attractive, and the other repulsive, of the poles of a magnet. Now conceive that the pole is not confined to a motion about the sides of the ring, or the flat spiral, or cylinder, it is evident that if placed in the axis of any of them at a proper distance for action, it, being impelled by two or more powers in equal circles, would move in a right line in the intersection of those circles, and approach directly to, or recede from, the points before spoken of, giving the appearance of a direct attraction and repulsion: and if placed out of that axis, it would move towards or from the same spot in a curve line, its direction and force being determined by the curve lines representing the active forces from the portions of wire forming the ends of the cylinder, spiral, or ring, and the strength of those forces.

Thus the phenomena of a helix, or a solid cylinder of spiral silked wire, are reduced to the simple revolution of the magnetic pole round the connecting wire of the battery, and its resemblance to a magnet is so great, that the strongest presumption arises in the mind they both owe their powers, as M. Ampere has stated, to the same cause. Filings of iron sprinkled on paper held over this cylinder, arranged in curved lines passing from one end to the other, shewing the path the pole would follow, and so they do over a magnet; the ends attract and repel as do those of a magnet; and in almost every point do they agree. The following experiments will illustrate and confirm the truth of these remarks on the action of the ring, helix, or cylinder; and will shew in what their actions agree with, and differ (for there are differences) from, the action of a magnet.

A small magnet being nearly floated in water by cork, a ring of silked copper wire, fig. 16, having its ends connected with the battery, was brought near its poles in different positions; sometimes the pole was repelled from, sometimes attracted into, the ring, according to the position of the pole, and the connexions with the battery. If the wire happened to be opposite to the pole, the pole passed sideways, and outwards when it was repelled, and sideways and inwards when it was attracted; and on entering within the ring and passing through, it moved sideways in the opposite direction, endeavouring to go round the wire. The actions also presented by M. de la Rive's ring are actions of this kind, and indeed are those which best illustrate the relations between the ring and the pole; some of them have been mentioned, and if referred to, will be found to accord with the statement given.

With a flat spiral the magnetic power was very much increased; and when the rings were not continued to the centre, the power of the inner edge over the outer was well shewn either by the pole of a needle, or iron filings. With the latter, the appearance was extremely beautiful and instructive; when laid flat upon a heap of them, they arranged themselves in lines, passing through the ring parallel to its axis, and then folding up on either side as radii round to the edge, where they met; so that they represented, exactly, the lines which a pole would have described round the sides of the rings: and those filings which were in the axis of the rings, stood up in perpendicular filaments, half an inch long and so as to form an actual axis to the ring, tending neither one way nor the other, but according in their form and arrangement with what has been described; whilst the intermediate portion also formed long threads, bending this way and that from the centre, more or less, according as they were further from, or nearer to, it.

With a helix the phenomena were interesting, because according to the view given of the attractions and repulsions, that is of the motions toward and from the ends, some conclusions should follow, that if found to be true in fact, and to hold also with magnets, would go far to prove the identity of the two. Thus the end which seems to attract a certain pole on the outside, ought to repel it as it were on the inside, and that which seems to repel it on the outside, ought to appear to attract it on the inside; *i. e.*, that as the motions on the inside and outside are in different directions for the same pole, it would move in the one case to and in the other case from the same end of the helix. Some phenomena of this kind have been described in explaining figs. 8, 11, 12, and 13; others are as follows.

A helix of silked copper wire was made round a glass tube, the tube being about an inch in diameter; the helix was about three inches long. A magnetic needle nearly as long was floated with cork, so as to move about in water with the slightest impulse; the helix being connected with the apparatus and put into the water in which the needle lay, its ends appeared to attract and repel the poles of the needle according to the laws before-mentioned. But, if that end which attracted one of the poles of the needle was brought near that pole, it entered the glass tube, but did not stop just within side in the neighbourhood of this pole (as we may call it for the moment) of the helix, but passed up the tube, drawing the whole needle in, and went to the opposite pole of the helix, or the one which on the outside would have repelled it. On trying the other pole of the magnet with its corresponding end or pole of the helix the same effect took place; the needle-pole entered the tube and passed to the other end, taking the whole needle into the same position it was in before.

Thus each end of the helix seemed to attract and repel both poles of the needle: but this is only a natural consequence from the circulating motion before experimentally demonstrated, and each pole would have gone through the helix and round on the outside, but for the counteraction of the opposite pole.

It has been stated that the poles circulate in opposite directions round the wires, and they would consequently circulate in opposite directions through and round the helix; when, therefore, one end of the helix was near that pole, which would, according to the law stated, enter it and endeavour to go through, it would enter, and it would continue its course until the other pole, at first at a distance, would be brought within action of the helix; and, when they were both equally within the helix and consequently equally acted on, their tendency to go in different directions would counterbalance each other, and the needle would remain motionless. If it were possible to separate the two poles from each other, they would dart out of each end of the helix, being apparently repelled by those parts that before seemed to attract them, as is evident from the first and many other experiments.

By reversing the needle and placing it purposely in the helix in that position, the poles of the needle and the corresponding poles of the helix as they attract on the outside, are brought together on the inside, but both pairs now seem to repel; and, whichever end of the helix the needle happens to be nearest to, it will be thrown out at. This motion may be seen to exhibit in its passing state, attraction between similar poles, since the inner and active pole is drawn towards that end on the inside, by which it is thrown off on the outside *.

These experiments may be made with the single curve of M. de la Rive, in which case it is the wire that moves and not the magnet, but as the motions are reciprocal, they may be readily anticipated.

A plate of copper was bent nearly into a cylinder, and its edges made to dip into two portions of mercury; when placed in a current it acted exactly as a helix.

A solid cylinder of silked wire was made exactly in fashion like a helix, but that one length of the wire served as the axis, and the folds were repeated over and over again. This

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* The magnetizing power of the helix is so strong that if the experiment be made slowly the needle will have its magnetism changed, and the result will be fallacious.

as well as the former helix, had poles the same in every respect as to kind as the north and south poles of a magnet; they took up filings, they made the connecting wire revolve, they attracted and repelled in four parallel positions as is described of common magnets in the first pages of this paper, and filings sprinkled on paper over them, formed curves from one to the other as with magnets; these lines indicating the direction in which a north or south pole would move about them.

Now with respect to the accordance which is found between the appearances of a helix or cylinder when in the voltaic circuit, and a cylindrical common magnet, or even a regular square bar magnet; it is so great, as at first to leave little doubt, that whatever it is that causes the properties of the one, also causes the properties of the other, for the one may be substituted for the other in, I believe, every magnetical experiment: and, in the bar magnet, all the effects on a single pole or filings, *&c.*, agree with the notion of a circulation, which if the magnet were not solid would pass through its centre, and back on the outside.

The following, however, are differences between the appearances of a magnet and those of a helix or cylinder: one pole of a magnet attracts the opposite pole of a magnetic needle in all directions and positions; but when the helix is held along-side the needle nearly parallel to it, and with opposite poles together, so that attraction should take place, and then the helix be moved on so that the pole of the needle gradually comes nearer to the middle of the helix, repulsion generally takes place before the pole gets to the middle of the helix, and in a situation where with the magnet it would be attracted. This is probably occasioned by the want of continuity in the sides of the curves or elements of the helix, in consequence of which the unity of action which takes place in the rings into which a magnet may be considered to be divided is interfered with and disturbed.

Another difference is that the poles, or those spots to which the needle points when perpendicular to the end or sides of a magnet or helix, and where the motive power may be considered perhaps as most concentrated, is in the helix at the extremity of its axis, and not any distance in from the end; whilst in the most regular magnets it is almost always situate in the axis at some distance in from the end: a needle pointing perpendicularly towards the end of a magnet is in a line with its axis, but perpendicularly to the side it points to a spot some distance from the end, whilst in the helix, or cylinder, it still points to the end. This variation is, probably, to be attributed to the distribution of the exciting cause of magnetism in the magnet and helix. In the latter, it is necessarily uniform every where, inasmuch as the current of electricity is uniform. In the magnet it is probably more active in the middle than elsewhere: for as the north pole of a magnet brought near a south one increases its activity, and that the more as it is nearer, it is fair to infer that the similar parts which are actually united in the inner part of the bar, have the same power. Thus a piece of soft iron put to one end of a horse-shoe magnet, immediately moves the pole towards that end; but if it be then made to touch the other end also, the pole moves in the opposite direction, and is weakened; and it moves the farther, and is made weaker as the contact is more perfect. The presumption is, that if it were complete, the two poles of the magnet would be diffused over the whole of its mass, the instrument then exhibiting no attractive or repulsive powers. Hence it is not improbable that, caused by some induction, a greater accumulation of power taking place in the middle of the magnet than at the end, may cause the poles to be inwards, rather than at the extremities.

A third difference is, that the similar poles of magnets, though they repel at most distances, yet when brought very near together, attract each other. This power is not strong, but I do not believe it is occasioned by the superior strength of one pole over the other, since the most equal magnets exert it, and since the poles as to their magnetism remain the same, and are able to take up as much, if not more, iron filings when together, as when separated; whereas opposite poles, when in contact, do not take up so much. With similar helix poles, this attraction does not take place.

The attempts to make magnets resembling the helix and the flat spirals, have been very unsuccessful. A plate of steel was formed into a cylinder and then magnetized, one end was north all round, the other south; but the outside and the inside had the same properties, and no pole of a needle would have gone up the axis and down the sides, as with the helix, but would have stopped at the dissimilar pole of the needle. Hence it is certain, that the rings of which the cylinder may be supposed to be formed, are not in the same state as those of which the helix was composed. All attempts to magnetize a flat circular plate of steel, so as to have one pole in the centre of one side, and the other pole in the centre of the opposite side, for the purpose of imitating the flat spiral, fig. 15, failed; nothing but an irregular distribution of the magnetism could be obtained.

M. Ampere is, I believe, undecided with regard to the size of the currents of electricity that are assumed to exist in magnets, perpendicular to their axis. In one part of his memoirs they are said, I think, to be concentric; but this cannot be the case with those of the cylinder magnet, except two be supposed in opposite directions, the one on the inside, the other on the outside surface. In another part, I believe, the opinion is advanced that they may be exceedingly small; and it is, perhaps, possible to explain the case of the most irregular magnet by theoretically bending such small currents in the direction required.

In the previous attempt to explain some of the electro-magnetic motions, and to shew the relation between electro and other magnets, I have not intended to adopt any theory of the cause of magnetism, nor to oppose any. It appears very probable that in the regular bar magnet, the steel, or iron, is in the same state as the copper wire of the helix magnet; and, perhaps, as M. Ampere supports in his theory, by the same means, namely, currents of electricity; but still other proofs are wanting of the presence of a power like electricity than the magnetic effects only. With regard to the opposite sides of the connecting wire, and the powers emanating from them, I have merely spoken of them as two, to distinguish the one set of effects from the other. The high authority of Dr. Wollaston is attached to the opinion that a single electro-magnetic current passing round the axis of the wire in a direction determined by the position of the voltaic poles, is sufficient to explain all the phenomena.

M. Ampere, who has been engaged so actively in this branch of natural philosophy, drew from his theory, the conclusion that a circular wire forming part of the connexion between the poles of the battery, should be directed by the earth's magnetism, and stand in a plane perpendicular to the magnetic meridian and the dipping needle. This result was said to be actually obtained, but its accuracy has been questioned, both on theoretical and experimental grounds. As the magnet directs the wire when in form of a curve, and the curve a needle, I endeavoured to repeat the experiment, and succeeded in the following manner:—A voltaic combination of two plates was formed, which were connected by a copper wire, bent into a circular form; the plates were put into a small glass jar with dilute acid, and the jar floated on the surface of water: being then left to itself in a quiet atmosphere, the instrument so arranged itself that the curve was in a plane perpendicular to the magnetic meridian; when moved from this position, either one way or the other, it returned again; and on examining the side of the curve towards the north, it was found to be that, which, according to the law already stated, would be attracted by a south pole. A voltaic circle made in a silver capsule, and mounted with a curve, also produced the same effect; as did likewise, very readily, M. de la Rive's small ring apparatus*. When placed on acidulated water, the gas liberated from the plates prevented its taking up a steady position; but when put into a little floating cell, made out of the neck of a Florence flask, the whole readily took the position mentioned above, and even vibrated slowly about it.

As the straight connecting wire is directed by a magnet, there

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* See Miscellanea.

is every reason to believe that it will act in the same way with the earth, and take a direction perpendicular to the magnetic meridian. It also should act with the magnetic pole of the earth, as with the pole of a magnet, and endeavour to circulate round it. Theoretically, therefore, a horizontal wire perpendicular to the magnetic meridian, if connected first in one way with a voltaic battery, and then in the opposite way, should have its weight altered; for in the one case it would tend to pass in a circle downwards, and in the other upwards. This alteration should take place differently in different parts of the world. The effect is actually produced by the pole of a magnet, but I have not succeeded in obtaining it, employing only the polarity of the earth.

Sept. 11, 1821.

ART. X. *Letter from* Dr. Hastings *to the* Editor, *respecting the Division of the Eighth pair of Nerves.*

SIR,

As the accuracy of my experiments on the par vagum, contained in a former Number of your *Journal,* has been established by those since conducted at the Royal Institution, I should not add one sentence to what has been already written on the subject, had not Mr. Broughton in his reply made personal allusions, which compel me to state,

That my observations were made in consequence of the accusation of inaccuracy, brought against me by that gentleman; and that I have again, with great care, compared them with his *first* paper, and can perceive no instance in which they are not fully applicable to it. In what part of Mr. Broughton's first paper does he say, that the contents of the stomachs of the rabbits operated on, were compared with those of the stomachs of healthy rabbits, similarly fed, and killed at the same time? where does he say that the eighth pair of nerves in the dog were found after death divided, or that he took care to place the animal in such a situation, that he must have observed it, if it had vomited any part of the milk? where does he say that he found the ends of the nerves uniformly apart from each other? what change does he mention in what he calls digested parsley, except change of colour? and does he not talk of the chyme of the rabbit resembling mucus, and being often found in greatest quantity in the cardiac portion of the stomach?

I am sorry that the discussion of this question, which it has been my wish to divest of all personality, should have produced any angry feelings in Mr. Broughton's mind; and remain,

Sir, Your obedient Servant,
CHARLES HASTINGS.

Worcester, July 18*th*, 1821.

ART. XI. *Meteorological Observations. By* J. F. DANIELL, Esq., F.R.S., *and* M.R.I.

I HAVE now completed the second year's observations with my hygrometer and the instruments connected with it, and propose, in this paper, to give a condensed abstract of their results. I have ceased to publish, quarterly, the Meteorological Journal at length, from a conviction that its utility, in such a state, is very doubtful, and that it would unprofitably occupy the room of more important or entertaining matter. Mr. Howard, in his late laborious work upon the climate of London, has shewn the proper use to be made of such accumulations of facts; it is only by collecting the means of different seasons, thereby neutralizing errors of observation and accidental irregularities, by arranging them in periods chosen according to the influence of particular circumstances, and by carefully collating and comparing them, however tedious the operation, that we can hope to arrive at certain consequences and useful results from the preliminary labour. Many, indeed, appear, at present, to be the observers of meteorological phenomena, to judge by the registers which regularly contribute their expletive powers to every magazine and journal which issue from the press; but, for want of proper direction and concert, their perseverance, it is to be feared, is wholly fruitless. The observers themselves rarely attempt to reason upon their observations, and no one will ever be found willing to sacrifice his time in arranging volumes of materials, when the very accuracy of the instruments with which they have been collected, may be doubted. But who can wonder at this want of co-operation, or to whom shall we look to turn this labour into a useful channel, when even the correctness of the tables published in the Transactions of the Royal Society, has been publicly called in question by Mr. Howard, Mr. Dalton, and Dr. Thomson? The parade of such observations, when confidence in their care has once been suspended, is worse than useless;—it is injurious to science.

Mr. Howard, in my humble judgment, has done more for the advancement of meteorology than all his predecessors or contemporaries, by his careful digest of observations contained in his admirable, and, I may add, entertaining, work. He has not only collected and arranged stores from which all future cultivators of the science must be content to draw, but has traced the path of useful application. At an humble distance I shall endeavour to tread in his footsteps; my only motive for holding the same course being a wish to establish the use of an instrument, which my own experience, as well as the acknowledgment of others, assure me, is entitled to be considered as a standard. Had Mr. Howard followed up the public approbation with which he has been pleased to honour my hygrometer by its adoption in practice, I should most willingly have surrendered into his abler hands the labour of observation; but as no one has yet undertaken to record its indications, I shall sedulously, if painfully, persevere, although the necessary division of my time is one, amongst other causes, which unfit me for the task.

With regard to the construction of the instrument, I have made one alteration which facilitates the observation. The condensation ball is now formed of black or other deep-coloured glass, and the dew is seen by reflected light, as upon a mirror. The most careless person may thus seize the exact moment of deposition with certainty; and, as no previous practice is necessary, the improvement may, probably, contribute to render it more popular.

The form in which I have extracted the observations from my register, the times of observing, and the division of the periods, are the first things that require remark. I have constructed three tables, which will be found to contain the greater part of the results necessary for our purpose. The first is similar to this one for the preceding year, published in the tenth volume of the *Journal*, p. 131. It contains the means of the different periods in large figures, and extremes in smaller, shewing the range of the several instruments in the respective intervals. The division of the year is into halves, quarters, and half-quarters, which I have found to be convenient and comprehensive. The grouping of the quarters represents Autumn, Winter, Spring and Summer; and agrees with that adopted by Mr. Howard, except that he commences each quarter with the second, instead of the first, week of the month. The reasons which he has assigned for departure from the customary division, are conclusive; mine, which was made before I had any knowledge of his work, approaches sufficiently near to excuse me from the laborious drudgery (not easily appreciated but by those who have undertaken such a task,) of recalculating the results. The commencing with Autumn was accidental. It was with this season that I began first to observe regularly, and of course at this season the first year ended. If this irregularity should be thought inconvenient, from the nature of the division, it may be easily rectified. The hours of the day at which the observations are made, are a consideration of greater moment; they are, nine in the morning, five in the afternoon, and eleven at night; very rarely, indeed, varying an hour from the appointed time. The maximum and minimum of temperature, by a register thermometer, may be added to these. If the selection of three periods in the day had been guided merely by meteorological considerations, I do not mean to say that these would have been the most proper; I think not: but it is not a question that is easily determined. They are not, however, destitute of peculiar advantages. The first object of repeated meteorological observations is to obtain accurate mean results. The most proper method of effecting this, even for the temperature, where we are assisted by an instrument which marks the changes in the absence of the observer, is still a subject of some dispute. The most we can hope for is a near approximation. Now the results of the barometer contained in the first column of Table 1, and those of the hygrometer in the second, third, and fourth columns, must be regarded first as the means of the particular hours at which the respective observations were made. It is further probable, that the former may be taken, without much fear of serious error, as the medium of the whole 24 hours. No particular known cause tends to prevent this assumption. But with regard to the latter, it is very different. The fall of the temperature during the night must exercise a very important influence upon the atmospheric vapour; and therefore the results of the second, third, and fourth columns, cannot be received as those of the 24 hours, without further inquiry.

The register thermometer has been the means of our attaining to a much greater degree of certainty with regard to temperature than we could ever have hoped to have arrived at by any other method. The same instrument will assist us much in our present inquiry. By referring to Tab. 3, (which is a table of temperature only, and which will be more fully explained hereafter,) it may be observed that the mean lowest temperature of the night is constantly several degrees below the mean constituent temperature of the vapour derived from the three before-named periods of the day. Now it is obvious, that once during the night the constituent temperature of the vapour cannot exceed that amount, nor is it at all probable that it should fall below it. Speaking from experiment, observation, and calculation, I have no doubt that a precipitation of vapour takes place during some period of every night in the year. I do not now allude to the deposition of dew from the cold produced by radiation, (the amount of which for the whole year is likewise shewn by the table of temperature,) but to a precipitation in the body of the atmosphere itself. In the most cloudless nights of the whole year, when the stars are bright, and the disc of the moon perfectly sharp and well defined, by bringing the hygrometer out of a warm room, it will be found that the point of deposition is often three or four degrees above the existing temperature of the air; proving thereby that particles of water, though invisible, are floating in the atmosphere. The ball of the instrument, being hotter than the surrounding medium, forms an atmosphere of vapour, from these round itself, of greater density than the general one, and which, consequently, is precipitated at a higher degree. The notice of such an effect is perpetually recurring in my register. Thus then we are justified in adding another observation of the hygrometer, though indirectly obtained, to the three already recorded, by assuming the minimum temperature of the night as the minimum temperature of the vapour. The mean of these four observations form, I have no doubt, a very near approximation to the mean of the whole day. I have calculated these results for the last year, and have included the pressure of the vapour so corrected in the fifth column of Table 1. The constituent temperature of the vapour, corrected for both years, is included in the seventh column of Table 3. The difference is not so great as might at first be supposed.

The mean degrees of dryness require correction upon the same principle, but in a somewhat different manner. The same fourth observation, so obtained, is available for this purpose; but the register-thermometer furnishes us with another datum of importance to this correction, namely the maximum of heat. I have made this the basis of another observation, by assuming the medium of the constituent temperature of the vapour found by the fore and afternoon experiments, and taking the difference of that and the greatest temperature. The degrees of dryness, so corrected, are included in the sixth column of Table 1. It will be observed that they scarcely differ from the former; so that half the dryness of the day, at its maximum heat, may be considered as the average degree of the twenty-four hours.

The same remarks apply, in a great degree, to the amount of evaporation contained in the seventh column; but the correction in this case would be so exceedingly small that I have not thought it worth while to calculate it. The method of estimating the force of evaporation from the results of the hygrometer appears to me to be fully as correct as that in common use, and, if any method could be devised of measuring accurately the velocity of the wind, would be infinitely preferable. The capacity of the atmosphere for moisture at any given time is measured; the rapidity with which that capacity is saturated is dependant upon the temperature of the body which yields the moisture. These conditions vary almost *ad infinitum.* They vary on the land and on the water, they vary in sunshine and in the shade, they vary as land is more or less clothed with vegetation, or as water is more or less deep. The hygrometer is capable of following and appreciating all these changes, but the common gauge can only give the amount of evaporation from the shallow body of water in the place where it has been fixed. After all, the estimation of the mean evaporation from the surface of the earth is the most difficult problem of meterology. From the sea, and other deep bodies of water, whose temperature is tolerably equable, the process is pretty regular; but one part of a field exposed to the sun is often yielding moisture at the rate of eight or nine grains per minute from a circular surface of six inches diameter, while another part is only yielding one grain and a half.

The eighth and ninth columns contain the quantity of rain, and the means of temperature, measured and calculated in the usual way. In Table 2, are inserted the mean results of both years in small figures, and the general mean in large. My great object has been to study facility of comparison, and I purpose, if I should continue my task, to collate in such a table the results of the current year, with the general average of all. This average will increase in accuracy as we proceed, and will, in a course of years, furnish a perfect standard by which to judge of the progress of the seasons. I am happy to find that the results which I have obtained with the barometer and thermometer correspond very closely with those deduced by Mr. Howard from a long series of years.

Table 3 is a general table of temperature, in which I have collected some particulars which were never before attended to, but which I am inclined to think may have much influence upon the general question. The first column contains the mean temperature, as usually estimated; the second and third, the mean highest and lowest; the fourth, the temperature at night, on the earth's surface, of such substances as are best fitted for radiation; the fifth the mean temperature of similar bodies exposed to the full influence of the sun's rays. This last is only complete for three quarters of a year, the idea having occurred to me last winter. My motives for adopting it I shall presently explain. The sixth column registers the mean constituent temperature of the vapour, and the seventh, the same corrected for a night observation as before explained. The extremes of all are included in small figures under their respective columns.

I shall divide the remarks I have to offer upon these particulars into two parts; the first shall include such observations as regard general consequences from the whole series, and the second shall consist of a short comparison of the several particulars of the two years.

The total weight of the atmosphere, and the grand problem of the variations of the barometer, are the first things that require our attention, and here it is worthy of remark, that no connexion can be traced between these fluctuations and the variations of the vapour. The barometer, in the last half quarter of the winter, when the pressure of the vapour is least, averages considerably higher than in the last half quarter of the summer, when the same pressure is greatest. A very cursory examination of the first and second columns of Table 2 will furnish plenty of instances of the like discrepancy. This result was certainly contrary to my expectations. Mr. Howard has inferred from his admirable system of averages, that " the winter barometer gains in its average .021 inches upon the autumnal; the vernal .030 inches or half as much more upon the winter; the summer .045 inches or half as much more still upon the vernal; but in the *autumnal* average, the whole difference is lost again, and the barometer comes back to its lowest level." In reasoning upon this gradation he ascribes the loss and gain to water; which is at one time converted into vapour, permanent as a part of the atmosphere for the season; at another dismissed in rain. In elucidating this connexion he continues, "Now in the *brumal* quarter, where we find the average of the barometer *lowest*, the temperature is lowest also; and there is every reason to conclude that the atmosphere in our district, and for many degrees of latitude and longitude around us, contains at this season, the lowest proportion of ponderable vapour." But this reasoning does not agree with the fact which he had just before stated; namely, that the average of the barometer is lowest in the *autumnal* quarter. Neither is the amount of this difference nearly sufficient to identify it with that of the vapour: it does not reach 0.1* inch while the excess of the summer vapour over the winter exceeds 0.2 inches. Moreover, if this influence were really appreciable in this manner, it is one of so constant a nature as must have shewn itself invariably in every recurrence of the seasons, not requiring to be elicited by a long series of averages.

But that there is a connexion between the great fluctuations of the atmosphere and the vapour which it contains at least in its precipitation, is evident from the following fact: of 674 observations of rain, hail, and snow, during the two years, only .94 took place, while the barometer was above the average of the season as deduced in Table 2. Of these 94 the greater part were very partial showers, chiefly in the winter quarters. Of the general results of the barometer alone, I shall not attempt to speak: the subject has been so ably elucidated by Mr. Howard, with such ample provision of observations, that it would be presumption in me to follow him, especially with such short experience. I am happy to observe that the mean of both years' observations agree with the mean as deduced by him for London: those periods where I differ, I consider as only oscillating round the true point, to which in a series of years they will be amenable.

The variations in the quantity and pressure of the aqueous

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* In Mr. Howard's work, Vol. II., page 281, this difference is misprinted 0.96 inches for .096.

vapour appear to be more equable, and to be regulated by more certain laws. In the winter the amount is least, and in the summer greatest, reaching very nearly to double. The spring and autumn quarters are intermediate, the latter having a slight excess over the former. There is, therefore, an evident connexion between these fluctuations and those of the mean temperature, which exactly follow the same gradation. Even the accidental small variations of the mean temperature, from the average, are uniformly accompanied by a corresponding alteration in the vapour. The utmost range appears to be from 0.090 to 0.631 inches, the mean pressure 0.327 inches, or rather, taking into the account the correction for the night, 0.313.

As we have been unable to trace any general effect produced upon the motions of the barometer by the variations of the vapour, so are we unable to discover any influence of the former upon the latter.

But although the force of the vapour increases with the mean temperature, and reaches its maximum at the same period of the year, they do not travel together with equal steps. Mr. Howard deduces the difference of the heat of the seasons exactly equal; *viz.*, 11½ degrees for each quarter. There can be no doubt that this is correct. My own tables agree as nearly with the determination as can be expected, from so short a series. The difference comes out from them, spring above winter, 13°; summer above spring, 11°; autumn below summer, 12°; winter below autumn, 12°. But the gradation of the constitutent temperature of the vapour is as follows:—Spring above winter, 9°; summer above spring, 11°; autumn below summer, 8¼; winter below autumn, 11°. The circumstances of the case prove, I think, that this series approximates very nearly to the correct progression. If the difference had been equal, as in the mean temperature, the dryness also of the seasons would have been equal: but now, from winter to spring, the capacity of the air for moisture increasing faster than the supply, the degree of dryness must also increase; we accordingly find it in the table rising from 1¾° to 7°. From spring to summer the increase is nearly equal, consequently the degree of dryness remains the same, 7°. From summer to autumn the temperature of the air decreases

12°, and that of the vapour only about 9°. We accordingly find that the dryness falls to 4°, which again diminishes, though at a less rate, from autumn to winter. The exact correspondence of the consequences, and their precise march, we cannot expect to ascertain, but from the average of a series of years; but enough, I think, is apparent, to enable us to speculate a little upon their causes. The sea, and other deep bodies of water, are, no doubt, the grand reservoirs from whence is supplied the greater part of the vapour of the atmosphere; and the cooling and heating of such bodies are regulated by particular laws. It is a well known fact, that the heat of water cannot be raised above 40°, till every particle of the mass, however deep, has attained that temperature; but that beyond this, the superficial water may be heated, without affecting the lower stratum. So in cooling, the whole volume must be cooled down to 40° before the superficial water can fall below that point. Now the Spring quarter, from March to May, includes, probably, the period when the whole body of water, in this climate of the globe, rises to the point of 40°. A large portion of heat must, therefore, be expended, in warming its entire bulk; and, consequently, its increase cannot keep pace with the surrounding air. In the next quarter, however, having attained its maximum of density, the whole heat is expended upon the superficial water alone; and the increase of heat in the upper stratum of water, and consequently, in the vapour, is commensurate with that of the air. The autumn again includes the point at which the whole mass of heated water must fall to 40°; which point being passed, the cooling of the upper stratum keeps pace with the cooling of the air. Daily observations upon the superficial temperature of the sea would greatly tend to elucidate this subject.

The rate of evaporation is in compound proportion to the heat of the water yielding the vapour, the degree of dryness, and the velocity of the wind. In the calculation taken in the tables, the temperature of the air has been always taken as the temperature of the evaporating fluid; whereas it is obvious that this can seldom, if ever, be the true state of the case. Did we but know the temperature of the sea at different hours of the day, and at different periods of the year, we should obtain, by the substitution, a much more useful result. The evaporation from the land must be much more irregular than that from the ocean and other deep accumulations of water: it must vary with almost every moment of the day, and every variety of situation. The fallow-field, and the field of corn, yield their moisture with different facility; and sun-shine and shade must produce still greater modifications.

I have found, from experiment, after a shower of rain, while the temperature of the air has been 66, and that of the vapour 60, that the temperature of a running stream has been 62; of wet grass, when the sun had been shining upon it, 69; of wet sand, 76; of stagnant water, 67; of damp garden-mould, in the sun, 84. So that at the very same moment of time these situations were respectively yielding at the rate of 0.18 grains, 0.89 grains, 1.82 grains, 0.67 grains, and 3.16 grains, per minute, from a surface of 6 inches diameter. Not that vapour of these various degrees of force existed at one time; for it is evident that those which were above the temperature of the air must have been instantly precipitated, and as instantly redissolved at the general temperature.

The facility and rapidity with which vapour of such different degrees of force in its nascent state is resolved into one general mass of equal density, is very worthy of observation. I have often endeavoured to detect it before its change, with the hygrometer, but with equivocal success. For example, when the temperature of the air was 56, after sun-set, and the hygrometer denoted the dew-point to be 47, I placed myself in the middle of a stream whose temperature was 63; the precipitation took place at 50, only 3° higher than before. As the water was here yielding vapour of 16° higher temperature than the general average, I expected to have found it more nearly approaching the point of saturation; especially as there was very little air stirring. From hence we must conclude that it diffuses itself with immense velocity, which, perhaps, we shall have the less difficulty in conceiving, when we call to mind that the rising vapour possessed a force of 0.578 inch, which was only opposed by one of 0.339 inch. When the general atmosphere approaches more nearly to the point of humidity, this rise of hotter vapour from water is very visible in the cloud which is often observed to hang over the course of rivers in a calm evening. In certain situations, also, where the free rise of vapour is opposed by impending obstacles, a small increase of elasticity may be observed. Thus, after a succession of wet days, when the weather had again become clear and settled, I have observed the temperature in the open country to be 64°, and the point of deposition 53°, when in the heart of a large wood they were respectively 62 and 55; the foliage of the trees forming a mechanical obstruction to equal diffusion.

It would be easy to reduce the data contained in the tables to the number of inches evaporated from a given surface, but in the present state of our knowledge it would be useless, or, what is worse, might tend to mislead. It is amusing to observe the attempts that are made to make the results of the common evaporating-gauge tally with those of the rain-gauge, when the former, so far from representing the circumstances of those bodies which yield the great body of vapour on the earth's surface probably does not correspond in all essential particulars with a dozen puddles in the course of the year. The results of the hygrometer accommodate themselves more easily to the ever-varying circumstances, but even from these we can only at present infer the capacity of the atmosphere for moisture, modified by the velocity of the winds.

The quantity of rain which falls in different seasons is probably the most variable result that meteorology affords; but nevertheless some general principles may be derived from the observation. The greatest quantity falls in the driest half of the year; and this quantity, when no other circumstance modifies the conditions, appears to be in direct proportion to the quantity of vapour in the air. Thus, in the two half years from September to February, and from February to August, the average of the barometer is exactly similar, but the quantity of vapour in the latter exceeds that in the former by one-third: the quantity of rain is also as 3 to 2. At the same time, the degree of dryness is more than double, and the force of evaporation more than treble. It is also perfectly evident that the height of the barometer has a very material influence, as well as the temperature. To illustrate the former, without the latter, let us take the quarter of a year from March to May 1820, and compare it with the same quarter of 1821. The force of vapour was nearly alike, and the temperature of the latter rather higher than that of the former; nevertheless the rain of this quarter was more than double; the height of the barometer, in the first, being rather above the mean, while in the latter it was 0.14 inch below it. The power of temperature in modifying the precipitation is strikingly illustrated in the two half-quarters of October and November. In 1821 the barometer was lower, and the force of the vapour higher, than in 1820. Nevertheless, the quantity of rain in the latter was double that in the former, for the average mean temperature was 5¼° lower. The joint influence of the two will not fail to strike any one who takes the trouble to inspect the tables.

We now come to the consideration of temperature; and I have endeavoured to give a complete view of the subject in the third table. I have therein included three series of observations not usually taken into account. The first is the greatest effect of radiation at night, the second the highest temperature of the sun in the course of the day, and the third the constituent mean temperature of the vapour.

I can add nothing to the ingenious views of Mr. Howard with regard to mean and extreme temperature in general. My motive for including observations upon radiant heat both from and to the earth were, first, their probable importance to vegetation and agriculture, and, secondly, their obvious connexion both with the production and precipitation of vapour. The medium effect of radiation from the earth at night, it will be seen, is about 4°: the maximum which I have observed is 13°. It will probably strike many people with surprise, to find that there is but one period of six weeks in the year in which vegetation, in particular situations, is not exposed to a freezing temperature, and only one quarter of a year in which the thermometer so situated is not subject to fall as much as six or seven degrees below the point of congelation. The effect of situation is extremely curious in modifying this process. In the latter end of the month of September 1821, in a garden placed in a sheltered situation in a valley surrounded by low hills, I observed that all the vines and Dahlias were frost-bitten and turned quite black, while in another garden not 100 yards distant, upon the gentle acclivity of a hill, no signs of frost were to be seen. At another time, early in the autumn, in the same situation, the grass has been quite rigid with hoar frost, while upon the hill nothing but a light dew was to be perceived. The effect of this cold upon vapour is sometimes very visible. In sheltered situations, and in very calm weather, a low mist may often be seen rising in the meadows, when the surrounding spaces are quite clear. If the ground which this mist covers be examined, it will constantly be found to be 5° or 6° below the heat of the clear regions. The slightest breeze is sufficient to mix the air of the two, and to dissipate the cloud. This will often even be effected by a person merely walking through it.

With regard to the quantity of dew deposited upon filamentous substances, I have found that a piece of cotton exposing a flat circular surface of two inches diameter, gained in weight twenty grains, when the force of the vapour was .481 inch, and the radiating thermometer 8° below the lowest standard, that is to say when the two thermometers were respectively 58 and 50. At another time, when they were only 45 and 37, the same piece of cotton had only gained eleven grains, the force of the vapour being .316, and the difference of temperature the same. It is most probable, that in neither instance had there been any loss from evaporation, as at the time of examination the hygrometer denoted the complete saturation of the atmosphere. Calculating from these experiments, and the mean force of vapour and radiation, the average deposition of dew for every night in the year, upon a like surface, would be about six grains.

The results of the experiments upon the direct radiation of the sun are still very incomplete; but I cannot help hoping, that when sufficiently advanced, they will afford matter for useful speculation. What is the difference of the effect in different years? What are its effects in maturing the fruits of the earth? What is its connexion with the production of vapour? How is its energy spent when veiled from the earth by clouds? Its average for the half year, from March to August, was 27° above the mean highest in the shade, its maximum effect 144°. Many reflections readily suggest themselves upon this subject, but it would be premature to indulge in them at present.

The mean constituent temperature of the vapour stands in an interesting relation to the other particulars of general temperature. Its connexion with the average of the mean is very obvious, and it appears to be influenced, in some measure, by the mean lowest, which it never exceeds by more than 4½°. The average degree of dryness for the whole twenty-four hours, by this comparison, would appear to be 1° below that obtained by the three direct experiments, and the correction before applied. But it must be remembered, that although the force of the vapour is affected by the maximum cold of the night, the degree of dryness is not liable to its influence. After the point of humidity is once attained, which it generally is long before the minimum, a further reduction of temperature does not alter the circumstances; so that an observation of the lowest temperature of the night as included in the general mean temperature, is not applicable to this purpose.

I have now, in conclusion, a few remarks to make upon the comparison of the two years. They will not be found to differ much in their general averages, but in their particulars very materially. The year 1819—1820 was, however, drier than the year 1820—1821, and a greater depth of rain fell in the latter than in the former. The first was also distinguished very much more by extremes than the second, all the instruments except the barometer denoting a very much wider range.

The autumns differed very essentially in their characters. In the first half-quarter the year 1819 was 1¾° drier, and 2¼°

warmer, than 1820. The depth of rain was, however, greater; for the barometer was not so high, and the vapour was more dense. As this is the season of the year when the most important fruits of the earth come to maturity, and the securing of them in good order is the most anxious care of the human race in these latitudes, the state of the weather at this time acquires a proportionate interest. In 1819 it was remarkable for being dry, clear, and warm. The finest harvest that had been known for years was housed in the most satisfactory manner. Not only in this country, but throughout Europe, it formed almost an epoch, and corn-fields, orchards, and vineyards, shared in the general benefit. The mean results of this period may therefore be considered as the standard of a fine season. The turnip-fields indeed, so important a part of English agriculture, suffered from great drought and never recovered, but the grasses, and pasture in general, though burnt in the beginning of the autumn, revived with the rains in the last half-quarter.

In the year 1820 the harvest was much later. The crops of corn, though abundant, were not of so fine quality as the last, and were much mildewed; but upon the whole, this was also reckoned a productive harvest. The weather was still too dry for the turnip-crops.

The second half-quarters of the autumn were precisely similar in point of dryness, but in 1820 the temperature exceeded that in 1819 as much as it fell below it in the first six weeks, making the averages of the whole quarters precisely the same. This accession of heat probably prevented the precipitation of the usual quantity of water, for the amount of rain was less than half.

The winters differed still more widely than the autumns. The first was remarkable for its severity, and the second for its mildness: the respective mean temperatures being 33 and 38. In this quarter the latter regained the dryness which it was behind in the preceding; and the means of the two half-years were exactly similar. This state of the atmosphere is reckoned by no means unfavourable to the farmer, and neither in the cold season of 1819-1820, or in the dry season of 1820-1821, were any complaints made. The last winter half-quarter of 1821 was particularly remarkable for a very high average of the barometer.

In the first half-quarter of the spring the year 1820 was very backward; the wheats looked very indifferent, and vegetation altogether very unpromising. The blossoms of fruit-trees were very much injured by frosts and cold winds. In the corresponding period of 1821, the weather, on the contrary, was extraordinarily fine and open. The operations of husbandry were unusually forward; the wheat was vigorous and firmly rooted, and every appearance of vegetation as flattering as could be wished. The former period was distinguished from the latter, by being 1° drier, and $2\frac{1}{2}$° colder, a much higher barometer, and half the quantity of rain.

In the second half-quarter, the advantage began to turn in favour of the first year. The temperature was higher, and the dryness continued. There was a sufficiency of rain, in the form of warm showers; the appearance of the wheat improved, and barley and oats promised very well. In the second year vegetation was checked by cold north winds: pasture was not forward, but still the whole prospect was good. At the commencement of the summer of 1820, the weather turned extraordinarily hot; the change was very sudden, and the produce of the fields made astonishing progress to maturity. The harvest commenced early, and, although the weather was rather unsettled, was well secured. The produce of all kinds was abundant, though not of the first quality.

The summer of 1821 was extremely backward, but favourable for growing wheats. The lowness of temperature was considered, at the time, rather favourable, as tending to check over-luxuriance of vegetation: barley, however, suffered materially from this cause. Near the usual harvest-time, the corn, though full-eared, had hardly completed the flowering process. Oats were heavy, full-eared, and promising. Turnips, and all kinds of pasture, particularly fine and luxuriant. A succession of hot days, at the latter part of the summer-quarter, raised the average temperature above the corresponding period of the first year, and rapidly brought on the ripening of the grain. Nothing was now wanting, but a favourable dry period to house the harvest. The reaping did not begin till the 25th or 26th August, more than a fortnight later than the usual time.

This summer must be reckoned altogether wet and cold, and owing to this, it is feared that the vintage on the Rhine, the Elbe, and in Switzerland, will entirely fail.

The great characteristic features of the two years were, in the first, a cold winter and a hot summer; and in the second, a very mild winter, and a backward cold summer.

ERRATA IN TABLE, Vol. X, p. 131.

Column of Temperature, February, *for* 31 *read* 32½
— 29 — 31
August, — 63 — 61

Art. XII. *An Account of some Observations and Experiments made by* Mr. Green, *during his ascent in a Balloon from Portsea, on the 6th of September*, 1821.

[Communicated by R. H. Solly, Esq., F.R.S. and M.R.I.]

Mr. Green has the merit of being the first person who has tried experiments upon the buoyant powers of coal-gas. In some of his preliminary trials, he ascertained that the ascensive force of a small balloon three feet in diameter, when filled with this air, was equal to eleven ounces, and when filled in the common way with hydrogen not more than fifteen ounces. He has since made three voyages in a balloon filled from the street-pipes, thereby reducing the expense of such ascents to a mere comparative trifle.

During his last ascent he made some experiments of much interest to science, and which, as he has promised to repeat

TABLE I.

From September 1820, to August 1821.

	MEAN PRESSURE OF ATMOSPHERE, *and Extremes.* Half Quarter	Quarter	Half Year	Year	MEAN PRESSURE OF VAPOUR, *and Extremes.* Half Quarter	Quarter	Half Year	Year	MEAN WEIGHT OF VAPOUR, *and Extremes.* Half Quarter	Quarter	Half Year	Year
Sept. Oct. Nov. 1820.	ins. [illegible] 0.07 [illegible]				ins. [illegible] .377 [illegible]				grs. [illegible] 4.166 [illegible]			
	[illegible] 29.63 [illegible]	[illegible] 29.84 [illegible]			[illegible] .284 [illegible]	[illegible] .310 [illegible]			[illegible] 3.206 [illegible]	[illegible] 3.686 [illegible]		
Dec. January, February, 1821.	[illegible] 29.75 29.03				.415 .237 .120				4.616 2.701 [illegible]			
	30.82 30.31 29.17	30.62 30.03 29.17	30.62 29.93 28.74		.375 .229 .134	.415 .233 .120	.635 .281 .120		4.193 2.619 1.524	4.616 2.660 1.391	6.822 3.173 1.391	
March, April, May,	30.35 29.64 29.04				.429 .283 .186				4.750 3.190 2.160			
	30.28 29.82 29.23	30.35 29.73 29.23			.490 .328 .174	.490 .305 .174			5.302 3.642 1.941	5.302 3.416 1.941		
June, July, August.	30.30 29.99 29.59				.512 .375 .221				5.950 4.142 2.425			
	30.28 29.90 29.50	30.30 29.94 29.50	[illegible] 29.83 29.23	30.62 29.88 28.74	.698 .490 .303	.698 .432 .221	.698 .368 .174	.698 .324 .120	7.366 5.340 3.922	7.366 4.741 2.425	7.366 4.078 1.391	7.366 3.625 1.391

	MEAN DEGREES OF DRYNESS, *and Extremes.* Half Quarter	Quarter	Half Year	Year	MEAN PRESSURE OF VAPOUR, *Corrected for 24 hours.* Half Quarter	Quarter	Half Year	Year	MEAN DEGREES OF DRYNESS, *Corrected for 24 hours.* Half Quarter	Quarter	Half Year	Year
Sept. Oct. Nov. 1820.	[illegible] 5¼				ins. .363				5½			
	9 1¾	[illegible] 3½			.273	.316			2¼	3¼		
Dec. January, February, 1821.	19 2¼				.23[illegible]				2			
	20 2	20 2¼	20 3		.223	.229	.270		2¼	2¼	3	
March, April, May,	16 4½				.273				4¾			
	23 8	23 6¼			.322	.296			8	6¼		
June, July, August.	25 7				.365				7¼			
	20 5¾	25 6¼	25 6¼	25 4¾	.482	.422	.357	.313	6	6½	6½	4¾

	MEAN EVAPORATION, *and Extremes.* Half Quarter	Quarter	Half Year	Year	QUANTITY OF RAIN. Half Quarter	Quarter	Half Year	Year	MEAN TEMPERATURE, *and Extremes.* Half Quarter	Quarter	Half Year	Year
Sept. Oct. Nov. 1820.	grs. [illegible] .33				ins. 1.90				76 53¾ 62½ 45½ [illegible]			
	[illegible] .06	[illegible] .19			1.57	3.47			[illegible] 43¼ 48 38¼ [illegible]	76 48¼ 55 41¼ [illegible]		
Dec. January, February, 1821.	[illegible] .08				3.32				55 38½ 41½ 36 24			
	.55 .06	.65 .07	1.48 .13		.49	3.81	7.28		48 37¼ 41½ 33 25	55 38 41½ 34½ 24	70 43 48 38 24	
March, April, May,	[illegible] .23				3.80				63 45½ 52¼ 38½ 30			
	1.59 .53	1.59 .38			2.00	5.80			74 53 61¼ 44½ 35	74 49¼ 56¾ 41½ 30		
June, July, August.	1.96 .52				3.52				78 56½ 65½ 47¼ 37			
	1.73 .51	1.96 .52	1.96 .45	1.96 .29	3.04	6.56	12.36	19.64	62 63¼ 71¼ 54¼ 49	82 60 68½ 51 37	62 54½ 62¾ 46¼ 30	84 48¾ 55 42 24

To follow p. 114.

TABLE II.

MEANS OF TWO YEARS.

Season	Years	MEAN PRESSURE OF ATMOSPHERE, *and Extremes.* Half Quarter	Quarter	Half Year	Year	MEAN PRESSURE OF VAPOUR, *and Extremes.* Half Quarter	Quarter	Half Year	Year	MEAN WEIGHT OF VAPOUR, *and Extremes.* Half Quarter	Quarter	Half Year	Year	MEAN DEGREES OF DRYNESS, *and Extremes.* Half Quarter	Quarter	Half Year	Year	MEAN EVAPORATION, *and Extremes.* Half Quarter	Quarter	Half Year	Year	MEAN QUANTITY OF RAIN. Half Quarter	Quarter	Half Quarter	Year	MEAN TEMPERATURE, *and Extremes.* Half Quarter	Quarter	Half Year	Year	MEAN HIGHEST AND LOWEST. Half Quarter	Quarter	Half Year	Year
		Ins.				Ins				Grs.								Grs.				Ins.											
Autumn.	1819 1820	29.99 30.01 30.03				.429 .403 .377				4.697 4.431 4.166				6¾ 6 5¼				.50 .41 .33				2.36 2.13 1.90				56 55 53¾				63 50 62¼ 47¾ 62½ 45½			
Autumn.	1819 1820	29.73 29.68 29.63	29.87 29.85 29.84			.262 .273 .284	.355 .342 .330			2.988 3.097 3.206	3.944 3.815 3.686			1¾ 1¾ 1¾	4½ 4 3½			.08 .07 .06	.32 .25 .19			3.47 2.52 1.57	5.83 4.65 3.47			38 40½ 43¼	48 48 48¼			43 34 45½ 36 48 38¼	54 42 54½ 42 55 41¾		
Winter.	1819 1820	29.73 29.74 29.75				.219 .228 .237				2.505 2.603 2.701				1 1½ 2¼				.03 .05 .08				2.02 2.67 3.32				32 35¼ 38½				35½ 28½ 38¼ 32¼ 41¼ 36			
Winter.	1820 1821	29.79 30.05 30.31	29.76 29.89 30.02	29.81 29.87 29.93		.225 .227 .229	.222 .227 .233	.268 .284 .261		2.580 2.599 2.619	2.643 2.601 2.560	3.243 3.208 3.173		1¾ 2 2	1½ 1¾ 2¼	3 3 3		.06 .06 .06	.05 .06 .07			1.18 0.83 0.48	3.20 3.50 3.81	9.06 8.17 7.28		34 35¼ 37½	33 35½ 38	40¼ 42 43½		38 32¾ 39¾ 33½ 41½ 33½	37 29 39 31½ 41½ 34½	45½ 35½ 47 36½ 48 36	
Spring.	1820 1821	29.83 29.72 29.61				.268 .275 .283				3.013 3.101 3.190				6½ 5 4½				.27 .25 .23				1.63 2.71 3.80				43 44¼ 45½				48 38 50 38 52½ 38½			
Spring.	1820 1821	29.93 29.88 29.82	29.88 29.80 29.73			.333 .330 .328	.300 .302 .305			3.680 3.661 3.642	3.346 3.381 3.416			10½ 9¼ 8	8 7 6¼			.70 .61 .53	.48 .43 .38			1.51 1.75 2.00	3.14 4.47 5.80			54 53½ 53	48½ 49 49½			63 45 62 44½ 61¼ 44	55 42 56 41¾ 56¾ 41½		
Summer.	1820 1821	30.01 30.00 29.99				.421 .398 .375				4.610 4.376 4.142				8 7½ 7				.59 .55 .52				2.39 2.95 3.52				59 57¾ 56½				68 50 66¾ 48½ 65½ 47¼			
Summer.	1820 1821	29.91 29.90 29.90	29.96 29.95 29.94	29.92 29.87 29.83	29.87 29.87 29.88	.473 .481 .490	.448 .440 .432	.374 .371 .368	.331 .327 .324	5.143 5.241 5.340	4.886 4.814 4.741	4.117 4.097 4.078	3.680 3.652 3.625	7¾ 6¾ 5¾	8 7 6¼	8 7 6¼	5½ 5 4¾	.66 .58 .51	.63 .57 .52	.65 .50 .45	.37 .33 .29	4.13 3.58 3.04	6.52 6.54 6.56	9.66 11.01 12.36	18.72 19.18 19.64	62½ 63 63½	61 60½ 60	55½ 55 54½	48 48¼ 48½	71 54 71 54½ 71¼ 54¾	70 52 69¼ 51½ 68½ 51	62 47 62¼ 46½ 62¾ 46	54 41 54½ 41¼ 55 42

EST IN SUN.			CONSTITUENT TEMPERATURE OF VAPOUR.				CONSTITUENT TEMPERATURE OF VAPOUR, *Corrected for 24 hours.*			
rter	Half Year	Year	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year
			54 66.33 66.35 50¼				53 49			
			40 56.25 54.29 42	48¼ 66.25 66.29 46¼			38¼ 41	46½ 45		
			35 53.10 53.18 37				33¼ 36¾			
½			35½ 49.22 50.21 36	35 53.10 53.18 36½	42¼ 66.10 66.18 41¾		34½ 35¼	33½ 36	40¼ 40¾	
			40½ 54.19 54.30 42				39¾ 41			
¾			46½ 58.27 58.28 46	43½ 58.19 58.29 44			46 45½	43 43¼		
			53½ 70.40 61.35 50				52½ 49¼			
	144 90¼	144	57 67.46 69.49 58	55½ 70.40 69.35 54¼	50 70.40 69.35 49½	46½ 70.10 69.18 45¾	56½ 57½	54½ 53½	49½ 48½	44¾ 44¾

To follow p. 114.

TABLE III.

MEAN TEMPERATURE AND EXTREMES.

		MEAN.				HIGHEST IN SHADE.				LOWEST IN SHADE.				LOWEST OF RADIATION.				HIGHEST IN SUN.				CONSTITUENT TEMPERATURE OF VAPOUR.				CONSTITUENT TEMPERATURE OF VAPOUR, *Corrected for 24 hours*			
		Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year.	Year	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year
Autumn,	1819	56				63				50												54				53			
						[illegible]				[illegible]				[illegible]								66.31 [illegible]							
	1820	53¼				62¼				45½				39½								50¼				49			
	1819	38	48			43	54			34	42			30½								40	48¼			38¼	46½		
						7 7	76 76			[illegible]	[illegible]			[illegible]	[illegible]							[illegible] 54.29	[illegible]						
	1820	43¼	48¼			48	55			38¼	41¾			35¼	37½							42	46¼			41	45		
Winter,	1819	32				35½				28½				23¼								35				33¼			
						[illegible]				11 [illegible]								[illegible]				53.16 53.16							
	1820	38½				41¼				36				33¼				42				37				36¾			
	1820	34	33	40		38	37	45½		32¼	31	35		29	26							35½	35	42¼		34½	33½	40¼	
						50 48	53 55	76 76		19 [illegible]	[illegible] 21	11 [illegible]		12 [illegible]	[illegible]	5 19		[illegible]	86			[illegible]	[illegible]	[illegible]					
	1821	37¼	38	43		41½	41½	48¼		33½	34¼	38		29¾	31¾	34½		50¼	46½			36	36½	41¼		35¼	36	40¼	
Spring,	1820	43				48				38				32¼								40½				39¾			
						[illegible] 61				[illegible]				[illegible] 22				110				51.19 54.30							
	1821	45½				52¼				38½				31¼				71				42				41			
	1820	54	48½			63	55			45	42			39¼	35¾							46½	43½			46	43		
						77 74	77 74			31 35	24 [illegible]			21 [illegible]	18 22			114	114			58.27 58.28	58.19 58.26						
	1821	53	49¼			61¾	56¾			44	41½			39¼	35½			88½	79¾			46	44			45½	43¼		
Summer,	1820	59				68				50				43¾								53½				52½			
						69 78				[illegible]				[illegible] 31				[illegible]				70.40 61.35							
	1821	56½				65½				47¼				45½				97				50				49¼			
	1820	62½	61	55½	48	71	70	62	54	54	52	47	41	49	46½	41						57	55½	50	46½	56½	54½	49½	44¾
						63 62	69 62	69 62	[illegible] 82	12 49	40 37	11 21	11 24	35 39	30 31	18 22	5 19	144	144	144	144	67.46 69.49	70.40 69.35	70.40 69.35	70.10 69.18				
	1821	63¼	60	54½	48¾	71¼	68½	62¼	55	54¾	51	46	42	51¼	48½	42	38	105	101	90¼		58	54¼	49½	45¾	57½	53½	48½	44¾

To follow p. 114.

them at no distant period, may, probably, furnish data of the utmost importance. The operation of filling the balloon commenced about eleven o'clock on Thursday the 6th of September, at Portsea, under the superintendence of the engineer of the gas company; the operation was completed by half-past twelve. The morning, although cloudy, seemed nevertheless to presage favourably. At half-past one, Mr. Green took his seat in the car, and immediately began to ascend slowly and majestically. On leaving the ground the barometer stood at 29.7 and the thermometer at 74°. The balloon took a N.E. direction, and he found that he could ascend to a great height without entering the clouds. After he had attained a height of about 4,500 feet, he launched a parachute with a cat which fell in safety; its descent was very rapid for about 300 feet, it afterwards became distended, and its oscillation was very inconsiderable: it reached the earth in about eight or nine minutes after its separation from the balloon. He now ascended with great rapidity to upwards of 10,000 feet. After completing his observations which are annexed, Mr. Green at twenty minutes after two determined to descend. By repeatedly discharging ballast the descent was rendered gradual, till half-past two; at this time a heavy shower of rain added considerably to the weight of the balloon, and occasioned likewise the condensation of the gas, accelerating the fall in a manner that might have been dangerous, had it not been checked by throwing out a large quantity of ballast. He at last landed in safety in a meadow near Frimley, Surrey, having completed a voyage of forty-five miles in forty-five minutes, in a direction N.N.E. from Portsmouth.

During this voyage, the attention of Mr. Green was chiefly directed to the indications of the barometer, thermometer, and Daniell's hygrometer. His observations on these instruments are included in the following table.

TIME.			Barom.	Therm.	Dew Point.
15′	before	2	29.7	74	
11	—	2	26.8	70	
7	—	2	23.2	72	
4	—	2	21.4	69	64
10	after	2	20.5	45	32
12	—	2	20.3	38	25
26	—	2	21.9	40	37
28	—	2	26.1	48	rain

At the point of greatest altitude Mr. Green filled two bottles, which had been previously prepared and filled with distilled water, with the atmospheric air; these have been since examined at the laboratory of the Royal Institution by Mr. R. H. Solly and Mr. Faraday; the first was weighed and opened under distilled water of the temperature of 60°, 660.5 grains of water entered, the whole quantity which the bottle would hold being 1,910 grains. The second bottle, when full, held 1,916 grains of water; the quantity which entered when opened was 636.7 grains.

The average of these two experiments gives rather more than one-third for the diminution of density of the atmosphere at the height to which Mr. Green ascended. This agrees very nearly with the results of the barometer. The analysis of the air confirmed the observation of M. Gay Lussac, that no difference can be detected in the atmospheric air taken from great altitudes and at the surface of the earth: the experiments were made by explosion with hydrogen, and the comparison of the condensation made over mercury between the air of the bottles and that of the laboratory; the differences were perfectly immaterial, sometimes on one side and sometimes on the other, evidently arising from unavoidable errors in experimenting.

Remarks by Mr. Daniell.

It is much to be regretted that Mr. Green omitted to take the point of deposition before he commenced his ascent, or after he had again landed, as such an observation would have added greatly to the value of his experiments; as they stand, they will, however, form a valuable addition to any future observations which it is to be hoped that he may be induced to make. Some interesting particulars may even now be derived from them. I have subjoined a calculation of the different heights as observed by the barometer, and the density of the vapour at those heights, as indicated by the hygrometer. The different degrees of dryness are also added in the following table.

TIME.			Barom.	Therm.	Dew Point.	Force of Vapour.	Height. Feet.	Dry-ness.
15	before	2	29.7	74				
11	—	2	26.8	70			2952	
7	—	2	23.2	72			7288	
4	—	2	21.4	69	64	.597	9893	5°
10	after	2	20.5	45	32	.200	11059	13°
12	—	2	20.3	38	25	.156	11293	13°
26	—	2	21.9	40	37	.237	8813	3°
28	—	2	26.1	48	48	.351	3630	0°

The state of the atmosphere on the day of Mr. Green's ascent was evidently not favourable for ascertaining the grand question of the rate of decrease in the density of the aqueous vapour, when the atmosphere is undisturbed. By an experiment which I made myself in the country not very distant from the point at which he descended, the degree of precipitation on the earth's surface, at two o'clock, was 64°, exactly the same as he found it at the height of 9,893 feet; so that it is very probable that there was but little difference in the density of the vapour in that immense stratum. The very small decrease in the temperature agrees with this supposition, which also accounts for the great height to which the balloon ascended without entering the clouds; thus the dryness would have decreased from 10°, on the surface of the earth to 5°, and before the next observation I should conjecture that the aëronaut must have passed the point of precipitation through a bed of clouds. After crossing this plane the temperature and the force of the vapour rapidly decreased, and the degree of dryness increased with the ascent.

The condition of the atmosphere appears to have varied considerably with the progress in-land, and in receding from the sea the force of the vapour seems to have decreased, and the temperature of the air still more rapidly. The point of deposition in the descent was passed in a shower of rain, at a distance of not more than 3,630 feet above the surface of the earth, the constituent temperature of the vapour having fallen 16°, and the temperature of the air not less than 24° in the short space of thirty-five minutes. The increase of the density of the vapour towards the surface of the earth, was much more gradual during the descent over the land than during the ascent over the sea: was the difference owing to a large body of vapour rising from the sea? The temperature of the water at the time would have been a valuable addition to the observations.

It is to be hoped that Mr. Green will be enabled to persevere in his exertions, as a continuation of such observations cannot fail to be of the utmost use to science. It would be desirable also that he should be a little more particular in noticing the different indications of the atmosphere, that is to say, the course of the wind on the surface of the earth, and the direction of the different currents—the height of the clouds and the temperature when crossing them, and whether there is more than one stratum at different heights. Now that he is aware of the tendency of the different experiments, there are, no doubt, many particulars which will readily suggest themselves to him, and he will not, of course, neglect to make observations before his departure, and after his return, if circumstances will permit.

Art. XIII. *Proceedings of the Royal Society.*

The following papers have been read at the table of the Royal Society, since our last report.

May 31. Some experiments on temperature, with a view of determining the ratio of temperature, and the point of absolute cold, by John Herapath, Esq., communicated by Davies Gilbert, Esq., F.R.S.

June 7. An account of the re-measurement of the cube, cylinder and sphere, used by the late Sir George Shuckburgh Evelyn, in his inquiries respecting a standard of weights and measures: by Capt. Henry Kater.

21. An account of observations made at the observatory of Trinity College, Dublin, since 1818, for investigating the parallax and aberration of the fixed stars, and the effects of lunar nutation: by the Rev. John. Brinkley, D. D.

28. On the effects produced in the rates of chronometers by the proximity of masses of iron: by Peter Barlow, Esq., communicated by John Barrow, Esq.

July 5. Some positions respecting the influence of the voltaic battery, in obviating the effects of the division of the 8th pair of nerves, by A. P. Wilson Philip, M. D., communicated by B. C. Brodie, Esq.

On the magnetic phænomena produced by electricity, and their relation to heat, occasioned by the same agent: by Sir Humphry Davy, Bart., P. R. S.

12. An investigation of some theorems relating to the theory of the earth, and the principle of equilibrium in fluids, by M. Hoene Wronski, communicated by John Pond, Esq., Astronomer Royal.

On the peculiarities that distinguish the Manatee or Dugong of the West from that of the East Indies, by Sir Everard Home, Bart., V. P. R. S.

On a new compound of Chlorine and Carbon, by Messrs. R. Phillips and Faraday, communicated by the President.

On the Nerves; giving an account of some experiments on their structure and functions, which lead to a new arrangement of the system, by Charles Bell, Esq., communicated by the President.

ART. XIV. ANALYSIS OF SCIENTIFIC BOOKS.

Philosophical Transactions of the Royal Society of London, for the Year 1821. *Part I.*

IN the limits assigned to this portion of our Journal, it will not be possible to enter into a detailed account of the important papers contained in the present half-volume of the Philosophical Transactions; we shall therefore aim at presenting our readers with a succinct view of the principal novelties which they contain, and with the heads of the leading subjects which they discuss.

The chemical papers, once forming so prominent and important a feature in the publications of the Royal Society, have lately suffered a serious defalcation, both in number and interest: in the part now before us there is, however, a very interesting essay on the compounds of chlorine and carbon, by Mr. Faraday, the chemical assistant in the laboratory of the Royal Institution: there is also a communication from Dr. Henry on the aëriform compounds of charcoal and hydrogen, which may be considered as supplementary to a memoir on the same class of bodies inserted in the Transactions for 1808.

Physiology was so favourite a study with the late president, and his means of patronising and propagating the pursuit were so extensive, and generally so well directed, that the Transactions, during the long series of years which he filled the chair of the Society, were the depositary of the principal investigations and discoveries in that department of knowledge; and comparative anatomy, which may be regarded as its basis, was proportionally promoted under the same auspices; it is, however, remarkable, when the botanical propensities of the late Sir Joseph Banks are taken into the account, that the structure and physiology of vegetables have been very sparingly and imperfectly noticed in the publications of the scientific body over which he presided; whereas the anatomy and functions of the animal creation are prominently treated of, and sumptuously illustrated, in almost every volume of the Transactions published during the last five-and-twenty years. For these contributions we are principally indebted to the indefatigable exertions of Sir Everard Home, who has thus shown that the abstract pursuits of science are not incompatible with laborious and extensive surgical avocations, and who, in furnishing the student with an example of the true means of acquiring professional celebrity, has also opened to himself an inexhaustible mine of occupation and amusement during those hours of leisure and retirement which declining years necessarily bring with them. This part of the Transactions contains two communications from Sir Everard upon physiological subjects; under the same head we may class an account of the urinary organs of two species of frogs, by Dr. Davy; and two letters addressed to the President, showing a singular influence of the male upon the progeny of the female.

Geology has not of late years constituted any prominent feature in the Transactions of this Society; this is partly to be attributed to the speculative tone of much that has been written upon this subject, and which renders it unfit for the Transactions of a society for the *promotion* of natural knowledge; and partly to the magnificent publications of the Geological Society; in which, with much that is light and trifling, there are many papers of standard excellence and laborious research. How far this and other similar philosophical associations affect the general interests of British science, is a question of such pith and moment, as to require extended and serious consideration. In our opinion, (which has certainly not been hastily, and we trust not superficially formed) with much good, they have been productive of more harm; they have in some instances perhaps brought talents into the field which might otherwise have lain dormant; they have also excited a temporary interest in the welfare and pursuit of science among a class of persons whose names are only known as contributors to the funds of the establishments; but then, they have created petty dissensions and paltry jealousies among those who once were friends and colleagues, and have tended to scatter and subdivide the forces of science in a way most unpropitious to its true welfare. We remember when all that was eminent in philosophy and excellent in art was to be found at the weekly meeting of the Royal Society in Somerset-Place; he who wanted information sought and found it there; strangers assembled to converse with the learned of the land; and, though a hostile breeze now and then ruffled the good understanding and friendly intercourse of the members, it soon subsided into a prosperous calm. But now, the forces of science are not merely scattered but disunited, and among philosophers as among politicians, we have, under other names, tories, whigs, and radicals. He who wants to consult the learned and examine the records of science, is driven from the east to the west, and from the north to the south; up stairs, down stairs, and even sometimes into my lady's chamber, before he can find the object of his search and inquiry; losing that time which might be occupied in digesting and extending his information, in traversing the most opposite quarters of the metropolis, mounting into back-rooms up two pair, calling in Moor-fields for what is only to be had in Albemarle-street, or the agreeable reverse; and at length finding that, for want of method and good understanding, he loses a week in London in search of that which a day would effect in Paris. We, however, by no means intend to hold up the literary and scientific establishments of France as deserving unalloyed praise or unqualified imitation; there is about them, that jobbing servility, and cringing subserviency, which betrays their shackles and dependance.

But, not to lose time in the further enumeration of the serious and multiplied inconveniences which obviously result from the divisions and subdivisions into which the scientific establishments of the metropolis are split, let us, briefly as may be, inquire whether any tangible remedy is at hand, which, while it includes the advantages of the disunion, may at the same time heal the dissensions which it creates, and fill up the chasms which it has induced.

Should government be seriously memorialized and appealed to in behalf of the semi-bankrupt state of our scientific bodies in general? should we humbly pray relief, and suggest the benefit that would accrue from the establishment of honorary rewards, and other distinctions, which very moderate pecuniary aid would confer? Should they be told that the apartments of the Royal Society in Somerset-House are altogether inadequate; that they are out of repair; that the valuable library is scattered through various rooms for want of one large enough to receive it; that the Society has been obliged to dispose of a variety of instruments and apparatus in consequence of the expense of hiring an apartment for their preservation? should not the Society at once pray for a palace in the new street, appropriately convenient and magnificent, and humbly request that the sum of 50,000*l.* be further granted to the body to defray various necessary expenses, and to establish a fund for its future exigencies? Should we not hint at the *Jardin des Plantes*, and its sumptuous museum and amphitheatre; at the palace of the Institute, and the magnificent hall of the Louvre appropriated to its members. To this and much more, the Chancellor of the Exchequer might reply nearly as follows: "Gentlemen; His Majesty's government has at all times shewn its disposition to promote the objects of science when fit opportunity has presented itself, but under existing circumstances, I should not feel justified in advising the increased expenditure which the adoption of your request would involve: the Parisian establishments, to which you have alluded, have not conferred such exaltation and improvement upon science as to render them objects of our imitation, for with means derived from private sources, you have achieved conquests in the dominions of knowledge which are without equal; and as neither the late Mr. Cavendish, nor Sir Joseph

Banks, who, as president of the Royal Society during a very extended period, must have been well acquainted with the real interests of science, thought fit to bequeath a small fraction of their enormous property, to the establishment of scientific prizes, or to any other similar purpose, it would be presumption on the part of His Majesty's government to adopt a measure, which, however plausible in appearance, must be open to objections and liable to misapplication." We do not mean to consent to this reply, because we are convinced that even in the present state of things, a very few thousand pounds, granted with discernment to the calls of science, would produce effects incalculably beneficial; we are also convinced that the Rumford and Copley medals, which are so judiciously bestowed by the council of the Royal Society *, have been productive of much honourable emulation; but yet we think the opposite argument fairly inferable from the premises.

If, however, we are not in error, the remedy of the evils adverted to is not necessarily dependant upon extraneous aid. That the Royal Society, as the parent establishment, claims precedence, will not be disputed; let it therefore stand at the head of a Scientific College, and let the other Societies arrange themselves as committees, each as heretofore pursuing their individual avocations, under a chairman, a president, and officers of their own; let the meetings of the Royal Society, and of the various scientific committees, be held under the same roof, and their publications appear in the same volume, either as communications to the Royal Society, or as the Transactions of the various committees; let the books, instruments, and collections also be preserved in the same building; and let the expenses of the whole be defrayed from a general fund raised as heretofore, by subscriptions to the Society and its committees, but rendered infinitely more effective by concentration upon one object, instead of being frittered away in the small *items* of separate establishments.

We are quite aware that this scheme will be called Utopian and chimerical, but much that has been thought impossible has come to pass; the steam-engine has done wonders in this way; and when gas-lights were first talked of, we remember one of the most eminent and profound men of science in this or any other country, asserted that " a company might as well be formed for lighting London with a slice of the moon, as for carrying the gas from pit-coal through the streets of the metropolis." It is not therefore impossible, we even trust, not

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* We wish it had been permitted to us to quote Dr. Wollaston's admirable discourse to the Royal Society, upon the 30th of November last, when the Copley medal was adjudged to Professor Oersted of Copenhagen. It spoke volumes to the point before us.

improbable, that if there were that desirable union among scientific establishments to which we have adverted, the nation might at least afford such assistance as would enable them to be suitably, and even sumptuously, lodged. We have indeed heard that it is in contemplation to remove Carlton Palace, to continue the new street down to Westminster Abbey, and to erect in an open part of it, opposite to the Horseguards and Whitehall, a building appropriated to the Royal Society and the Royal Academy: if this be so, we trust that the British Museum will not be forgotten; and that its treasures will be removed to some repository less frail than that which now contains them; not one of those evanescent combinations of lath, plaster, cement and brickbats, which adorn Waterloo-Place, but a good honest stone building, which, in our opinion, could not be better situated than upon the site of the present edifice. It has, indeed, been rumoured, that the Duke of Bedford has thrown difficulties in the way of such an undertaking; but surely the ungracious impediment of which we heard could not come directly from a person possessed of less patriotism than his Grace, for the houses which surround the garden of the present Museum would be rather improved than injured by the erection of a series of courts and galleries for the reception of the various collections; there is "rottenness" somewhere, or something would, long ere this, have been accomplished towards redressing those grievances of which we have only given a scanty outline, and of which not only the scientific world, but the public at large, have a right to complain.

The casual mention of the Geological Society led us to this digression, which is not, we hope, altogether misplaced in an examination of the labours of the Royal Society, to which we now revert. One geological paper is contained in this part of the transactions, relating to a subject which requires more attention than it has received; and if the author be not in error, he has discovered a fact of much curiosity, and some importance. The mountain limestone of Plymouth lies directly upon clay-slate, and is remarkably scanty in organic remains; it includes certain caverns, perfectly insulated, some of which are incrusted with stalactite, and present nothing remarkable. In others, Mr. Whidbey, the author of this communication, has discovered certain fossil bones; these caves were without any stalactitical incrustation, having only a little dry clay at the bottom, and we beg the reader's attention to the following particulars: "The cavity was *entirely* surrounded by compact limestone rock, about eight feet above high-water mark, fifty-five feet below the surface of the rock, one hundred and seventy-four yards from the original face of the quarries, and about one hundred and twenty yards in that direction from the spot where the former bones were found in 1816." The bones, as appears from Sir E. Home's annexed catalogue, are those of the rhinoceros, the bear, and an animal of the deer kind, and of another animal of the size of the bear. Mr. Whidbey abstains from any speculations respecting the catastrophe by which these bones were thus entombed in the solid rock, and properly enough rests content with a naked statement of what he considers to be the fact.

The late volumes of the *Philosophical Transactions* have abounded in important contributions to physical science, and the part before us is, in this respect, not deficient; it includes a paper on the magnetic phænomena produced by electricity, by Sir H. Davy; two important communications, results of the accurate diligence of Captain Kater; and a notice from the same, respecting a volcanic appearance in the moon; and lastly, an account of a micrometer made of rock crystal, by Mr. Dollond.

Having thus generally surveyed the contents of this volume, we shall proceed in a few instances to particularize their details.

i. *On the black Rete Mucosum of the Negro, being a Defence against the scorching Effects of the Sun's Rays.* By Sir E. Home, Bart., F.R.S.

As black surfaces become infinitely more heated than white, by exposure to the sun's rays, it has long puzzled physiologists to account for the black colour of the negro; or to develope the ends attained by that intensity of colour bestowed by the unerring hand of nature upon the inhabitants of tropical climates. In this paper Sir Everard has rendered it probable that the blistering and irritation which Europeans, not accustomed to much exposure, suffer from the scorching rays of an equatorial sun, is attributable to a peculiar effect of the solar radiant matter, unconnected with the heat which it excites. By the black colour of the negro's skin the radiant matter of the sun is absorbed, and converted into sensible or thermometric heat, and is thus disarmed of its mischievous tendency.

If we expose the back of the hand to very intense sunshine, uncovered, or covered with any thin white tissue, it becomes inflamed and blistered; but if similarly exposed under a covering of black, it suffers no inconvenience, though the temperature in the former case falls far short of that excited in the latter.

Having described a variety of experiments to this point, Sir Everard very properly takes opportunity to observe that "the same wise Providence which has given so extraordinary a provision to the negro for the defence of his skin while living within the tropics, has extended it to the bottom of the eye, which otherwise would suffer in a greater or less degree when exposed to a strong light, the retina from its transparency allowing it to pass through without injury." "That the nigrum pigmentum is not necessary for vision, but only provided as a defence against strong light, is proved by its being darker in the Negro than in the European, and lighter in fair people than in dark, and therefore lightest in those countries farthest removed from the effects of the sun." "In the monkey it is dark, and in all animals that look upwards. In all birds, exposed to the sun's rays, it is black. In fishes, the basking shark which lies upon the surface of the ocean, has a nigrum pigmentum. The turbot and skate which lie upon banks of sand in shallow water, have nigrum pigmentum. In all ruminating animals and birds of prey, there is a broad tapetum at the bottom of the eye. The owl, that never sees the sun, has no nigrum pigmentum." "The *coup de soleil* I attribute to the scorching effects of the sun's rays upon the scalp. The Egyptian ophthalmia I consider to be the effect of the sun's rays, and the glare of reflected light."

ii. *On the Magnetic Phænomena produced by Electricity, in a Letter from* Sir H. Davy, Bart., F.R.S., *to* W. H. Wollaston, M.D., P.R.S.

The intimate connexion, if not the identity, of magnetism and electricity, has long been suspected, and indeed partly demonstrated; the recent discoveries of M. Oersted have completely proved the subsisting relation of these powers of matter, and have set a number of persons to work at the repetition and extension of his inquiries. In our last Number we have translated M. Biot's communication to the Parisian Academy of Sciences upon this subject, and upon a former occasion (vol. x. p. 361,) our readers have been presented with an outline of the recent discoveries connected with it. Since the date of this paper, Sir Humphry has himself extended his interesting experiments and observations; and in July last, he read a second memoir to the Royal Society, in which much that is left incomplete and undetermined in the paper now before us, is perfected and cleared up; of this we shall give an account in due course.

iii. *Communication of a singular Fact in Natural History. By the Right Hon. the* Earl of Morton, F.R.S.

iv. *Particulars of a Fact nearly similar to that related by* Lord Morton, *by* Daniel Giles, Esq.

Under these mysterious titles the following facts will be found. Some years ago Lord Morton, desirous of domesticating the quagga, endeavoured to procure a male and female of that species, but succeeded only in obtaining a male; he therefore tried to breed from the male quagga and a chestnut mare, and the result was a female hybrid of decidedly mixed origin; the mare was afterwards bred from by a black Arabian horse, and this latter produce exhibits in colour and mane a striking resemblance to the quagga. It is curious enough that features not belonging to the dam should be communicated through her to the progeny of another male.

Mr. Giles says, "I will now give the best account I can of my sow and her produce." She was of Mr. Western's black and white breed. Ten years ago she was bred from by a boar of the wild breed, of a deep chestnut colour; the pigs, (her first litter,) were duly mixed, the colour of the boar being in some of them very predominant. The sow was afterwards twice bred from by Mr. Western's boars, and in both instances chestnut marks were prevalent in the litter, which in other instances had never presented any appearance of the kind.

v. *The Croonian Lecture. Microscopical Observations on the following Subjects: On the Brain and Nerves; showing that the Materials of which they are composed exist in the Blood. On the Discovery of Valves in the Branches of the Vas Breve, lying between the villous and muscular Coats of the Stomach. On the Structure of the Spleen. By Sir* EVERARD HOME, *Bart. V. P. R. S.*

This lecture will be read with much interest by those who are bold enough to fathom the depths of modern physiology, and who put that confidence in Mr. Bauer's microscopical observations which we think they deserve. He examined the optic nerve and found it to consist of many bundles of fine fibres, formed of very minute globules united by a soluble transparent jelly. "By the discovery of this transparent substance," says Sir Everard, "we become acquainted with the nature of the medullary structure of the nerves, and can form some idea of their action, which till now I confess myself to have been totally unacquainted with. The nerves as well as the retina are composed of this newly-discovered transparent substance which is very elastic and soluble in water, and globules of $\frac{1}{2800}$ and $\frac{1}{4000}$ parts of an inch in diameter. Its transparency and solubility account for its having remained concealed; and were it not coagulable, in which state it becomes opaque, its existence might even now be considered as equivocal." The brain is also, as Mr. Bauer's observations show, a conglomerate of globules and soluble mucus, the former arranged into fibres and bundles, held together by the latter. It is pervaded by blood-vessels, but the arteries never anastomose, and the veins, which are very small, are supplied with valves, and perform the office of lymphatics, carrying the absorbed matter into the superior longitudinal sinus.

Our readers are probably aware that among metaphysicians there is some slight discrepancy of opinion respecting the head-quarters of the sensorium and the seat of the soul. Physiologists have also differed respecting the functions of the various parts of the brain; after the trash that has lately been current upon these subjects, we feel particularly indebted to Sir Everard for the following sterling facts:

> "That the cortical part of the brain is the seat of memory, is an opinion I have long entertained, from finding that any continued undue pressure upon the upper anterior part of the brain entirely destroys memory, and a less degree materially diminishes it. Pressure upon the dura mater, where the skull has been trepanned, puts a temporary stop to all sense, which is restored the moment that pressure is removed; and the organ appears to receive no injury from repeated experiments of this kind having been made. In hydrocephalus, when the fluid is in large quantity, and there only remains the cortical part of the brain and pons Verolii connecting it to the cerebellum, all the functions go on, and the memory can retain passages of poetry, so as to say them by heart; but a violent shake of the head produces instant insensibility. Pressure in a slight degree upon the sinciput, produced in one case complete derangement, with violent excess of the passion of lust, both of which went off upon removing, by the crown of the trepan, the depressed bone."

A little further on, adverting to the abundance and office of the transparent mucus, Sir Everard says, "There can be no doubt that the communication of sensation and volition more or less depend upon it." Indeed, it is evident that those functions cannot be ascribed to any individual component of the brain and nerves, but belong to them as entire structures.

The remainder of this part of the lecture is taken up in attempting to show that the above-mentioned mucus exists ready formed in the blood, and that it is the medium "by which the colouring matter is attached to the surface of the red globules," and that fat may exist in the blood.

Having dismissed the structure of the brain, the next portion of this lecture is devoted to a subject which some of our readers will perhaps consider as of paramount importance; namely, to the provision for carrying off the fluids taken into the stomach, whenever the quantity or quality interferes with the process of digestion.

> "To do this by the route of the thoracic duct, was not only too circuitous to correspond with the general simplicity of the operations of nature, but was mixing these heterogeneous liquids in too crude a state, with the general circulation of the blood. That there was some unusual mode of conveying fluids from the stomach to the urinary bladder, I have upon a former occasion established, since they arrived there when both the pylorus and thoracic duct were tied up, and the spleen was removed out of the body; but till the fact of valvular vessels supplying the office of absorbents was ascertained, any opinion respecting the route of fluids from the stomach, must continue to be entirely hypothetical."

Our author then proceeds to demonstrate the existence of such vessels, and to describe their situation and appearance, aided by engravings of Mr. Bauer's admirable drawings.

"To show the course of the absorbed fluids, as well as to give a clear idea of every thing connected with so *important* a discovery, a drawing of the spleen, the vas breve, and cardiac portion of the stomach, is annexed [Pl. IV;] and as the trunk of the splenic vein forms one of the trunks of the vena portæ, the liquids are directly carried to the liver, forming a part of the materials employed in producing the bile; the remainder only returning by the vena cava to the heart.

"This additional quantity of liquids passing along the splenic vein, accounts for its being five times the size of the artery, as well as for the blood in that vein having a greater proportion of serum than the blood in any other, which has been long asserted, and which I found by actual experiment to be the case; but being unable to account for it, as I can now, I was willing to admit that the mode of measuring might be erroneous."

The remaining section of this lecture is dedicated to the structure and uses of the spleen. It is shown to consist of blood-vessels, between which there is no cellular membrane, the interstices being filled with serum, and with the colouring matter of the blood from the lateral orifices in the veins when these vessels are distended, which serum is afterwards removed by the absorbents belonging to the organ, and carried into the thoracic duct by a very large absorbent trunk; so that from this mechanism "the spleen appears to be a reservoir for the superabundant serum, lymph globules, soluble mucus, and colouring matter, carried into the circulation immediately after the process of digestion is completed."

vi. *On two new Compounds of Chlorine and Carbon, and on a new Compound of Iodine, Carbon, and Hydrogen. By* Mr. M. FARADAY, *Chemical Assistant in the Royal Institution.*

In a former number of our Journal we have abstracted the material facts contained in this paper*, (vol. x., p. 386;) our readers will there observe that the author has described the properties of two new compounds of chlorine and carbon; in conjunction with Mr. Phillips, he has more recently ascertained the existence of another combination of the same elements in different proportions, so that chlorine and carbon which a year ago were not known to be susceptible of chemical union, are now shown to constitute three distinct compounds.

The various direct and collateral researches by which the nature and properties of these bodies are determined and illustrated, are amply and explicitly given in the paper before us, which we consider as a truly valuable contribution to che-

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* It has also been published at length in Mr. Phillips's *Annals of Philosophy.*

mical science; Mr. Faraday is simple and perspicuous without being superficial or hasty; he has sometimes handled the more abstract parts of chemistry, but has always kept clear of that affected jargon which characterizes a set of writers that may be designated the *Berzelian School*, and who are fond of mystifying and obscuring the simplest facts and doctrines of the science, by symbols and algebra: nothing is easier than to write a, b, c, instead of 1, 2, 3; to represent carbon by a great C, and chlorine by a little c, and then to call the chloride of carbon $C + c$, and the bichloride $C + c^2$, and so forth; but what then? chemistry is not yet ripe for these innovations; and when it is, they must come from abler heads than those of their present propounders.

vii. *An Account of the Comparison of various British Standards of linear Measure.* By Captain HENRY KATER, F.R.S.

Captain Kater's communications to the Royal Society have of late been numerous and important; they are not, generally speaking, of a popular cast, and have not therefore attracted that general notice which is bestowed upon productions carrying more sail with less ballast; independently too of their intrinsic value, they are written in a style peculiarly clear and intelligible. Nothing is hastily thrust forward in an unfinished state, with the mere view of pre-occupying the ground; no immature opinions are started for the purpose of anticipating the results of others engaged in similar pursuits *.

The paper before us, one only of several connected with the same subject, contains the details of a laborious series of inquiries connected with the Reports of the Commissioners of Weights and Measures; for the third and last of which we refer to the last volume of this *Journal*, p. 378. Our readers will also there find the results of some further inquiries instituted by Captain Kater, relative to the re-measurement of Sir George Shuckburgh's cube, sphere, and cylinder, the details of which he has also given to the Royal Society, whose Transactions are certainly the fit repository for these and all similar documents.

viii. *An Account of the Urinary Organs and Urine of two Species of the genus Rana.* By JOHN DAVY, M.D. F.R.S.

The bull-frog, *(rana taurina,* Cuvier,*)* and the brown toad, *(bufo fuscus,* Laurenti,) are the two species to which the author's

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* These remarks are suggested to us by the sweeping and unbounded surmises in which some cotemporary *sçavans* are accustomed to indulge, for

observations are limited in this paper, which is dated from Ceylon: the former inhabits the lake of Colombo; the latter abounds in the houses and streets of the Pettah. The specific gravity of the urine of the bull-frog is about 1003, and common salt, with a little phosphate of lime, and a trace of urea, are the only ingredients detected in it. The specific gravity of the urine of the brown-toad is 1008; its constituents similar to the above, but the proportion of urea more considerable. For the anatomical facts we must refer to the paper itself; the following physiological observations are important in relation to the treatment of urinary disorders; as such we quote them at length.

> "Perhaps additional facts are not required to prove, that the secretion of the kidneys of animals depends more on the intimate and invisible structure of these organs, than on the kind of food the animals consume; were such facts wanting, there would be no difficulty in furnishing them. How different is the urine of the brown-toad and that of any species of small lizards! yet flies are the favourite and common diet of both animals. Other remarkable instances might be mentioned, of similarity of diet and difference of urinary secretion; and, *vice versâ*, instances might be afforded of difference of diet and similarity of urine: I will mention one only; it is that of parrots and snakes; their urine, as I have found, being much the same, consisting chiefly of uric acid, though their diet is altogether different, the birds feeding entirely on vegetable matter, and the reptiles entirely on animal matter. But let me not be supposed to maintain that the urinary secretion depends entirely on the organ, quite independent of the nature of the food or of the blood, from which the elements of the urine are derived. It appears to be pretty satisfactorily proved, that *cæteris paribus*, there is a certain relation between the nature of the food and of the urine. Whilst this has been generally admitted, the relation between the organ and the secretion has been less insisted on, though perhaps not less curious and deserving of attention."

ix. *An Account of a Micrometer made of Rock Crystal.* By G. Dollond, F.R.S.

Rock crystal has been applied to telescopes in various ways for the purposes of micrometrical measurements: Mr. Dollond's improvement consists in making a sphere or lens from a piece of rock crystal, and adapting it to a telescope in the place of the usual eye-glass, and from its natural double refracting property rendering it useful as a micrometer.

The advantages of this application of the crystal consist in the saving of the time required to find the proper angle for cutting it, and working its surfaces with sufficient accuracy; in being able to take the angle on each side zero, without reversing the eye-tube, and also to take intermediate angles

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the mere purpose, as we presume, of laying claim to the merit of future discoveries, to which they are less entitled than the Marquis of Worcester to the invention of the steam-engine, or Roger Bacon to that of gunpowder.

between zero and the greatest separation of the images without exchanging any part of the eye-tube, it being only required to move the axis in which the sphere is placed. It also possesses the property of an eye-tube or lens not intended for micrometrical purposes, for when the axis of the crystal is parallel to that of the object glass, only one image will be formed. A plate is annexed to this paper, illustrative of the contrivance.

x. *The Bakerian Lecture: on the best kind of Steel and Form for a compass-needle.* By Captain HENRY KATER, F.R.S.

On the return of the expedition under the command of Captain Ross, which sailed in search of a north-west passage, it appeared that the compasses became nearly useless from the diminution of directive force, in consequence of the vicinity of the magnetic pole. Some of the azimuth compasses then employed were of Captain Kater's invention; he was therefore naturally anxious that the next expedition, (under the command of Lieutenant Parry,) should be furnished with instruments combining as much power and sensibility as possible; with this intention, our author undertook the experiments which form the subject of the valuable practical document before us, and from which he draws the following inferences:

"1. That the best material for compass-needles is *clock spring;* but care must be taken in forming the needle to expose it as seldom as possible to heat, otherwise its capability of receiving magnetism will be much diminished.

"2. That the best form for a compass-needle is the *pierced rhombus,* in the proportion of about five inches in length to two inches in width, this form being susceptible of the greatest directive force.

"3. That the best mode of tempering a compass-needle is, first to harden it at a red heat, and then to soften it from the middle to about an inch from each extremity, by exposing it to a heat sufficient to cause the blue colour which arises again to disappear.

"4. That in the same plate of steel of the size of a few square inches only, portions are found varying considerably in their capability of receiving magnetism, though not apparently differing in any other respect.

"5. That polishing the needle has no effect on its magnetism.

"6. That the best mode of communicating magnetism to a needle, appears to be by placing it in the magnetic meridian, joining the opposite poles of a pair of bar magnets (the magnets being in the same line), and laying the magnets so joined flat upon the needle with their poles upon its centre; then having elevated the distant extremities of the magnets, so that they may form an angle of about two or three degrees with the needle, they are to be drawn from the centre of the needle to the extremities, carefully preserving the same inclination, and having joined the poles of the magnets at a distance from the needle, the operation is to be repeated ten or twelve times on each surface.

"7. That in needles from five to eight inches in length, their weights being equal, the directive forces are nearly as the lengths.

"8. That the directive force does not depend upon extent of surface, but in needles of nearly the same length and form, is as the mass.

"9. That the deviation of a compass needle occasioned by the attraction of soft iron, depends, as Mr. Barlow has advanced, on extent of surface, and is wholly independent of the mass, except a certain thickness of the iron, amounting to about two-tenths of an inch, which is requisite for the complete developement of its attractive energy."

xi. *Notice respecting a Volcanic Appearance in the Moon, in a Letter addressed to the President.* By Captain Henry Kater, F.R.S.

We have heard of volcanos in the moon, but when we consider the fallacies to which observations of her surface are liable, even when made with the most experienced eyes and perfect instruments, we are induced to doubt the identity of most of these appearances with terrestrial volcanic phenomena, and to applaud Captain Kater for the caution shewn in the title of this letter, where he calls the luminous spot which he discovered, not a volcano, but a *volcanic appearance.* On Sunday evening, the 4th of February, 1821, the moon being two days old, and the evening clear, Captain Kater observed a luminous spot in the dark part of her orb; its appearance was that of a small nebula, subtending an angle of three or four seconds, and of variable brightness. " A luminous point," says our author, " would suddenly appear in its centre, and as suddenly disappear, and these changes would sometimes take place in the course of a few seconds. On the 5th, 6th, and 7th, it was again observed, but not afterwards." We should have mentioned that the telescope which Captain Kater used was a Newtonian, of 6¼ inches aperture, with a power of seventy-four. An engraving accompanies this paper.

xii. *A further Account of Fossil Bones, discovered in Caverns, enclosed in the Limestone Rocks at Plymouth.* By Joseph Whidbey, Esq.

We have already given the essence of this paper; it includes Mr. Whidbey's account of the situation of the caverns, and Sir Everard Home's catalogue of the relics, which we think are not quite properly called fossil bones; this, however, might easily be determined by chemical analysis. They are deposited in the museum of the College of Surgeons.

xiii. *On the Aëriform Compounds of Charcoal and Hydrogen, with an account of some additional Experiments on the Gases from Oil and from Coal.* By William Henry, M.D. F.R.S., *&c.*

The labours of Dr. Henry have tended more perhaps than those of any other individual to elucidate the nature and properties of those compound inflammable gaseous bodies of which charcoal and hydrogen are the principal constituents, and which are evolved during the destructive distillation of various vegetable and animal products. Much, however, still remains to be effected; it remains to be ascertained whether there are two or more definite combinations of carbon and hydrogen, or whether the supposed varieties are merely mixtures of one definite compound (olefiant gas) with variable proportions of pure hydrogen; the exact nature, too, of the gases from coal and oil is by no means made out; indeed we do not know whether the anomalies which they present to the analyst, are referable to the presence of some highly inflammable and volatile oily vapour, or to the existence of some hitherto unexamined compound of hydrogen and carbon; we are inclined to lean to the former opinion, for the illuminating power of the gas from oil is diminished by violent compression; by subjection to intense cold; by long exposure to water; and by passing it through alcohol; and in all these cases it loses oily matter, and requires consequently less oxygen for its perfect combustion. Whether there be two definite compounds of carbon and hydrogen, or one only, is a question which should be determined before the other ramifications of this inquiry can be satisfactorily traced; in the Bakerian lecture for 1820, Mr. Brande has advanced the former opinion; Dr. Thomson was the first to oppose it in the late edition of his *System*; but we have already canvassed and quashed his criticism[*].

Dr. Henry also argues in favour of the existence of light hydrocarbonate; such parts of his argument as hinge upon Dr. Thomson's authority we shall not notice; the others are chiefly the following. The gas obtained from the clayey bottom of a stagnant pool contained $\frac{1}{20}$ its volume of carbonic acid, and $\frac{1}{15}$ of nitrogen; the pure portion required two volumes of oxygen for combustion, and gave one volume of carbonic acid; its specific gravity was about 556. Now as these results agree with those obtained by Sir H. Davy in his analysis of *fire-damp*, Dr. Henry says there appears no reason for refusing to consider carburetted hydrogen (light) as a true chemical compound, and were any additional argument necessary to establish its definite nature, he observes that it might be derived from the action of water, which absorbs it in a constant proportion, and again evolves it unaltered when heated. But with great deference for Dr. Henry's opinion, we suspect that this latter point has not been sufficiently verified, and even if admitted, we doubt whether it would justify the inference which is here drawn from it.

[*] Vol. XI., page 145.

Dr. Henry then says, that "the process by which carburetted hydrogen gas is evolved in natural operations is no doubt the decomposition of water, and admits of being explained on the atomic theory of Mr. Dalton, by supposing two atoms of charcoal to act at once on two atoms of water," *&c.* Now we confess that we have great doubts as to the above origin of carburetted hydrogen gas, and rather suspect that the decomposition of vegetable and animal matter, is its principal, if not exclusive, source in nature; arising perhaps in coal-mines, from the action of whin dikes upon the coal, as suggested by Dr. Hutton; and derived in stagnant pools from the decomposition of the organic relics contained in the mud. As to the absence of carbonic oxide in the products of stagnant water, we would, in the first place, ask, has its absence been satisfactorily ascertained? and, secondly, admitting that it does not exist, why should we expect the presence of a compound which neither fermentation, putrefaction, nor respiration produce, and which is only sparingly and equivocally generated during the action of heat on pit-coal?

The next portion of Dr. Henry's paper relates to the action of chlorine on carburetted hydrogen; a subject, putting the purity of the carburetted hydrogen out of the question, so full of difficulties, so mixed up with the joint agency of light, heat, and water, upon the mixed gases, that we cannot presume here to review it in all its bearings, and shall therefore rest content with thanking Dr. Henry for the new facts which he has stated in relation to it.

In his "*Experiments on the gas from oil,*" Dr. Henry has shewn that the gas obtained at different times from oil of the same quality is by no means uniform in composition. The readers of this Journal will probably require an apology for this mere mention of the "oil question;" but the following remark, coming from a person of Dr. Henry's experience and accuracy, we think weighty in relation to it. "So far," he says, "as my experience goes, no temperature short of ignition is sufficient for the decomposition of oil into permanent combustible gases; but the lower the heat that is employed, provided it be adequate to the effect, the heavier and more combustible is the gas, and the better suited to artificial illumination." This paragraph is also important to the manufacturer of oil gas for domestic use, but least he should be led into error, we would observe that although Dr. Henry is perfectly correct, it still does not answer to the manufacturer to obtain the gas in Messrs. Taylors' stoves, at the lowest possible temperature, that is, a dull red heat, for at that temperature, much empyreumatic condensible vapour is produced, and the bulk of the gas proportionably diminished: and though it be perfectly true that the increased bulk of the gas obtained at higher temperatures is partly referable to the decomposition, and consequent expansion of the olefiant portion, and therefore attended by a certain diminution of illuminating power, still, as far as economy is concerned, it will be found advantageous to make oil gas at a bright, or cherry-red, heat.

The next section of Dr. Henry's paper contains the results of his experiments on coal gas, which are conducted with his usual skill and patience, but the analyses are so much affected by the questions discussed in the concluding section, that we shall prefer employing our remaining space in calling the reader's attention to the author's "*inferences respecting the composition of that part of the gas from coal and oil, which is condensed by contact with chlorine.*" By a variety of experiments, Dr. Henry considers it as proved, that that portion of oil and coal gas, which is condensible by chlorine, (light being excluded), is of a specific gravity of 1.4 to 1.2, and consequently, that it is not pure olfiant gas. Again, both Dr. Henry and Mr. Dalton, agree that the portion of oil and coal gas condensible by chlorine, requires for its entire combustion not three volumes of oxygen (which would indicate olefiant gas,) but four volumes and a half; so that as our author himself remarks—

> "It is evident from these facts that the aëriform ingredient of oil gas and coal gas, which is reducible to a liquid form by chlorine, is not identical with the olefiant gas obtained by the action of sulphuric acid on alcohol, but considerably exceeds that gas in specific gravity and combustibility. Two views may be taken of its nature; for it may either be a gas *sui generis*, hitherto unknown, and constituted of hydrogen and charcoal in different proportions from those composing any known compound of those elements; or it may be merely the vapour of a highly volatile oil, mingled in various proportions with olefiant gas, carburetted hydrogen, and the other combustible gases. Of these two opinions, Mr. Dalton is inclined to the first, considering it as supported by the fact that oil gas, or coal gas, may be passed through water, without being deprived of the ingredient in question; and that this anomalous elastic fluid is absorbed by agitation with water, and again expelled by heat or other gases, unchanged as to its chemical properties, as we have both satisfied ourselves by repeated experiments. On the other hand, I have found that hydrogen gas, by remaining several days in narrow tubes in contact with fluid naphtha, acquires the property of being affected by chlorine, precisely as if it were mixed with a small proportion of olefiant gas; and I am informed by Dr. Hope, that oil gas, when forcibly compressed in Gordon's portable gas lamp, deposits a portion of a highly volatile essential oil. The smell also of the liquid which is condensed on the inner surface of a glass receiver, in which oil gas or coal gas has been mixed with chlorine, denotes the presence of chloric ether, evidently however mingled with the odour of some other fluid, which seems to me to bear most resemblance to that of spirit of turpentine. This part of the subject is well worthy of further investigation; but having devoted to the inquiry all the leisure which I am now able to command, I must remain satisfied at present with such conclusions as are safely deducible from the foregoing investigation."

We entirely agree with Dr. Henry that this subject deserves further investigation, and as no one can be better qualified for its prosecution than himself, we trust that he will resume and complete that which he has so ably commenced. It is not only interesting as an abstract chemical inquiry, but may also prove important in its relations to the economy of gas illumination.

C.

ART. XV. ASTRONOMICAL AND NAUTICAL COLLECTIONS. No. VII.

i. *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. XI. p. 182.]

SECTION II.

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

§ 40.

Now the chord k'' being (§ 7) $= \surd\,([x''' - x']^2 + (y'''-y')^2 + (z''' - z')^2)$; if we develope this formula, and remember that $r'^2 = x'^2 + y'^2 + z'^2$, and $r'''^2 = x'''^2 + y'''^2 + z'''^2$, we shall find $k'' = \surd\,(r'^2 + r'''^2 - 2x'\,x''' - 2y'\,y''' - 2z'\,z'')$: and since we had $x' = \varrho'\cos\alpha' - \mathrm{R}'\cos \mathrm{A}'$, $y' = \varrho'\sin\alpha' - \mathrm{R}'\sin \mathrm{A}'$ $z' = \varrho'\,\mathrm{tang}\,\beta'$; and $x''' = \mathrm{M}\varrho'\cos\alpha''' - \mathrm{R}'''\cos \mathrm{A}'''$, $y''' = \mathrm{M}\,\varrho \sin\alpha''' - \mathrm{R}'''\sin \mathrm{A}'''$, $z'' = \mathrm{M}\varrho'\,\mathrm{tang}\,\beta'''$; we obtain $x'\,x'' + y'\,y''' = \mathrm{R}'\,\mathrm{R}'''\cos(\mathrm{A}''' - \mathrm{A}') - \varrho'\,\mathrm{R}'''\cos(\mathrm{A}''' - \alpha') - \mathrm{M}\varrho'\,\mathrm{R}'\cos(\mathrm{A}' - \alpha''') + \mathrm{M}\varrho'^2\cos(\alpha''' - \alpha';)$ and $z'\,z''' = \mathrm{M}\varrho'^2\,\mathrm{tang}\,\beta'\,\mathrm{tang}\,\beta'''$; so that the whole equation becomes $k''^2 = r'^2 + r'''^2 - 2\,\mathrm{R}'\,\mathrm{R}'''\cos(\mathrm{A}'''-\mathrm{A}') + 2\,\varrho'\,\mathrm{R}''\cos(\mathrm{A}'''-\alpha') + 2\mathrm{M}\,\varrho'\,\mathrm{R}'\cos(\mathrm{A}' - \alpha''') - 2\,\mathrm{M}\varrho'^2\cos(\alpha''' - \alpha') - 2\,\mathrm{M}\varrho'^2\,\mathrm{tang}\,\beta'\,\mathrm{tang}\,\beta'''$: for which we may write $k = \surd\,(\mathrm{F} + \mathrm{G}\varrho' + \mathrm{H}\varrho'^2)$.

§ 41.

Now T being the time between the first and third observation, we have, from LAMBERT's very elegant theorem,

$\mathrm{T} = \dfrac{1}{12\,m}\left([r' + r'' + k'')^{3} - (r' + r''' - k'')^{\frac{3}{2}}\right)$. But if we substituted, in this equation, the values of r', r''' and k'', we should arrive at an equation of enormous difficulty: it might, however, be reduced to the 12th degree by substituting for the equation of LAMBERT the approximation of DUSE'JOUR, $\mathrm{T}^2 = \dfrac{(r' + r''')\,k''^2}{4f}$; and even to the 6th, by supposing $\dfrac{r' + r'''}{2} = \surd\,\dfrac{r'^2 + r'''^2}{2}$, which is, however, only at all admis-

sible when r' and r''' differ but little from each other, and k'' and T are very small. There is, however, no occasion for these awkward abbreviations; for although the value of ϱ' cannot be found immediately from LAMBERT's formula, yet it may be obtained by a few easy trials, since we have three equations,
$r' = \sqrt{(R'^2 - 2\,R' \cos{(A' - \alpha')}\,\varrho' + \sec^2\beta'\,\varrho'^2)}$
$r''' = \sqrt{(R'''^2 - 2\,R''' \cos{(A''' - \alpha''')}\,M\varrho' + \sec^2\beta'''\,M^2\,\varrho'^2)}$
$k'' = \sqrt{(F + G\varrho' + H\,\varrho'^2)}$; in all which the coefficients of ϱ' are known magnitudes, expressed in numbers; so that we have only to assume a value for ϱ', and we obtain those of r' r''' and k'' by the extraction of the square root only. From these we may find without difficulty, by means of the table of the descent towards the sun in a parabola, or by an easy direct reckoning, the time that ought to elapse between the observations, according to the assumed value of ϱ'. This time, compared with the observed time, immediately shews whether we ought to increase or diminish the value of ϱ', in order to come nearer to the observation. In this manner we approximate rapidly to the truth, and may at last employ a simple interpolation. It will seldom be necessary to make more than four, or at most five, suppositions; and the first of them will not require any accuracy of calculation: at least the determination of the true value of ϱ' from these three equations will always be much more convenient than the solution of a single equation of the 6th degree.

§ 42.

As soon as the value of ϱ' is determined, the determination of all the elements readily follows; for the computation gives us at once r', r''', ϱ', and $\varrho''' = M\varrho'$. Now if we call the heliocentric latitudes, in the first and third observations, λ' and λ''', we have $\sin\lambda' = \frac{\tan\beta'\,\varrho'}{r'}$, and $\sin\lambda''' = \frac{\tan\beta'''\,\varrho'''}{r'''}$. If also we put ε', ε''' for the heliocentric elongations of the comet from the earth, we find $\sin\varepsilon' = \frac{\varrho'\sin{(A' - \alpha')}}{r'\cos\lambda'}$, $\sin\varepsilon''' = \frac{\varrho'''\sin{(A''' - \alpha''')}}{r''\cos\lambda'}$ whence we obtain the two heliocentric longitudes, which may be called C′ and C‴. If now we put $\cot\omega = \frac{\operatorname{tang}\lambda'''}{\operatorname{tang}\lambda' \sin(C''-C')} - \cot(C'''-C')$, we shall have ω the distance of the comet in longitude, at the time of the first observation, from the ascending node; and consequently $C'-\omega$ the longitude of the node. The inclination of the orbit is obtained from the formula $\operatorname{tang} i = \frac{\operatorname{tang}\lambda'}{\sin\omega}$. For the two heliocentric distances of the comet from its node in the plane of its orbit, u', u''', we have $\cos u' = \cos\lambda' \cos\omega$, and $\cos u''' = \cos\lambda''' \cos(C'''-C'+\omega)$; and $u'''-u'$ is the difference of the two true anomalies. Now if we make φ the true anomaly, in the first observation φ, we have, from the known properties of the parabola, $\operatorname{tang}\frac{1}{2}\varphi = \cot\frac{u'''-u'}{2} - \operatorname{cosec}\frac{u'''-u'}{2} \cdot \sqrt{\frac{r'}{r'''}}$, whence we have the longitude of the perihelium; and the distance ϖ at the perihelium is found $= r' \cos^2 \frac{1}{2}\varphi$; the time may also readily be found either by direct computation, or by a table. [Or since we must previously have determined the values of r', r''' and k'', we may obtain that of ϖ from the equation A, Note 3; $= \frac{1}{4} \cdot \frac{k''^2-(r'''-r')^2}{r'+r'''-\sqrt{(r'+r''')^2-k''^2}}$, and then $\cos\varphi = \frac{2\varpi - r'}{r'} = \frac{2\varpi}{r'} - 1$. The longitude of the node may also be deduced from its tangent $\frac{y}{x} = \frac{y'z'''-y'''z'}{x'z'''-x'''z'}$, Note 2. Tr.] Note *communicated* by the Author. [It may be somewhat more convenient to employ the formula $\operatorname{tang}\left(\omega + \frac{C''-C'}{2}\right) = \frac{\sin(\lambda'''+\lambda')}{\sin(\lambda'''-\lambda')} \operatorname{tang}\left(\frac{C'''-C'}{2}\right)$.]

§ 43.

It is natural, as soon as ϱ' is found, to have the curiosity to calculate all the elements of the orbit, although there is no actual necessity to do so, until further corrections have been introduced; and, as Laplace has justly remarked, in so long a computation, we ought to spare ourselves every unnecessary labour. We might therefore be contented to determine either the longitude of the node and the inclination of the orbit, or the time and distance of the perihelium, according to the mode of correction which we might prefer. Thus if χ be $= u'' - u'$, or the angle formed by the two positions of the revolving radius, we have $\cos \chi = \frac{r'^2 + r'''^2 - k''^2}{2r'r'''}$, and thence φ by the formula $\text{tang}\ \frac{1}{2}\varphi = \cot \frac{1}{2}\chi - \sqrt{\frac{r'}{r'''}} \cdot \frac{1}{\sin \frac{1}{2}\chi}$; which enables us to determine the time and distance. We may also find φ more directly, since $\sin^2 \frac{1}{2}\chi = \frac{k'' - (r''' - r')^2}{4\,r'''r'}$ and $\cos^2 \frac{1}{2}\chi = \frac{(r''' + r')^2 - k''^2}{4r'''\,r'}$, whence $\cot^2 \frac{1}{2}\chi = \frac{(r''' + r')^2 - k''^2}{k''^2 - (r' - r')^2}$, consequently $\text{tang}\ \frac{1}{2}\varphi = \frac{\sqrt{([r''' + r']^2 - k''^2)} - 2r'}{r\,(k''^2 - (r''' - r')^2}$.

§ 44.

It will be convenient to recapitulate the formulæ immediately necessary for the computation, that the whole may be found together. We first find $m = \frac{\text{tang}\ \beta''}{\sin (A'' - \alpha')}$ then $M = \frac{(m \sin (A' - \alpha') - \text{tang}\ \beta')\,t''}{(\text{tang}\ \beta''' - m \sin [A'' - \alpha'''])\,t'}$, and the coefficients of

$$r'^2 = R'^2 - 2\,R' \cos\ (A' - \alpha')\ \varrho' + \sec^2 \beta' \varrho'^2$$

$$r'''^2 = R'''^2 - 2\ R \cos\ (A''' - \alpha''')\ M\varrho' + \sec^2 \beta''\ M^2 \varrho'^2$$

$$k''^2 = r'^2 + r'''^2 - 2\,R'\,R''' \cos\ (A''' - A') + 2\,R''' \cos\ (A''' - \alpha')\ \varrho' + 2\,M\,R'^2 \cos\ (A' - \alpha''')\,\varrho' - 2\,M \cos\ (\alpha''' - \alpha')\ \varrho'^2 - 2\,M\ \text{tang}\ \beta,$$

$\text{tang}\ \beta'''\ \varrho'^2$; hence we obtain ϱ' by a few trials, and from it all the other elements of the orbit, by means of the formulas in §. 42 and 43.

§ 45.

Even a superficial comparison of this method with any other that has been hitherto proposed, will be sufficient to show its superior conciseness and convenience. It has also the advantage of being universally applicable, whenever we have three observations of a comet. It is true that the proportion, in which the chords are supposed to be divided, is not mathematically correct; but this source of error is by no means very material: Euler and Lambert have supposed the same for the orbit of the comet; I have only extended the supposition to that of the earth, and by this extension the inaccuracy will never be considerably increased, and frequently even diminished. None of the direct methods which have been proposed is so accurate; for they all take for granted, either tacitly or expressly, that the portion of the comet's orbit is a right line described uniformly; or if the arcs be assumed, with Laplace, so small that the supposition creates no error, these arcs must be determined by means of an awkward interpolation. I shall also show, in the next section, how easily the error arising from the imperfection of the supposition may be corrected.

§. 46.

This method of calculation may, however, be more completely illustrated by an example; and I shall take for this purpose that of the comet of 1769, partly because the elements of its orbit are so well established, and partly because it has been the most frequently employed for an example of other methods. The observations are from Pingre''s Cométographie.

Times			α			β		
Sept.	4	14h	80°	56′	11″	17°	51′	39″ S
	8	14	101	0	54	22	5	2
	12	14	124	19	22	23	43	55

For these three observations we have

A			Log R
162°	42′	5″	.003132
166	35	31	.002665
170	20	20	.002184

Consequently $t' = t'' = 4$ days, $\frac{t''}{t'} = 1$, and $T = 8$ days.

The computation for M will stand thus

Log tang β''	9.608237
$-$Log sin $(A''-\alpha'')$	9.959299
$=$Log m	9.648938
$+\Big\{$Log sin $(A''-\alpha')$	9.998750
Log sin $(A''-\alpha''')$	9.827766
$=\Big\{$Log m sin $(A''-\alpha')$	9.647688
Log m sin $(A''-\alpha''')$	9.476704

Tang β'''	.43963
m sin $(A''-\alpha''')$	.29971
Tang $\beta'''-m$ sin $(A''-\alpha''')$	.13992
m sin $(A''-\alpha')$	.44431
Tang β'	.32221
m sin $(A''-\alpha')-$tang β'	.12210

Log .12210	9·086716
$-$Log .13992	9.145880
$=$Log M	9.940836

We next compute the formulas
$r'^2=R'^2-2\,R'\cos(A'-\alpha')\,\varrho'+\sec{}^2\beta'\,\varrho'^2$, and
$r'''^2=R'''^2-2\,R'''\cos(A'''-\alpha''')\,M\,\varrho'+\sec{}^2\beta'''\,M^2\,\varrho'^2$, which become $r'^2=1.01453-0.28854\,\varrho'+1.10393\,\varrho'^2$
$r'''^2=1.01011-1.21482\,\varrho'+.90869\,\varrho'^2$; then for the chord, $k^2=r'^2+r'''^2-2\,R'\,R'''\cos(A'''-A')+2\,R'''\cos(A'''-\alpha')\,\varrho'+2\,R'\cos(A'-\alpha''')M\varrho'-2\,M\cos(\alpha'''-\alpha')\,\varrho'^2-2\,\text{tang}\,\beta'\,\text{tang}\,\beta'''\,M\,\varrho'^2$; and the calculation stands thus

(1) Log R'	.003132	(2) Log R'''	.00218
$+$ Log R'''	.002184	$+$ Log cos $(A'''-\alpha')$	7.89400
$+$ Log cos $(A'''-A')$	9.995976	$=$ Log .007875	7.89618
$=$ Log 1.00298	0.001292		

(3) Log M	9.940836	(4) Log M	9.940836
$+$ Log R'	.003132	$+$ Log cos $(\alpha'''-\alpha')$	9.861377
$+$ Log cos $(A'-\alpha''')$	9.894274	$=$ Log .63418	9.802213
$=$ Log .689035	9.838242		

(5) Log M	9.940836
+ Log tang β'	9.508173
+ Log tang β'''	9.643090
= Log .12362	9.092099

Then adding twice each of these numbers to $r'^2+r'''^2$, we have

$$\begin{array}{ll} r'^2+r'''^2= & 2.02464-1.50336\,\varrho'+2.01262\,\varrho'^2 \\ & -2.00596+1.39382\,\varrho'-1.51560\,\varrho'^2 \\ \hline k''^2= & .01868-\ .10954\,\varrho'+\ .49702\,\varrho'^2 \end{array}$$

Our three equations therefore become

$$r'''=\surd(1.01011-1.21482\,\varrho'+\ .90869\,\varrho'^2)$$
$$r'=\surd(1.01453-\ .28854\,\varrho'+1.10393\,\varrho'^2)$$
$$k''=\surd(\ .01868-\ .10954\,\varrho'+\ .49702\,\varrho'^2).$$

In which if we take $\varrho'=1$, we have $r'=1.40...$, $r'''=.84...$, and $k''=.62...$, giving the time in which the area is described 26.88 days; while the observation gives only 8 : consequently this value of ϱ' is much too great.

We will therefore take $r=.5$; then r' will be 1.07, $r'''=.80$, and $k''=.297$, and the time 11.83 days, which is still too great. We may next try $\varrho'=\frac{1}{3}$; giving $r'=1.02$, $r'''=.84$, $k''=.194$, and the time 7.79 days, or somewhat too small. Hence it may be inferred that the true value of ϱ' cannot differ much from .35. We may therefore compute more accurately for .345 and .350, and we shall find

$\varrho'=.345$	$\varrho'=350$
$r'=1.02294$	$r'=1.02409$
$r'''=.83616$	$r'''=.83441$
$k''=.20012$	$k''=.20304$
T= 7.9271	T=8.0410

Consequently the error of the former supposition is $-.0729$, of the latter $+.0410$; whence the true value of ϱ' is .34820; and by an easy interpolation we find $r'=1.02367$, $r'''=.83504$; and Log $\varrho'''=$Log M $\varrho'=9.482665$.

§ 47.

In order to determine the whole orbit, we obtain the heliocentric latitudes from the formula.

$\text{Sin } \lambda = \frac{\varrho \text{ tang } \beta}{r}$; whence $\lambda' = 6°.17'.34''$, $\lambda''' = 9°.12'.19''$. For the elongations from the earth, we have

$\text{Sin } \varepsilon = \varrho\frac{\sin (A - \alpha)}{r \cos \lambda}$; hence $\varepsilon' = 19°.47'.47''$, and $\varepsilon'' = 15°.25'.16''$: consequently the heliocentric longitudes of the comet are $C' = 0.^s 2°.29'.5''$, and $C''' = 0^s.5°.54'.3''$. By means of the expression $\cot \omega = \frac{\text{tang } \lambda'''}{\text{tang } \lambda' \sin (C''' - C')} - \cot (C''' - C')$, we find $\omega = 7°.11'.45''$; whence the longitude of the descending node, since the latitudes are south, will be $C' - \omega = 0^s.2°.29'.5'' - 7°.11'.45'' = 11^s.25°.18'.7''$. The inclination is found, from the equation $\text{tang } i = \frac{\text{tang } \lambda'}{\sin \omega}, = 41°.21'.30''$. In the next place, we have $\cos u' = \cos \lambda' \cos \omega$, and $\cos u''' = \cos \lambda''' \cos (C''' - C' + \omega)$, consequently $u' = 9°.32'.54'$, $u''' = 14°.0'.40''$, and $u''' - u' = \chi = 4°.27'.46'$. Hence we find φ, taking the third observation, because it is nearer to the sun, by means of the formula $\text{tang } \frac{1}{2} \varphi = \cot \frac{1}{2} \chi - \sqrt{\frac{r'''}{r'}} \cdot \frac{1}{\sin \frac{1}{2} \chi}$; which gives $\frac{1}{2} \varphi = 67°.56'.12''$, and the true anomaly of the comet in the third observation $135°.52'.24''$. If we add to φ the distance of the comet from the ☋, or u''', $= 14°.0'.40''$, we obtain the distance of the perihelium from the descending node $= 149°.53'.4''$; consequently, the longitude of the perihelium is $4^s.25°.11'.11''$. The distance ϖ at the perihelium is $r''' \cos^2 \frac{1}{4} \varphi = .11782$: whence we find the time from the third observation to the perihelium, $=$ 24d.20h.22m.; and the time of the perihelium October 7, 10h.22m.

[*Note.*—It must here be remarked that it has been usual with all astronomers, since the time of Halley, to apply the term *Longitude of the Perihelium* to the sum of two angles lying contiguous to each other in different planes; one the longitude of the node, reckoned on the ecliptic, the other the distance of the perihelium from the node, measured on the plane of the orbit. To reduce the longitude of the perihelium to the ecliptic, in the common sense of the term, would be of no practical advantage. TR. From a letter of the AUTHOR.]

§ 48.

The elements thus found are these:

Longitude of the ☊ $5^s.25^\circ.18'.7''$
Inclination of the orbit $41^\circ.21'.30''$
Longitude of the perihelium $4^s.25^\circ.11'.11''$
Distance at the perihelium .11782
Time of the perihelium 1769 October 7 10h.22m.

If now we compare these elements with those which are already known, they will approach very near to them, and they agree almost exactly with those which Lambert has deduced, from observations, like these, before the perihelium, but computed with much greater labour and by frequent repetitions. The inclination is somewhat too great in both cases, probably from some irregularity of the observations. Pingre' has computed the orbit by Laplace's method from the same observations which I have employed: but the distance and time of the perihelium, which are the only elements that he has determined, differ much more from the truth than mine: and a very superficial comparison is sufficient to shew the superior conciseness of this method.

§ 49.

The errors of the method and those of the observations being combined in this example, I shall add a second, from which the latter are excluded. The following longitudes and latitudes of the comet of 1681 are not derived from observation, but computed by Halley, according to his parabolic theory of this comet; so that it will appear from this instance, how accurately we may determine again, by the method here explained, the distances from the earth and sun.

Times	α	β	A	Log R
h m	s ° ″ ′	° ″ ′	s ° ″ ′	
Jan. 5. 6 1½	0 8 49 49	26 15 15	9 26 22 18	9.99282
9. 7 0	0 18 44 36	24 12 54	10 0 29 2	9.99303
13. 7 9	0 26 0 21	22 17 30	10 4 33 20	9.99325

Consequently $t'=4.0411$, $t''=4.0055$, and $T=8.0466$. Hence we find Log $M=.137562$, and the three quadratic equations

$$r'=\surd(.96754-.59292\,\varrho'+1.24328\,\varrho'^2)$$
$$r''=\surd(.96941-.40185\,\varrho'+2.20087\,\varrho'^2)$$
$$k''=\surd(.019726-.122756\,\varrho'^2+.265982\,\varrho'^2)$$

Now assuming $\varrho'=1$, r' becomes 1.27, $r'''=1.65$, $k''=.40$; and $T=19.75$, instead of 8.0466, that is 11.7 days too much. If we take $\varrho'='.5$, we have $r'=.99$, $r'''=1.14$, $k''=.155$, and $T=6.15$ days, or 1.9 too little. Hence we may conclude, that ϱ' cannot be very different from .56; and we find for

$\varrho'=.56$	$\varrho'=.57$
$r'=1.01262$	$r'=1.01662$
$r'''=1.19773$	$r'''=1.20641$
$k''=.18546$	$k''=.19020$
$T=8.0121$	$T=8.2402$

The error of the former supposition is $-.0345$, the difference of the two values of T .2281: consequently, the true curtate distance ϱ' is .56151, and we have

$r'=1.0139$ $r'''=1.1991$.

Now, according to Halley's theory, the true values were $r'=1.0144$ and $r'''=1.2000$; so that our method gives these distances perfectly correct to the third place of decimals.

§ 50.

These examples are sufficient to show the convenience and conciseness and certainty of the proposed method; upon which I shall make a few further remarks. In order to determine the time from the distances and the chord, we have the formula $T=\frac{1}{12m}\left([r'+r'''+k''']^{\frac{3}{2}}-(r'+r'''-k'')^{\frac{3}{2}}\right)$; and in order to find it the more readily, tables have been computed upon these principles; taking $B=\frac{r'+r'''+k''}{2}$, and $D=\frac{r'+r'''-k''}{2}$, we find the respective times in the tables, and their difference, or, if the angle is greater than 180°, their sum gives the value of T. Such tables are to be found in the Berlin collection, but they are not very accurate: Pingré has improved and corrected them, in his *Cométographie.*

Since these tables are only calculated to hundredth parts of B and D, I have only found it convenient to employ them when no great accuracy is required, as in the first preliminary experiments with a value of ϱ'. If we wish for great precision, the proportional parts are troublesome, since the first differences alone are scarcely ever sufficient for the purpose. In this case it is much easier to compute the proper times immediately from B and D. This may be done very conveniently by the formulas Log $s' =$ Log B$+\frac{1}{2}$ log B$+1.4378117$ and Log $s'' =$ Log D $+\frac{1}{2}$ log D$+1.4378117$, the time T, in which the space in question is described, being expressed by $s'-s''$, in days. If, for instance, we take r', as in the last article, $=1,01262$, $r''' = 1.19773$, and $k'' = .18546$, the computation will stand thus

$r' =$ 1.01262
$+r''' =$ 1.19773
$=$ 2.21035
Half 1.10517
$\frac{1}{2}k''$ .09273
B= 1.19790
D= 1.01244

Log B	.078421	Log D	.005369
$\frac{1}{2}$ Log B	.039211	$\frac{1}{2}$ Log D	.002685
Const Log	1.437812	Const Log	1.437812
Log s'	1.555444	Log s''	1.445866
s'	35.9290	s''	27.9169

Hence $s'-s'' = T = 8.0121$ days. If we require the time to seconds, we must take out the fifth place of decimals, for $1'' = .0000116$, and .0001 day is $8''.64$.

§ 51.

In calculations of any length, it is always an advantage to have some check by which we may from time to time examine their accuracy. In the present instance we have several such expedients. At the end of the calculation it will be also of advantage to compute χ again from the elements which have been determined, and thence to find the geocentric longitude and latitude for the time of the middle observation: the one of these steps checks the calculation, at least the latter part of it, the other serves as a test of the accuracy of the elements of the orbit. I find, for instance, from the elements of the comet of 1769 determined in §§ 47, 48, for the 8 September 14h, the true anomaly $138^{\circ}.19'.55''$, and the logarithm of the distance from the sun 9.969155, whence the geocentric longitude is $3^{s}.10^{\circ}.5'.57''$, the latitude $22^{\circ}.5'.52''$ S. The error of longitude in $-2'57''$, in latitude $+30''$; quantities not at all considerable for the first rough estimate.

§ 52.

We have many tables for finding the true anomaly of a comet from the time given, and the time from the anomaly, which are to be found in astronomical works and collections. But the most convenient and complete is unquestionably that which is contained in a book not very bulky and but little known, though extremely valuable, entitled, *An Account of the Discoveries concerning Comets, by* Thomas Barker, Gent. 4. Lond. 1757. Pp. 54, 1 plate. The second table in this little work gives, for every five minutes of the true anomaly, the corresponding parabolic area, and the logarithm of the distance, with the first differences, supposing the distance at the perihelium unity; and hence the true anomaly and distance from the sun may be found for every comet, and for every given time, with perfect accuracy, by a calculation which is much simpler than that which is required by the common tables. It is much to be regretted that Barker's essay was not known to PINGRE'. The mode of finding the orbit, described in it, is that of NEWTON, adapted to calculation by the author, and illustrated by a particular specification of all the triangles and proportions required in it. We find three valuable observations of the great and celebrated comet of 1744, almost six weeks earlier than those which are more commonly known, communicated to BARKER by Morris; they are

			Long.			Lat. N.	
		s	°	′		°	′
1743 Oct.	22	2	26	46		7	35
	27	2	24	14		8	28
Nov.	1	2	21	25		9	26

The hour was not recorded, but BARKER supposes that it may have been about 8 or 9 in the evening, or 8h. 17m.; and he finds that these observations agree with the parabolic elements of the comet. [The table is also reprinted in *Sir Henry Englefield's* Essay *On the Determination of the Orbits of Comets,* according to *the Methods of Father Boscovich and M. de Laplace,* 4. Lond. 1793. Pp. 204, 4 plates; but without corrections: it is here presented in an improved form, having been completely re-calculated, as an appendix to Dr. OLBERS's Essay, together with a table of the anomaly in a very eccentric ellipsis, computed by a formula of LAPLACE.—Preface of the Editor, Mr. VON ZACH.]

§ 53.

Before I conclude this section, I must observe in the last place, that Mr. SCHULZE has proposed, in the *Memoirs of the Academy of Berlin* for 1782, a method which somewhat resembles mine in the principles on which it depends, and in the general progress of the calculation. But the computation is much more circuitous, and less convenient; principally because the chord of the earth's orbit is not supposed to be divided in proportion to the times, and because instead of the curtate distance from the earth, the distance of the comet from the sun in the first observation is employed as the principal unknown quantity to be determined. There is also a slight oversight with respect to the choice of observations. Mr. SCHULZE observes that LAMBERT has demonstrated that when the intervals are nearly equal, the revolving radius in the middle observation divides the chord of the comet's orbit very nearly in proportion to the times: "*pourvu qu'on emploie des observations assez distantes entr'elles.*" This might have been supposed to be an error of the press; but in applying his method to the comet of 1779, he actually selects the remotest observations which he possessed, making an interval of more than 80 days, and very naturally obtains elements of the comet from these, which differ from the truth in an unusual degree.

SECTION IV.

Correction of the Elements of an Orbit which has been determined.

§ 54.

Some correction of the method explained in the preceding section will always be required: partly on account of its own imperfection, and partly on account of the errors of observation, which must have so much the more effect on the elements as they must necessarily be taken at short intervals of time.

§ 55.

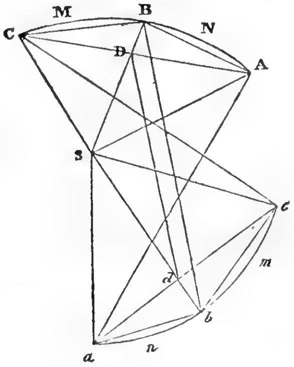
Supposing us to be in possession of a long series of observations, we may proceed immediately to employ the most distant of them for the correction of the elements, in a manner which will hereafter be explained. But if, as often happens, the comet has only been seen for two or three weeks, it will be sufficient to correct the errors depending on the mode of computation, by a process which is very easy and convenient: and in this case it will be best to begin with distances somewhat remote from each other, for instance, at an interval of 12, 14, or 16 days, especially when the apparent distance of the comet from the sun is not too small.

§ 56.

It has often been observed that our method would be mathematically correct if the revolving radii actually divided the chords of the orbits in the precise proportion of the times; for then in fact we should have correctly $\varrho'''=\mathrm{M}\varrho'$: since, however, this is seldom exactly true, we have in reality $\varrho'''=(\mathrm{M}+v)\varrho'+h$; and knowing the elements pretty nearly, we may now find the value of the corrections v and h.

§ 57.

For the orbit of the earth, we have $ad : dc=\mathrm{R}' \sin(\mathrm{A}''-\mathrm{A}) : \mathrm{R}''' \sin(\mathrm{A}'''-\mathrm{A}'')$; [since $\mathrm{S}a=\mathrm{R}'$ and $\mathrm{S}c=\mathrm{R}'''$, and the sines of the angles at d are the same in the two triangles compared].



For the orbit of the comet, having obtained ϱ' (§ 41), we must determine the time and distance for the perihelium, and then the true anomaly ψ, at the time of the middle observation. Hence, as we have already φ and χ, the difference of the true anomalies between the first and second observation being τ, and between the first and third σ, we have $\tau = \psi - \varphi$ and $\sigma = \varphi + \chi - \psi = \chi - \tau$; and then for the chord of the orbit of the comet we have $AD : DC = r' \sin \tau : r''' \sin \sigma$: [φ being however here the true anomaly at the time of the *first* observation].

[*To be continued.*]

vi. *An easy Method of computing the Aberration of the Stars. By the* Rev. John Brinkley, D. D., *Professor of Astronomy in the University of Dublin.*

Tables for finding the constant Arc and Maximum of Aberration in Declination for any Star.

Rule.

$$\text{Tab. I.} \times \sin \text{Delci.} * + \text{Tab. II.} = a$$

$$\text{Tab. III.} \times \sin \text{Decli.} * = a'$$

$$\tan \text{K (or Arc for Aberr. in Decli)} = \frac{a'}{a}$$

$$(\text{K nearer } 0^\circ \text{ or } 180^\circ)\ m = \frac{a}{\cos \text{K}}$$

$$(\text{K nearer } 90^\circ \text{ or } 270^\circ)\ m = \frac{a'}{\sin \text{K}}$$

Aberration in N.P.D. $= - m \cos (\text{K} \backsim \odot$ Long.

When a is $+$ & a' $-$ K is in 2d Quadrant.
a $+$ & a' $+$ K is in 3d Quadrant.
a $-$ & a' $-$ K is in 1st Quadrant.
a $-$ & a' $+$ K is in 4th Quadrant.

N. B. When Declination of Star is S. its sine is negative.

EXAMPLE.

To find K and *m* for Capella

AR 1820 = 2^s. 15° 51′
Decli. * 45° 48′ N

Tab. I = —1800 log.	1. 2553	Tab. III. = +4,96 log.	0. 6955
Decli. * log. sin *	9. 8555	Log. sin. Decli. *	9.8555
—12,90	1. 1108	+ *a*′ log.	10. 5510
Tab. II. = + 5,62		— *a* log.	0. 8627
a = —7,29 log.	0. 8627	tan.	9. 6883
Ar. Comp. cos. K	0. 0463	K = 11^s. 4° 0′	
m = 8,11 . .	0. 9090		

For Aberration in AR in Time.

Aberration in AR = *m* cos (K ∽ ⊙ Long.)

log tan K = log tan AR + log sec. 23°. 28′

K is in the Quadrant opposite to that in which AR is ⊙

log *m* = const. log. 0. 1303 + log sin AR + log sec D + log co-sec K

EXAMPLE.

AR α Cassiopeiæ = 7° 35′
Decli. = 55°. 33 N

tan. 7°. 35′		9. 1243
sec. 23 28		10. 0375
K = 6^s. 8°. 15′.	tan.	9. 1618

			s. ° ′	
const. log.	0. 1303	⊙ Long.	8. 5. 4.	
sin 7°. 35	9. 1205′	K	6. 8. 15.	
sec. 55°. 33	10. 2474′			
co sec : K	10. 8432	cos	1. 26.49	9. 7382
				0. 3414
m = 2, 20	0. 3414			
		Aberr. in AR = +1,20		0. 0796

TAB. I.

ARGUMENT.

AR. *

S.	9 . 3	10.4	11.5	S.	S.	9 . 3	10.4	11.5	S.
D.	+ −	+ −	+ −	D.	D.	+ −	+ −	+ −	D.
0	18,57	16,08	9,30	30	18	17,66	12,43	3,87	12
1	18,57	15,92	9,02	29	19	17,57	12,19	3,56	11
2	18,57	15,75	8,72	28	20	17,46	11,93	3,23	10
3	18,56	15,58	8,43	27	21	17,34	11,69	2,92	9
4	18,54	15,40	8,13	26	22	17,23	11,43	2,59	8
5	18,51	15,23	7,85	25	23	17,10	11,17	2,27	7
6	18,48	15,02	7,57	24	24	16,98	10,91	1,93	6
7	18,44	14,83	7,26	23	25	16,84	10,66	1,62	5
8	18,40	14,64	6,98	22	26	16,68	10,38	1,30	4
9	18,35	14,44	6,66	21	27	16,54	10,12	0,96	3
10	18,29	14,23	6,36	20	28	16,40	9,85	0,65	2
11	18,23	14,02	6,05	19	29	16,25	9,56	0,31	1
12	18,16	13,81	5,75	18	30	16,08	9,30	0,00	0
13	18,09	13,57	5,44	17	D	+ −	+ −	+ −	D
14	18,01	13,35	5,12	16					
15	17,94	13,14	4,80	15	S	8.2	7.1	6.0	S
16	17,85	12,91	4,50	14					
17	17,75	12,67	4,17	13					

TAB. II.

ARGUMENT.

Decli. *

D.	0	30	60	D.	0	30	60
D.	+	+	+	D.	+	+	+
0	8,06	6,98	4,02	15	7,80	5,70	2,08
1	8,06	6,92	3,90	16	7,76	5,60	1,96
2	8,06	6,84	3,78	17	7,72	5,50	1,82
3	8,06	6,76	3,66	18	7,68	5,40	1,68
4	8,04	6,68	3,54	19	7,64	5,30	1,54
5	8,04	6,60	3,40	20	7,58	5,18	1,40
6	8,02	6,52	3,28	21	7,54	5,08	1,28
7	8,00	6,44	3,16	22	7,50	4,98	1,14
8	7,98	6,36	3,02	23	7,42	4,86	0,98
9	7,96	6,28	2,90	24	7,36	4,74	0,84
10	7,94	6,18	2,76	25	7,30	4,62	0,70
11	7,92	6,10	2,64	26	7,24	4,52	0,56
12	7,90	6,02	2,48	27	7,18	4,40	0,42
13	7,88	5,92	2,36	28	7,12	4,28	0,28
14	7,84	5,82	2,22	29	7,06	4,14	0,14
				30	6,98	4,02	0,00

TAB. III.

ARGUMENT.

AR. *

S.	0 . 6	1 . 7	2 . 8	S.
D.	+ —	+ —	+ —	D.
0	20,25	17,54	10,12	30
1	20,25	17,36	9,82	29
2	20,23	17,17	9,50	28
3	20,22	16,98	9,19	27
4	20,20	16,78	8,87	26
5	20,17	16,59	8,55	25
6	20,14	16,38	8,23	24
7	20,10	16,17	7,90	23
8	20,06	15,96	7,58	22
9	20,01	15,74	7,24	21
10	19,95	15,51	6,92	20
11	19,89	15,28	6,59	19
12	19,82	15,05	6,25	18
13	19,73	14,81	5,92	17
14	19,65	14,57	5,58	16
15	19,56	14,32	5,24	15
16	19,47	14,07	4,90	14
17	19,37	13,81	4,55	13

S.	0 . 6	1 . 7	2 . 8	S.
D.	+ —	+ —	+ —	D.
18	19,26	13,55	4,21	12
19	19,15	13,29	3,86	11
20	19,04	13,01	3,51	10
21	18,90	12,75	3,16	9
22	18,77	12,47	2,81	8
23	18,64	12,19	2,47	7
24	18,50	11,91	2,11	6
25	18,36	11,62	1,76	5
26	18 20	11,32	1,42	4
27	18,04	11,02	1,06	3
28	17,88	10,73	0,71	2
29	17,71	10,42	0,35	1
30	17,54	10,12	0,00	0
D.	+ —	+ —	+ —	D.
S.	11 . 5	10 4	9 . 3	S.

Art. XVI.—*The Clock's daily Rate and Error at one time of the Day being known, a Table to find its corresponding Error throughout the day.* *By* James South, Esq. F.R.S., &c.

To the person attached to the pursuit of practical astronomy, the employment of observing is by no means an ungrateful one; but it too often happens that observations, when made, require some reductions or corrections before they can be converted to any useful purpose; a circumstance which has a considerable share in causing the science to be pursued with less advantage by private individuals, than it otherwise would be. Indeed, in this respect, astronomy seems to labour under greater inconveniences than any other department of natural knowledge, unless it be that of chemistry, in which perhaps the amusement of making experiments is sometimes not a little marred by the consciousness that the washing of apparatus, *&c.*, must be the inevitable consequence.

Unable altogether to remove the drudgery of reducing observations, it behoves us, where we can, to lessen it; and, for this purpose, I beg to offer the accompanying tables shewing the error of the clock for any part of the day, its rate and error at one time of it being known; nor can I allow this opportunity to escape me, without earnestly recommending the amateur observer, to ascertain the precise state of his clock every night, previous to his quitting his observatory. He will find that a constant and uniform attention to the clock, will insensibly entail upon him, habits of accuracy in his other observations; nor do I know any one thing which distinguishes the accurate, from the slovenly astronomer so much, as the manner in which the performance of the transit-clock, is watched and registered.

⁂ The calculations upon which this table is founded, were extended to five places of decimals, so that the third place here given may be considered correct; and, for the convenience of the travelling or nautical astronomer, a table for reducing sidereal to mean solar time is subjoined, for which I stand indebted to the industry of the late Rev. Mr. Wollaston.

H.	M.	0.″10	0.″11	0.″12	0.″13	0.″14	0.″15	0.″16	0.″17	0.″18	H.	M.
0	5	.000	.000	.000	.000	.000	.001	.001	.001	.001	0	5
	10	.001	.001	.001	.001	.001	.001	.001	.001	.001		10
	20	.001	.002	.002	.002	.002	.002	.002	.002	.002		20
	30	.002	.002	.003	.003	.003	.003	.003	.004	.004		30
	40	.003	.003	.003	.004	.004	.004	.004	.005	.005		40
	50	.003	.004	.004	.004	.005	.005	.006	.006	.006		50
1	0	.004	.005	.005	.005	.006	.006	.007	.007	.008	1	0
2	0	.008	.009	.010	.011	.012	.012	.013	.014	.015	2	0
3	0	.013	.014	.015	.016	.017	.019	.020	.021	.022	3	0
4	0	.017	.018	.020	.022	.023	.025	.027	.028	.030	4	0
5	0	.021	.023	.025	.027	.029	.031	,033	.035	.037	5	0
6	0	.025	.027	.030	.033	.035	.037	.040	.042	.045	6	0
7	0	.029	.032	.035	.038	.041	.044	.047	.050	.052	7	0
8	0	.033	.037	.040	.043	.047	.050	.053	.057	.060	8	0
9	0	.038	.041	.045	.049	.052	.056	.060	.064	.068	9	0
10	0	.042	.046	.050	.054	.058	.062	.067	.071	.075	10	0
11	0	046	.050	.055	.060	.064	.069	.073	.078	.082	11	0
12	0	.050	.055	.060	.065	.070	.075	.080	.085	.090	12	0
13	0	.054	060	.065	.070	.076	.081	.087	.092	.098	13	0
14	0	.058	.064	.070	.076	.082	.087	.093	.099	.105	14	0
15	0	.063	.069	.075	.081	.087	.094	.100	.106	.112	15	0
16	0	.067	.073	.080	.087	.093	.100	.107	.113	.120	16	0
17	0	.071	.078	.085	.092	.099	.106	.113	.120	.128	17	0
18	0	.075	.082	.090	.098	.105	.112	.120	.127	.135	18	0
19	0	.079	.087	.095	.103	.111	.119	.127	.135	.143	19	0
20	0	.083	.092	.100	.108	.117	.125	.133	.142	.150	20	0
21	0	.088	.096	.105	.114	.122	.131	.140	.149	.157	21	0
22	0	.092	.101	.110	.119	.128	.137	.147	.156	.165	22	0
23	0	.096	.105	.115	.125	.134	.144	.153	.163	.173	23	0

H.	M.	0″.19	0″.20	0″.21	0″.22	0″.23	0″.24	0″.25	0″.26	0″.27	H.	M.
0	5	.001	.001	.001	.001	.001	.001	001	.001	.001	0	5
	10	.001	.001	.001	.002	.002	.002	.002	.002	.002		10
	20	.003	.003	.003	.003	.003	.003	.003	.004	.004		20
	30	.004	.004	.004	.005	.005	.005	.005	.005	.006		30
	40	.005	.006	.006	.006	.006	.007	.007	.007	.008		40
	50	.007	.007	.007	.008	.008	.008	.009	.009	.009		50
1	0	.008	.008	.009	.009	.010	.010	.010	.011	.011	1	0
2	0	.016	.017	.018	.018	.019	.020	.021	.022	.023	2	0
3	0	.024	.025	.026	.028	.029	.030	.031	.032	.034	3	0
4	0	.032	.033	.035	.037	.038	.040	.042	.043	.045	4	0
5	0	.040	.042	.044	.046	.048	.050	.052	.054	.056	5	0
6	0	.048	.050	.053	.055	.057	.060	.063	.065	.067	6	0
7	0	.055	.058	.061	.064	.067	.070	.073	.076	.079	7	0
8	0	.063	.067	.070	.073	.077	.080	.083	.087	.090	8	0
9	0	.071	.075	.079	.083	.086	.090	.094	.097	.101	9	0
10	0	.079	.083	.087	.092	.096	.100	.104	.108	.112	10	0
11	0	.087	.092	.096	.101	.105	.110	.115	.119	.124	11	0
12	0	.095	.100	.105	.110	.115	.120	.125	.130	.135	12	0
13	0	.103	.108	.114	.119	.125	.130	.135	.141	.146	13	0
14	0	.111	.117	.123	.128	.134	.140	.146	.152	.157	14	0
15	0	.119	.125	.131	.138	.144	.150	.156	.162	.169	15	0
16	0	.127	.133	.140	.147	.153	.160	.167	.173	.180	16	0
17	0	.135	.142	.149	.156	.163	.170	.177	.184	.191	17	0
18	0	.043	.150	.157	.165	.172	.180	.188	.195	.202	18	0
19	0	.150	.158	.166	.174	.182	.190	.198	.206	.214	19	0
20	0	.158	.167	.175	.183	.192	.200	.208	.217	.225	20	0
21	0	.166	.175	.184	.193	.201	.210	.219	.227	.236	21	0
22	0	.174	.183	.192	.202	.211	.220	.229	.238	.247	22	0
23	0	.182	.192	.201	.211	.220	.230	.240	.249	.259	23	0

H.	M.	0″.28	0″.29	0″.30	0″.31	0″.32	0″.33	0″.34	0″.35	0″.36	H.	M.
0	5	.001	.001	.001	.001	.001	.001	.001	.001	.001	0	5
0	10	.002	.002	.002	.002	.002	.002	.002	.002	.003	0	10
0	20	.004	.004	.004	.004	.004	.005	.005	.005	.005	0	20
0	30	.006	.006	.006	.006	.007	.007	.007	.007	.008	0	30
0	40	.008	.008	.008	.009	.009	.009	.009	.010	.010	0	40
0	50	.010	.010	.010	.011	.011	.012	.012	.012	.013	0	50
1	0	.012	.012	.013	.013	.013	.014	.014	.015	.015	1	0
2	0	.023	.024	.025	.026	.027	.028	.028	.029	.030	2	0
3	0	.035	.036	.038	.039	.040	.041	.043	.044	.045	3	0
4	0	.047	.048	.050	.052	.053	.055	.057	.058	.060	4	0
5	0	.058	.060	.063	.065	.067	.069	.071	.073	.075	5	0
6	0	.070	.072	.075	.078	.080	.082	.085	.087	.090	6	0
7	0	.082	.085	.088	.090	.093	.096	.099	.102	.105	7	0
8	0	.093	.097	.100	.103	.107	.110	.113	.117	.120	8	0
9	0	.105	.109	.112	.116	.120	.124	.128	.131	.135	9	0
10	0	.117	.121	.125	.129	.133	.137	.142	.146	.150	10	0
11	0	.128	.133	.137	.142	.147	.151	.156	.160	.165	11	0
12	0	.140	.145	.150	.155	.160	.165	.170	.175	.180	12	0
13	0	.152	.157	.163	.168	.173	.179	.184	.190	195	13	0
14	0	.163	.169	.175	.181	.187	.192	.198	.204	.210	14	0
15	0	.175	.181	.188	.194	.200	.206	.213	.219	.225	15	0
16	0	.187	.193	.200	.207	.213	.220	.227	.233	.240	16	0
17	0	.198	.205	.212	.220	.227	.234	.241	.248	.255	17	0
18	0	.210	.217	.225	.233	.240	.247	.255	.262	.270	18	0
19	0	.222	.230	.238	.245	.253	.261	.269	.277	.285	19	0
20	0	.233	.242	.250	.258	.267	.275	.283	.292	.300	20	0
21	0	.245	.254	.263	.271	.280	.289	.298	.306	.315	21	0
22	0	.257	.266	.275	.284	.293	.302	.312	.321	.330	22	0
23	0	.268	.278	.287	.297	.307	.316	.326	.335	.345	23	0

H.	M.	0″.37	0″.38	0″.39	0″.40	0″.41	0″.42	0″.43	0″.44	0″.45	H.	M.
0	5	.001	.001	.001	.001	.001	.001	.001	.002	.002	0	5
0	10	.003	.003	.003	.003	.003	.003	.003	.003	.003	0	10
0	20	.005	.005	.005	.006	.006	.006	.006	.006	.006	0	20
0	30	.008	.008	.008	.008	.009	.009	.009	.009	.009	0	30
0	40	.010	.011	.011	.011	.011	.012	.012	.012	.012	0	40
0	50	.013	.013	.014	.014	0.14	.015	.015	.015	.016	0	50
1	0	.015	.016	.016	.017	.017	.018	.018	.018	.019	1	0
2	0	.031	.032	.033	.033	.034	.035	.036	.037	.038	2	0
3	0	.046	.047	.049	.050	.051	.053	.054	.055	.056	3	0
4	0	.062	.063	.065	.067	.068	.070	.072	.073	.075	4	0
5	0	.077	.079	.081	.083	.085	.088	.090	.092	.094	5	0
6	0	.093	.095	.098	.100	.102	.105	.108	.110	.113	6	0
7	0	.108	.111	.114	.117	.120	.122	.125	.128	.131	7	0
8	0	.123	.127	.130	.133	.137	.140	.143	.147	.150	8	0
9	0	.139	.142	.146	.150	.154	.158	.161	.165	.169	9	0
10	0	.154	.158	.163	.167	.171	.175	.179	.183	.188	10	0
11	0	.170	.174	.179	.183	.188	.193	.197	.202	.206	11	0
12	0	.185	.190	.195	.200	.205	.210	.215	.220	.225	12	0
13	0	.200	.206	.211	.217	.222	.228	.233	.238	.244	13	0
14	0	.216	.222	.228	.233	.239	.245	.251	.257	.263	14	0
15	0	.231	.237	.244	.250	.256	.262	.269	.275	.281	15	0
16	0	.247	.253	.260	.267	.273	.280	.287	.293	.300	16	0
17	0	.262	.269	.276	.283	.290	.297	.305	.312	.319	17	0
18	0	.278	.285	.293	.300	.307	.315	.323	.330	.338	18	0
19	0	.293	.301	.309	.317	.325	.333	.340	.348	.356	19	0
20	0	.308	.317	.325	.333	.342	.350	.358	.367	.375	20	0
21	0	.324	.332	.341	.350	.359	.367	.376	.385	.394	21	0
22	0	.339	.348	.358	.367	.376	.385	.394	.403	.413	22	0
23	0	.355	.364	.374	.383	.393	.403	.412	.422	.431	23	0

H.	M.	0″.46	0″.47	0″.48	0″.49	0″.50	0″.51	0″.52	0″.53	0″.54	H.	M.
0	5	.002	.002	.002	.002	.002	.002	.002	.002	.002	0	5
0	10	.003	.003	.003	.003	.003	.004	.004	.004	.004	0	10
0	20	.006	.007	.007	.007	.007	.007	.007	.007	.007	0	20
0	30	.010	.010	.010	.010	.010	.011	.011	.011	.011	0	30
0	40	.013	.013	.013	.014	.014	.014	.014	.015	.015	0	40
0	50	.016	.016	.017	.017	.017	.018	.018	.018	.019	0	50
1	0	.019	.020	.020	.020	.021	.021	.022	.022	.023	1	0
2	0	.038	.039	.040	.041	.042	.043	.043	.044	.045	2	0
3	0	.058	.059	.060	.061	.062	.064	.065	.066	.068	3	0
4	0	.077	.078	.080	.082	.083	.085	.087	.088	.090	4	0
5	0	.096	.098	.100	.102	.104	.106	.108	.110	.113	5	0
6	0	.115	.117	.120	.123	.125	.127	.130	.132	.135	6	0
7	0	.134	.137	.140	.143	.146	.149	.152	.155	.157	7	0
8	0	.153	.157	.160	.163	.167	.170	.173	.177	.180	8	0
9	0	.173	.176	.180	.184	.187	.191	.195	.199	.203	9	0
10	0	.192	.196	.200	.204	.208	.212	.217	.221	.225	10	0
11	0	.211	.215	.220	.225	.229	.234	.238	.243	.247	11	0
12	0	.230	.235	.240	.245	.250	.255	.260	.265	.270	12	0
13	0	.249	.255	.260	.265	.271	.276	.282	.287	.292	13	0
14	0	.268	.274	.280	.286	.292	.298	.303	.309	.315	14	0
15	0	.288	.294	.300	.306	.312	.319	.325	.331	.338	15	0
16	0	.307	.313	.320	.327	.333	.340	.347	.353	.360	16	0
17	0	.326	.333	.340	.347	.354	.361	.368	.375	.383	17	0
18	0	.345	.352	.360	.368	.375	.383	.390	.397	.405	18	0
19	0	.364	.372	.380	.388	.396	.404	.412	.420	.427	19	0
20	0	.383	.392	.400	.408	.417	.425	.433	.442	.450	20	0
21	0	.403	.411	.420	.429	.437	.446	.455	.464	.473	21	0
22	0	.422	.431	.440	.449	.458	.468	.477	.486	.495	22	0
23	0	.441	.450	.460	.470	.479	.489	.498	.508	.518	23	0

H.	M.	0″.55	0″.56	0″.57	0″.58	0″.59	0″.60	0″.61	0″.62	0″.63	H.	M.
0	5	.002	.002	.002	.002	.002	.002	.002	.002	.002	0	5
0	10	.004	.004	.004	.004	.004	.004	.004	.004	.004	0	10
0	20	.008	.008	.008	.008	.008	.008	.008	.009	.009	0	20
0	30	.011	.012	.012	.012	.012	.012	.013	.013	.013	0	30
0	40	.015	.016	.016	.016	.016	.017	.017	.017	.018	0	40
0	50	.019	.019	.020	.020	.021	.021	.021	.022	.022	0	50
1	0	.023	.023	.024	.024	.025	.025	.025	.026	.026	1	0
2	0	.046	.047	.048	.048	.049	.050	.051	.052	.053	2	0
3	0	.069	.070	.071	.073	.074	.075	.076	.077	.079	3	0
4	0	.092	.093	.095	.097	.098	.100	.102	.103	.105	4	0
5	0	115	.117	.119	.121	.123	.125	127	.129	.131	5	0
6	0	.138	.140	.143	.145	.147	.150	.153	.155	.158	6	0
7	0	.160	.163	.166	.169	.172	.175	.178	.181	.184	7	0
8	0	.183	.187	.190	.193	.197	.200	.203	.207	.210	8	0
9	0	.206	.210	.214	.218	.221	.225	.229	.232	.236	9	0
10	0	.229	.233	.238	.242	.246	.250	.254	.258	.263	10	0
11	0	.252	.257	.261	.266	.270	.275	.280	.284	.289	11	0
12	0	.275	280	.285	.290	.295	.300	.305	.310	.315	12	0
13	9	.298	.303	.309	.314	.320	.325	.330	.336	.341	13	0
14	0	.321	.327	.333	.338	.344	.350	.356	.362	.367	14	0
15	0	.344	.350	.356	.363	.369	.375	.381	.387	.394	15	0
16	0	.367	.373	.380	387	.393	.400	.407	.413	.420	16	0
17	9	.390	.397	.404	.411	.418	.425	.432	.439	.446	17	0
18	0	.413	.420	.428	.435	.442	.450	.458	.465	.473	18	0
19	0	.435	.443	.451	.459	.467	.475	.483	.491	.499	19	0
20	0	.458	.467	.475	.483	.492	.500	.508	.517	.525	20	0
21	0	.481	.490	.499	.508	.516	.525	.534	.542	.551	21	0
22	0	.504	.513	.523	.532	.541	.550	.559	.568	.578	22	0
23	0	.527	.537	.546	.556	.565	.575	.585	.594	.604	23	0

H.	M.	0″.64	0″.65	0″.66	0″.67	0″.68	0″.69	0″.70	0″.71	0″.72	H.	M.
0	5	.002	.002	.002	.002	.002	.002	.002	.002	.003	0	5
0	10	.004	.005	.005	.005	.005	.005	.005	.005	.005	0	10
0	20	.009	.009	.009	.009	.009	.010	.010	.010	.010	0	20
0	30	.013	.014	.014	.014	.014	.014	.015	.015	.015	0	30
0	40	.018	.018	.018	.019	.019	.019	.019	.020	.020	0	40
0	50	.022	.023	.023	.023	.024	.024	.024	.025	.025	0	50
1	0	.027	.027	.028	.028	.028	.029	.029	.030	.030	1	0
2	0	.053	.054	.055	.056	.057	.058	.058	.059	.060	2	0
3	0	.080	.081	.082	.084	.085	.086	.088	.089	.090	3	0
4	0	.107	.108	.110	.112	.113	.115	.117	.118	.120	4	0
5	0	.133	.135	.138	.140	.142	.144	.146	.148	.150	5	0
6	0	.160	.162	.165	.168	.170	.173	.175	.177	.180	6	0
7	0	.187	.190	.193	.195	.198	.201	.204	.207	.210	7	0
8	0	.213	.217	.220	.223	.227	.230	.233	.237	.240	8	0
9	0	.240	.244	.248	.251	.255	.259	.263	.266	.270	9	0
10	0	.267	.271	.275	.279	.283	.288	.292	.296	.300	10	0
11	0	.293	.298	.303	.307	.312	.316	.321	.325	.330	11	0
12	0	.320	.325	.330	.335	.340	.345	.350	.355	.360	12	0
13	0	.347	.352	.358	.363	.368	.374	.379	.385	.390	13	0
14	0	.373	.379	.385	.391	.397	.403	.408	.414	.420	14	0
15	0	.400	.406	.413	.419	.425	.431	.438	.444	.450	15	0
16	0	.427	.433	.440	.447	.453	.460	.467	.473	.480	16	0
17	0	.453	.460	.468	.475	.482	.489	.496	.503	.510	17	0
18	0	.480	,487	.495	.503	.510	.518	.525	.532	.540	18	0
19	0	.507	.515	.523	.530	.538	.546	.554	.562	.570	19	0
20	0	.533	.542	.550	.558	.567	.575	.583	.592	.600	20	0
21	0	.560	.569	.578	.586	.595	.604	.613	.621	.630	21	0
22	0	.587	.596	.605	.614	.623	.633	.642	.651	.660	22	0
23	0	.613	.623	.633	.642	.652	.661	.671	.680	.690	23	0

H.	M.	0″.73	0″.74	0″.75	0″.76	0″.77	0″.78	0″.79	0″.80	0″.81	H.	M.
0	5	.003	.003	.003	.003	.003	.003	.003	.003	.003	0	5
0	10	.005	.005	.005	.005	.005	.005	.005	.006	.006	0	10
0	20	.010	.010	.010	.011	.011	.011	.011	.011	.011	0	20
0	30	.015	.015	.016	.016	.016	.016	.016	.017	.017	0	30
0	40	.020	.021	.021	.021	.021	.022	.022	.022	.022	0	40
0	50	.025	.026	.026	.026	.027	.027	.027	.028	.028	0	50
1	0	.030	.031	.031	.032	.032	.033	.033	.033	.034	1	0
2	0	.061	.062	.063	.063	.064	.065	.066	.067	.068	2	0
3	0	.091	.092	.094	.095	.096	.098	.099	.010	.101	3	0
4	0	.122	.123	.125	.127	.128	.130	.132	.133	.135	4	0
5	0	.152	.154	.156	.158	.160	.163	.165	.167	.169	5	0
6	0	.183	.185	.188	.190	.192	.195	.198	.200	.203	6	0
7	0	.213	.216	.219	.222	.225	.227	.230	.233	.236	7	0
8	0	.243	.247	.250	.253	.257	.260	.263	.267	.270	8	0
9	0	.274	.277	.281	.285	.289	.293	.296	.300	.304	9	0
10	0	.304	.308	.313	.317	.321	.325	.329	.333	.338	10	0
11	0	.335	.339	.344	.348	.353	.358	.362	.367	.371	11	0
12	0	.365	.370	.375	.380	.385	.390	.395	.400	.405	12	0
13	0	.395	.401	.406	.412	.417	.423	.428	.433	.439	13	0
14	0	.426	.432	.438	.443	.449	.455	.461	.467	.473	14	0
15	0	.456	.462	.469	.475	.481	.488	.494	.500	.506	15	0
16	0	.487	.493	.500	.507	.513	.520	.527	.533	.540	16	0
17	0	.517	.524	.531	.538	.545	.553	.560	.567	.574	17	0
18	0	.548	.555	.563	.570	.577	.585	.593	.600	.608	18	0
19	0	.578	.586	.594	.602	.610	.618	.625	.633	.641	19	0
20	0	.608	.617	.625	.633	.642	.650	.658	.667	.675	20	0
21	0	.639	.647	.656	.665	.674	.683	.691	.700	.709	21	0
22	0	.669	.678	.688	.697	.706	.715	.724	.733	.743	22	0
23	0	.700	.709	.719	.728	.738	.748	.757	.767	.776	23	0

H:	M.	0″.82	0″.83	0″.84	0″.85	0″.86	0″.87	0″.88	0″.89	0″.90	H.	M.
0	5	.003	.003	.003	.003	.003	.003	.003	.003	.003	0	5
0	10	.006	.006	.006	.006	.006	.006	.006	.006	.006	0	10
0	20	.011	.012	.012	.012	.012	.012	.012	.012	.012	0	20
0	30	.017	.017	.018	.018	.018	.018	.018	.019	.019	0	30
0	40	.023	.023	.023	.024	.024	.024	.024	.025	.025	0	40
0	50	.029	.029	.029	.030	.030	.030	.031	.031	.031	0	50
1	0	.034	.035	.035	.035	.036	.036	.037	.037	.038	1	0
2	0	.068	.069	.070	.071	.072	.073	.073	.074	.075	2	0
3	0	.103	.104	.105	.106	.107	.109	.110	.111	.112	3	0
4	0	.137	.138	.140	.142	.143	.145	.147	.148	.150	4	0
5	0	.171	.173	.175	.177	.179	.181	.183	.185	.187	5	0
6	0	.205	.207	.210	.213	.215	.218	.220	.222	.225	6	0
7	0	.239	.242	.245	.248	.251	.254	.257	.260	.262	7	0
8	0	.273	.277	.280	.283	.287	.290	.293	.297	.300	8	0
9	0	.308	.311	.315	.319	.322	.326	.330	.334	.338	9	0
10	0	.342	.346	.350	.354	.358	.363	.367	.371	.375	10	0
11	0	.376	.380	.385	.390	.394	.399	.403	.408	.412	11	0
12	0	.410	.415	.420	.425	.430	.435	.440	.445	.450	12	0
13	0	.444	.450	.455	.460	.466	.471	.477	.482	.487	13	0
14	0	.478	.484	.490	.496	.502	.507	.513	.519	.525	14	0
15	0	.513	.519	.525	.531	.537	.544	.550	.556	.562	15	0
16	0	.547	.553	.560	.567	.573	.580	.587	.593	.600	16	0
17	0	.581	.588	.595	.602	.609	.616	.623	.630	.638	17	0
18	0	.615	.622	.630	.638	.645	.652	.660	.667	.675	18	0
19	0	.649	.657	.665	673	.681	.689	.697	.705	.713	19	0
20	0	.683	.692	.700	.708	.717	.725	.733	.742	.750	20	0
21	0	.718	.726	.735	.744	.752	.761	.770	.779	.787	21	0
22	0	.752	.761	.770	.779	.788	.798	.807	.816	.825	22	0
23	0	.786	.795	.805	.815	.824	.834	.843	.853	.862	23	0

H.	M.	0″.91	0″.92	0″.93	0″.94	0″.95	0″.96	0″.97	0″.98	0″.99	1″.00	H.	M.
0	5	.003	.003	.003	.003	.003	.003	.003	.003	.003	.003	0	5
0	10	.006	.006	.006	.007	.007	.007	.007	.007	.007	.007	0	10
0	20	.013	.013	.013	.013	.013	.013	.013	.014	.014	.014	0	20
0	30	.019	.019	.019	020	.020	.020	.020	.020	.021	.021	0	30
0	40	.025	.026	.026	.026	.026	.027	.027	.027	.028	.028	0	40
0	50	.032	.032	.032	.033	.033	.033	.034	.034	.034	.035	0	50
1	0	.038	.038	.039	.039	.040	.040	.040	.041	.041	.042	1	0
2	0	.076	.077	.078	.078	.079	.080	.081	.082	.083	.083	2	0
3	0	.114	.115	.116	.118	.119	.120	.121	.122	.124	.125	3	0
4	0	.152	.153	.155	.157	.158	.160	.162	.163	.165	.167	4	0
5	0	.190	.192	.194	.196	.198	.200	.202	.204	.206	.208	5	0
6	0	.228	.230	.233	.235	.237	.240	243	.245	.248	.250	6	0
7	0	.265	.268	.271	.274	.277	.280	.283	.286	.289	.292	7	0
8	0	.303	.307	.310	.313	.317	.320	.323	.327	.330	.333	8	0
9	0	.341	.345	.349	.353	.356	.360	.364	.367	.371	.375	9	0
10	0	.379	.383	.388	.392	.396	.400	.404	.408	.413	.417	10	0
11	0	.417	.422	.426	.431	.435	.440	.445	.449	.454	.458	11	0
12	0	.455	.460	.465	.470	.475	.480	.485	.490	.495	.500	12	0
13	0	.493	.498	.504	.509	.515	.520	.525	.531	.536	.542	13	0
14	0	.531	.537	.543	.548	.554	.560	.566	.572	.578	.583	14	0
15	0	.569	.575	.581	.588	.594	.600	.606	.612	.619	.625	15	0
16	0	.607	.613	.620	.627	.633	.640	.647	.653	.660	.667	16	0
17	0	.645	.652	.659	.666	.673	.680	.687	.694	.701	.708	17	0
18	0	.683	.690	.698	.705	.712	.720	.728	.735	.743	.750	18	0
19	0	.720	.728	.736	.744	.752	.760	.768	.776	.784	.792	19	0
20	0	.758	.767	.775	.783	.792	.800	.808	.817	.825	.833	20	0
21	0	.796	.805	.814	.823	.831	.840	.849	.857	.866	.875	21	0
22	0	.834	.843	.853	.862	.871	.880	.889	.898	.908	.917	22	0
23	0	.872	.882	.891	.901	.910	.920	.930	.939	.949	.958	23	0

H.	M.	2″.00	3″.00	4″.00	5″.00	6″.00	7″.00	8″.00	9″.00	10″.00
0	5	0.007	0.010	0.014	0.017	0.021	0.024	0.028	0.031	0.035
0	10	0.014	0.021	0.028	0.035	0.042	0.049	0.056	0.062	0.069
0	20	0.028	0.042	0.056	0.069	0.083	0.097	0.111	0.125	0.139
0	30	0.042	0.063	0.083	0.104	0.125	0.146	0.167	0.187	0.208
0	40	0.056	0.083	0.111	0.139	0.167	0.194	0.222	0.250	0.278
0	50	0.069	0.104	0.139	0.174	0.208	0.243	0.278	0.312	0.347
1	0	0.083	0.125	0.167	0.208	0.250	0.292	0.333	0.375	0.417
2	0	0.167	0.250	0.333	0.417	0.500	0.583	0.667	0.750	0.833
3	0	0.250	0.375	0.500	0.625	0.750	0.875	1.000	1.125	1.250
4	0	0.333	0.500	0.667	0.833	1.000	1.167	1.333	1.500	1.667
5	0	0.417	0.625	0.833	1.042	1.250	1.458	1.667	1.875	2.083
6	0	0.500	0.750	1.000	1.250	1.500	1.750	2.000	2.250	2.500
7	0	0.583	0.875	1.167	1.458	1.750	2.042	2.333	2·625	2.917
8	0	0.667	1.000	1.333	1.667	2.000	2.333	2.667	3.000	3.333
9	0	0.750	1.125	1.500	1.875	2.250	2.625	3.000	3.375	3.750
10	0	0.833	1.250	1.667	2.083	2.500	2.917	3.333	3.750	4.167
11	0	0.917	1.375	1.833	2.292	2.750	3.208	3.667	4.125	4.583
12	0	1.000	1.500	2.000	2.500	3.000	3.500	4.000	4.500	5.000
13	0	1.083	1.625	2.167	2.708	3.250	3.792	4.333	4.875	5.417
14	0	1.167	1.750	2.333	2.917	3.500	4.083	4.667	5.250	5.833
15	0	1.250	1.875	2.500	3.125	3.750	4.375	5.000	5.625	6.250
16	0	1.333	2.000	2.667	3.333	4.000	4.667	5.333	6.000	6.667
17	0	1.417	2.125	2.833	3.542	4.250	4.958	5.667	6.375	7.083
18	0	1.500	2.250	3.000	3.750	4.500	5.250	6.000	6.750	7.500
19	0	1.583	2.375	3.167	3.958	4.750	5.542	6.333	7.125	7.917
20	0	1.667	2.500	3.333	4.167	5.000	5.833	6.667	7.500	8.333
21	0	1.750	2.625	3.500	4.375	5.250	6.125	7.000	7.875	8.750
22	0	1.833	2.750	3.667	4.583	5.500	6.417	7.333	8.250	9.167
23	0	1.917	2.875	3.833	4.792	5.750	6.708	7.667	8.625	9.583

A TABLE,

SHEWING THE ACCELERATION OF SIDEREAL ON MEAN SOLAR TIME.

Hours	Min.	Sec.	M.	S.	Sec.	Min.	Sec.
1	0	9.830	0	5	0.014	28	4.587
2	0	19.659	0	10	0.027	29	4.751
3	0	29.488		20	0.055	30	4.915
4	0	39.388		30	0.082	31	5.077
5	0	49.147		40	0.109	32	5.242
6	0	58.977		50	0.135	33	5.406
7	1	8.806	1	...	0.164	34	5.570
8	1	18.636	2	...	0.328	35	5.734
9	1	28.466	3	...	0.491	36	5.898
10	1	38.296	4	...	0.655	37	6.061
11	1	48.125	5	...	0.819	38	6.225
12	1	57.955	6	...	0.983	39	6.389
13	2	7.784	7	...	1.147	40	6.553
14	2	17.614	8	...	1.311	41	6.717
15	2	27.443	9	...	1.474	42	6.881
16	2	37.273	10	...	1.638	43	7.044
17	2	47.102	11	...	1.802	44	7.208
18	2	56.932	12	...	1.966	45	7.372
19	3	6.761	13	...	2.129	46	7.536
20	3	16.591	14	...	2.293	47	7.700
21	3	26.420	15	...	2.457	48	7.863
22	3	36.250	16	...	2.621	49	8.027
23	3	46.080	17	...	2.785	50	8.191
24	3	55.909	18	...	2.949	51	8.355
			19	...	3.112	52	8.519
			20	...	3.276	53	8.681
			21	...	3.440	54	8.846
			22	...	3.604	55	9.040
			23	...	3.768	56	9.174
			24	...	3.932	57	9.338
			25	...	4.099	58	9.502
			26	...	4.259	59	9.665
			27	...	4.423	60	9.830

ART. XVII. *Miscellaneous Intelligence.*

I. MECHANICAL SCIENCE.

§ THE ARTS, MANUFACTURES, &c.

1. *Application of the Air Pump.*—Mr. John Oldham of the Bank of Ireland, has recently applied the air pump to many operations in the arts, and in which substances are to be impregnated with fluids, and with great success; thus, in the sizing of paper, for instance, the paper is piled up evenly in a vessel capable of being rendered air-tight, an exhaustion is made, the size introduced, and the air's pressure admitted; when the fluid passing into the pores of the paper rises it regularly, and without injury to the fabric. In the same way paper, silk, flax, cotton, and woollen staples, either raw, spun, or woven, may be dyed very regularly. In the processes of boiling, soaking, or pickling food, &c., this process may be used to remove the air and introduce the fluids, and its application is easy and evident in numerous similar cases. An apparatus of this kind is erected at the Bank of Ireland for wetting bank-note paper preparatory to its being printed on; an immense quantity of this paper is wetted without delay, and without any injury to the paper.

2. *Adhesive Pelt.*—Mr. William Wood of Bow, Middlesex, has discovered that a light felt of hide or hair, or mixture of hide, hair, and wool, when saturated with tar is highly elastic and water-proof, and conceiving the useful application of the substance as a lining for the sheathing of ships, he manufactures it in an expeditious and economical manner, in sheets of suitable size for that purpose; such sheets being attached to the external sides and bottom of the ship, by simply nailing with copper nails, are covered with planking. The substance he terms adhesive felt; it possesses the property of elasticity in so considerable a degree, as to stretch uniformly without fracture or injury either to its texture or its complete impermeability to water, whenever the ship's seams are opened by straining in hard weather, or in more dangerous cases of the starting of planks, or the breaking of timbers as in stranding. In all such cases this material forms an impenetrable and elastic case or garment for the whole ship's bottom, and in the case of the opening of seams by straining, it recovers its first dimensions with the return of the part so opened in the release of the strain; in such cases it generally falls into the openings in a certain degree so as to render them afterwards more secure against a recurrence. He also finds it to be a complete protection against every destruction of worm in all climates; this

destructive animal is never known to penetrate the material in the slightest degree. The hair, or hair and wool, is prepared for felting by the operation of dressing or bowing, as in the practice of hat-making, and is filted in the usual manner. Sheets or portions, thus felted, are dipped into the melted tar and pitch, certain stated proportions to each other, and then undergo a slight compression to take away the extraneous or dripping quantity of the material; they are then exposed for a short time to air to dry and cool, and are considered fit for use.—*New Monthly Magazine*, July, p. 347.

3. *The Apograph.*—This is a newly-invented instrument for copying drawings upon paper, copper, or any other substance capable of receiving an impression, and upon any scale. It is said, the arts furnish no instance of an instrument resembling it either in its operation or appearance, except, perhaps, the pantograph, from which it differs in the position of the beam in an horizontal plane, and in the addition of a counterpoise to return the hand when the beam is not vertical, and in other respects. Mr. Smith, of Ayr, is the inventor.

4. *New Madder Lake.*—Mr. Field, after seven years of much labour, has prepared a lake from madder which, in point of brilliancy and strength both for oil and water-colours has, till within a short time, had nothing comparable to it in the arts, it is also of a very durable nature.—*New Monthly Mag*. Sept.

5. *Contamination of Salt for Manufactories.*—The following question having been proposed to the Academy of Sciences by the French Ministry: "What are the processes to be adopted in contaminating common salt without injury to the soda manufactories, which will not permit of its re-appropriation to the uses of common life by any secret process, or at so little expense, as to make the chances or the profits encourage fraud?"

The academy in answering say, that it is impossible to resolve the question because of the high price of salt, but that the following means will render the fraud the most difficult.

1. Colour the salt by $\frac{1}{200}$ of wood charcoal.
2. Infect it by $\frac{1}{2000}$ of oil, distilled from animal substances, or by $\frac{1}{400}$ of tar.
3. To make the mixture in the magazines.

II. CHEMICAL SCIENCE.

§ CHEMISTRY, ELECTRICITY, MAGNETISM.

1. *On the Analysis of Alkaline Minerals*, by M. Berthier.—M. Berthier, remarks in his paper on this important part of Analytical Chemistry, that the use of barytes is inconvenient from the necessity there is of repeatedly heating it with the mineral to be analyzed before the action is complete, and uncertain from the

circumstance that every time an alkaline glass is heated very highly with fixed bases in quantity sufficient to combine with the silica a portion of the alkali is volatilized, amounting in some cases, even to one half. Boracic acid complicates the analysis, and especially that part of it which relates to the alkali.

Here M. Berthier has been induced to suggest, in place of either those substances, the use of oxide of lead, and has found it so convenient in his own experiments as to have employed it constantly for more than a year at l'Ecole des Mines. At first, carbonate of lead in the proportion of three to one of the stone was used, but on fusing this in platinum or silver vessels, though a mass was obtained perfectly soluble in acids, yet the crucibles were frequently acted upon, because a portion of lead was sometimes reduced by something in the stone, or by the fumes of the fuel.

To obviate this inconvenience, nitrate of lead was used, and the following is the process: The mineral is to be pulverized and then exactly mixed with two parts of nitrate of lead and one part of ceruse also pulverized. The mixture is to be placed in a small covered platinum crucible, and this placed in another crucible also to be covered; they are then to be heated red for a quarter of an hour without being uncovered: the fusion takes place tranquilly and without swelling, and a yellow or brown transparent liquid glass is obtained. The crucible is then taken out of the furnace, and its contents poured into water, as much of it being taken out with a spatula as may be, and then the crucible itself plunged into the water. The glass splits into small portions and is readily acted on by acids. It is then boiled with nitric acid, and from time to time the portions of silica are crushed with an agate or porcelain pestle. The silica remains in a gelatinous state. The lead is precipitated by sulphuric acid, the liquor being tested ultimately by sulphuretted hydrogen that no portion may remain. The solution is then boiled with carbonate of ammonia, and the precipitate analyzed by the ordinary method. Finally, the liquid is to be evaporated to dryness, the salts calcined in a platinum crucible, the residuum collected and weighed. This residuum contains the alkali in the state of sulphate, and almost always mixed with sulphate of magnesia. It may be treated as follows:

1. Precipitate the sulphuric acid by acetate of barytes, collect the precipitate and weigh it; precipitate the barytes by carbonate or oxalate of ammonia. Evaporate the clear solution to dryness, and collect the salt left: it will be a mixture of alkaline subcarbonate and caustic magnesia; weigh it, and expose it to the air. If it contains potassa it will deliquesce; wash off the alkali from the magnesia, and weigh the latter, then ascertain whether the alkaline carbonate be of potassa, soda or lithia. When the sulphates first obtained are neutral, it is sufficient to determine very

exactly the quantity of sulphuric acid and magnesia, to learn the nature of the alkali.

Or, 2. Precipitate the magnesia and sulphuric acid by barytes water, and the excess of barytes by carbonate of ammonia; evaporate, calcine, and the alkaline subcarbonate is left pure. The magnesia may be separated from the sulphate of barytes by means of an acid.

Or, 3. Precipitate the magnesia by lime water, the lime by carbonate or oxalate of ammonia, evaporate, calcine, then the magnesia is obtained immediately, and the alkali in the state of sulphate.

In examining a mineral for alkali, in the first instance, one part of it powdered may be mixed with three parts of ceruse, the mixture put into a crucible so large as to half fill it, that crucible placed in another, and the whole heated to whiteness, until it be perfectly fluid. Remove the small crucible, and when cold, collect the glass, and those parts to which it adheres. Pulverize it and boil in common muriatic acid. When it has acted sufficiently, evaporate to dryness, and then wash with repeated small portions of boiling water. The silica will be left with most of the lead in the state of muriate. Precipitate the solution by lime water, which throws down all the other earths and oxides, and then precipitate the lime by carbonate of ammonia, boiling both together, evaporate to dryness, add a little sulphuric acid to the residue and heat it, the alkali remains as a sulphate. If there be no magnesia present, the lime water may be spared, and the precipitation at once made with carbonate of ammonia.—*Ann. de Chim.* xvii. p. 28.

2. *Compounds of Molybdenum, Chromium, Tungsten, Antimony, &c.*—The following results are from a paper on the composition of several inorganic combinations by M. Berzelius. They are to be considered as the most accurate results to be obtained by that chemist.

Molybdate of lead	Molybdic acid	39.185	-	100
	Oxide of lead	60.815	-	155.2
Molybdic acid	Molybdenum .	66.613	-	100
	Oxygen . .	33.387	-	50.12
Chromate of lead	Chromic acid	31.853	-	100
	Oxide of lead	68.147	-	213.924
Chromic acid	Chromium .	53.98	-	100
	Oxygen . .	46.02	-	85.65
Chromic oxide (green)	Chromium .	70.11	-	100
	Oxygen . .	29.89	-	42.64
Sulphuret of tungsten	Tungsten . .	74.891	-	100
	Sulphur . .	25.109	-	33.53

The blue substance which remains after the decomposition by heat of tungstate of ammonia in close vessels, is of a particular nature. It dissolves in caustic alkalies rather more slowly than the yellow acid of tungsten, and loses its colour. The solution yields the ordinary tungstates. If heated in an open cylindrical vessel, it becomes yellow at the surface, but remains blue within; a proof that its change of colour is produced by the access of air. When heated until the yellow colour was perfect, it gained in weight, but the increase was never more than 0.0025, or at the very utmost 0.003 of its weight.

I have found that tungsten forms an oxide beneath the acid. It may be produced by passing hydrogen gas over red hot tungstic acid in a tube. When the hydrogen passes without depositing vapour of water, the acid is reduced to the state of oxide. This oxide is a chocolate-coloured powder which does not in the moist way combine either with acids or bases. It takes fire at a temperature much below redness, and burns like tinder: 100 parts of the oxide produce 107 of tungstic acid, from whence it follows that the oxygen in this oxide is to that in the acid as 2 to 3.

On the much disputed and uncertain subject of the oxides of antimony, M. Berzelius after remarking on the difficulty of giving an accurate result, states that he has found two data which are easy to verify and important in their evidence: the one is, that 100 of antimony oxidated by nitric acid, and heated red, and sufficiently to decompose all antimonic acid, gives 124.8 of antimonious acid. The second equally invariable point is, that when antimonious acid is mixed with pure antimony, pulverized and heated in close vessels without access of air, the acid is converted into fusible oxide combining with a portion of the mixed metal, equal to one-third of that in the antimonious acid, proving that the ratio between the oxygen in the oxide and that in the acid is as 3 : 4. The composition is as follows:

Oxide of Antimony	Antimony	84.32	100
	Oxygen	15.68	18.6
Antimonious acid	Antimony	80.13	100
	Oxygen	19.87	24.8
Antimonic acid	Antimony	76.34	100
	Oxygen	23.66	31

The oxygen being as 3, 4, and 5.

Silica	Silicum	49.641	49.716
	Oxygen	50.359	50.284
Deutoxide of Copper	Copper	79.825	100
	Oxygen	20.175	25.272

Annales de Chim. xvii. 1.

3. *Oxide of Chromium.*—If the hydrated green oxide of chro-

nium be heated by an alcohol lamp, it will lose its water, and become of a deep green, nearly black. Let it be weighed, and then heated to redness, it will appear to inflame at the moment vividly. The cold oxide is of a fine green, has lost nothing in weight, but is insoluble in acids, or at least the loss in weight is so small as not to surpass 0.0025, and is occasioned by a little acid that adheres to it, notwithstanding its being precipitated with excess of ammonia. The acid becomes evident to the smell at the moment of ignition. This phenomenon is of a similar nature with the ignition of many metallic antimoniates, gadolinite, and the hydrate of zircon.—*Berz. An. Chim.* xvii. 12.

4. *Carburet of Nickel.*—Carburet of nickel, when obtained by heating the oxide with resin and oil, is, according to Mr. Ross, a substance very much resembling plumbago in its appearance, but is more micaceous. When nickel, reduced as above, and consequently containing carburet, was reheated to fusion, a portion of the compound rose to the surface, having the appearance of micaceous iron. It is not acted on by nitric acid, and may therefore be obtained by dissolving out the nickel by that acid.—*Annals of Phil.* ii. p. 149.

5. *On the Alloys of Chromium, Iron, and Steel.*—M. Berthier has lately been engaged on these compounds, and has given much interesting information respecting them, in a paper published in the *Annales de Chimie*, xvii. p. 55. Chromium has so strong an affinity for iron, that the presence of the latter metal very much facilitates the reduction of the former, and the combinations which result are, according to M. Berthier, more analogous to sulphurets and phosphurets than to alloys. The oxide of chrome also has so strong an affinity for the oxide of iron, as frequently to prevent its reduction, an effect that is not observed with any other substance.

Oxide of chromium heated very intensely, in a crucible lined with charcoal, was completely reduced, and gave a button that had suffered hasty fusion, was brittle, hard, grey in some places, grey-black in others, perhaps containing carbon in combination.

Mixtures of oxide of iron and oxide of chromium, in various proportions, were heated in crucibles lined with charcoal, and reduced, giving perfect combinations of the two metals. These alloys are generally hard, brittle, crystalline, of a whiter grey than iron, and very bright, less fusible, much less magnetic, and much less acted on by acids than iron, and these characters are more marked in proportion as more chromium is present. An alloy, resulting from an equal mixture of per-oxides of iron and oxide of chromium, gave a rounded button, full of cavities, lined with prismatic crystals, its fracture crystalline. Its colour whiter than platinum, and hard enough to scratch glass like a diamond. It was easily reducible to powder in a mortar, and its powder was metallic. Strong acids, and even nitro-muriatic acid, scarcely acted on it *.

Chromate of iron, being heated in a crucible lined with charcoal, the iron was only reduced into a minor state of oxidation, and acted on the magnet. Without the presence of the oxide of chrome, the iron would have been reduced.

On heating chromate of iron with an equal quantity of glass, containing 16 per cent. soda, there was reduction of part of the metals, and a loss of 10 per cent. which M. Berthier thinks is iron and chrome volatilized, because a metallic scoria appeared on the surface of the crucible †: and this loss was greater on adding borax, and increased with its quantity.

The best method of obtaining the alloy from chromate of iron, is to fuse it in a crucible lined with charcoal, with .30 of lime and .70 of silica, or with 1. of alkaline glass, or better still with .40 of borax; and to obtain as much chromium as possible, a portion of oxide of iron should be added.

In consequence of the experiments described, p. 319, Vol. ix. of this Journal, M. Berthier was induced to try the effect obtained by adding a portion of this alloy to steel. Two alloys of cast-steel and chromium were made, one with 0.01, the other with 0.015 of chromium. These both forged well, the first better than cast-steel. A knife and a razor were made from them, and both proved very good; their edges were hard and solid, but their most remarkable character was the fine damask they took, when washed over with sulphuric acid. This damask was composed of white silvery veins, and nearly resembled that given by the alloy of steel and silver. The white parts are probably pure chromium, on which acids have no action. There is room to suppose that chromic steel will be found proper for the manufacture of damask blades, which will be solid, hard, and have a fine appearance, and also for many other instruments. It was prepared by fusing together cast-steel and the alloy of chromium and iron.

6. *On the Combination of Oxides with Chlorine, Iodine, and Cyanogen, by* M. Granville.—The compounds formed by the union of oxides with chlorine, &c., have never been examined in a general manner. Messrs. Vauquelin, and Thomson have been engaged with some of them, but still a chasm exists in the chemical knowledge of substances with regard to these compounds. M. Granville has added to the number which was

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* Query, the proportion of these properties due to carbon combination with the iron?

† May not this loss be alkali?

known to exist, and has given the composition of many of them obtained by direct analysis.

The substance known in this country by the name of bleaching-powder, is a subchloride of lime; it consists of

Hydrate of lime,	1 atom	67.914
Chlorine	1 atom	32.086

when put into water, half the lime is liberated, and a solution of neutral chloride of lime is obtained, which consists of

Hydrate of lime,	1 atom	51.416
Chlorine	2 atoms	48.584

This solution is not altered by long boiling, for it still destroys the colour of indigo, and that even after evaporation, provided it has not been perfectly dried. Acids liberate chlorine, even the carbonic acid of the air can effect this decomposition.

The chloride of hydrated baryta is not a sub, but a neutral chloride, being formed of

2 atoms of chlorine	29.28
1 atom of hydrate baryta	70.72

Its properties are the same as those of the neutral chloride of lime.

Hydrate of zinc dissolves readily in chlorine, and the solution, when boiled for a quarter of an hour, strongly discolours indigo; evaporated to dryness, it leaves oxide of zinc, and disengages a little chlorine. On being analyzed, it was found to contain

oxide of zinc,	53.2
chlorine, 1 atom	46.8

The chlorides of magnesia and of the oxide of copper resemble that of the oxide of zinc.

Hydrated per-oxide of iron, dried between paper, when placed in chlorine gas, immediately liquefied, and gave a deep red solution that discoloured indigo. When boiled, the chlorine was disengaged, and oxide of iron deposited.

Oxides of alumium, bismuth, antimony, tin, tellurium, were not dissolved by chlorine; per-oxide of barium was reduced to protoxide, and then a chloride of that oxide formed. The protoxide of lead, nickel, cobalt, and manganese were peroxidized.

Hence chlorine appears capable of combining with most of the metallic oxides, which are not reducible by heat; of those with which it will not combine, some do not act at all on it, except at high temperatures; others peroxidize, and thus lose their tendency to combine with it, just as they lose in part their affinity for acids. Desiccation converts the chlorides of oxides, either into metallic chlorides, or chlorates; all of them, except the sub-chloride of lime, are, in their composition, analogous to the metallic chlorides, into which they may be transformed by losing their oxygen.

When red oxide of mercury is boiled with solution of chlorine, or when treated by a boiling solution of corrosive sublimate, a black crystalline matter is obtained, which is a combination of the red oxide, and the deutochloride of mercury. It is also formed by precipitating a hot solution of corrosive sublimate by potassa, not in excess. When analyzed it gave as its composition,

5 atoms of deutoxide -	79.942
1 atom of deutochloride -	20.058

This compound, M. Granville remarks, tends to strengthen the opinion, that the chlorides dissolve in water, without being converted into muriates; otherwise it must be admitted that decompositions and recompositions of water are effected by very weak causes, such as the affinity of chloride of mercury for oxide of mercury, or the cohesion of the oxido-chloride.

The precipitate obtained by pouring muriate of antimony into water, is an oxido-chloride of antimony. It fuses without decomposition, and is composed of

Protoxide of antimony, 7 atoms	82.01
Chloride of antimony, 1 atom	17.99

The similar preparation of bismuth is also an oxido-chloride; it has the same properties as the compound of antimony, and when analyzed gave, as its composition, 1 atom chloride of bismuth, 4 atoms oxide of bismuth.

Chlorides combine with ammonia as well as with oxides. The composition of the following three are given:—Ammonia chloride of phosphorus,

Chloride of phosphorus, 1 atom	63.502
Ammonia, 7 atoms	36.498

Ammonia chloride of tin,

Chloride of tin, 1 atom	79.06
Ammonia, 4 atoms	20.94

Ammonia chloride of mercury,

Deuto-chloride of mercury, 1 atom	94.09
Ammonia	5.91

Iodine appears to combine with oxides in the way that chlorine does. The compound with lime and strontia appear each to consist of one atom of the oxide, and one atom of iodine. Those formed by oxide of zinc, potassa, *&c.*, appeared to be perfect; but the composition is not given.

In addition to what M. Gay Lussac has said of the combination of cyanogen with oxides, M. Granville adds that the hydrate of strontian instantly absorbed much cyanogen. Hydrate of lime did the same, and became black. Hydrate of magnesia, dried in the air, also absorbed the gas. The cyanides of lime and strontian contain 1 atom of oxygen, and 2 atoms of cyanogen.

A combination of oxide of mercury and cyanide of mercury also exists; it is probably formed of

3 atoms of cyanide of mercury	1.737
2 atoms of peroxide	.992

Ann. de Chim. xvii., p. 37.

7. *On a New Salifiable Base, by* Dr. G. Brugnatelli.—Dr. Brugnatelli, in pursuing his experiments on uric acid, has discovered a new salifiable base. This substance, with his experiments on it, have been described in a memoir read to the Royal Institute at Milan, on January 4, 1821. An abridged account of it is inserted in the *Giornale de Fisica,* t. iii., p. 464.

The new substance is produced by the action of liquid acids on uric acid. Those that have been used are the sulphuric, nitric, muriatic, and acetic; and the uric acid may be either that of calculi or of birds or snakes. It is formed by adding concentrated sulphuric acid, for instance, in small quantities at a time, to uric acid, until a thick paste is formed; it will occasion swelling the liberation of gas, and a particular odour. When these signs have ceased, add water, the mass will become very white; and on standing, will separate into two parts. The solid portion is a neutral combination of the new base with sulphuric acid. The fluid is a portion of this compound dissolved in the excess of acid, and containing impurities. The sulphate is but little soluble in water, but the solution, decomposed by alkaline subcarbonates, yields a white light flocculent substance, which is the base in question. Muriatic acid is, perhaps, better than the sulphuric for the preparation of this substance, inasmuch as the muriate is more soluble. Acetic acid requires boiling to form it, and nitric acid produces it among other products at the time of its violent action.

The flocculent matter collected on a filter, appears like gelatine; in drying it contracts and splits, and when pulverized has the appearance of an earth. It has no taste or smell. It is slightly soluble in water, alcohol, acids, and alkalies. The impure acid solution is eminently distinguished by its property of giving a very fine azure precipitate with triple prussiate of potassa, and which may readily be distinguished, after a few experiments, from that caused by iron. It may, perhaps, be applicable to dyeing or painting. The neutral combination of the substance with acids does not give the blue precipitate, it requires for this purpose excess of acid.

This substance combines with various simple bodies. With iodine it forms a compound at common temperatures, of a dull yellow colour, resolved by heat into its two principles. When fused with sulphur they unite together; its compound with phosphorus is of a fine red colour, and when dissolved in water, occasions the formation of phosphuretted hydrogen, and a phosphate.

This substance has extraordinary powers of resisting heat. It might be taken for an earth, or metallic oxide, in this respect. The following are given as experimental demonstrations of its properties. An acid solution, put on a plate of zinc, gave a yellow spot with metallic splendour. This, well washed, dissolved in an acid, and tested by triple prussiate of potassa, gave a blue-white precipitate; the blue colour being attributed to the new substance. The solution that had acted on the zinc gave no blue colour with the test, but only a white.

A portion of it mixed with lamp-black and oil, and heated violently in a crucible for half an hour, left a reddish crust, the solution of which, in acids, gave an azure precipitate with the triple prussiate.

The azure matter burnt in the fire with facility, and left a residuum of a bright red colour, if the heat had been intense; but if moderate and continued, the residuum is scarcely red, and when placed in water produces flocculi of the substance and bubbles of the gas.

Ammonia dissolves the substance, making it first yellow, then green; when heated moderately, a residuum is obtained of a yellow metallic colour; if more heated it becomes white, and does not seem to differ from the substance first dissolved. The yellow matter dissolved in dilute acid gives a red tint to ferroprussiate of potassa, which exposed to the air becomes green. Other changes take place.

Nitric acid appears to alter the nature of the new substance. When it is added in a concentrated state to the substance or its salts, the prussiate does not then produce a blue precipitate, but a yellow tinge. Sulphuric acid, when assisted by heat offers similar phenomena.

One cannot help suspecting, that the blue precipitates in these experiments are occasioned by iron, and yet it is difficult to conceive how that metal, if present, should escape the observation of Dr. Brugnatelli. Further experiments are required, and of a more decisive nature, to clear up this matter.

8. *Effects of Copper on Vegetation.*—Some time since (says Mr. Phillips) I accidentally spilt some solution and oxide of copper near the root of a young poplar tree, in a short time the tree began to droop; the leaves on the lower branches dying first, and eventually those on the upper ones. On cutting a branch from the tree, I observed that the knife was covered with copper to the whole breadth of the branch, shewing that

the copper had been absorbed, and had undoubtedly proved fatal to the life of the tree.—*Ann. Phil.* ii. p. 77.

9. *On Succinic Acid, by* M. Julien.—The distillation of this acid from amber in the general way produces it in a very small quantity; by coarsely powdering the amber, and then mixing with it previous to distillation $\frac{1}{12}$ part of sulphuric acid, diluted with an equal weight of water; the succinic acid will be produced in about twice the quantity got in the old way. The acid is easily purified by crystallization from sulphurous and sulphuric acids with which it will be found contaminated. I believe the adding of sulphuric acid, in order to increase the production of succinic acid was first noticed in a late German paper; I have forgotten by whom it was there advised, that the diluted acid after being mixed with the amber should be evaporated: this makes the process tedious, and is not necessary; the distillation of the mixture without any previous evaporation will be found more advantageous.—*Annals of Phil.* ii. p. 76.

10. *Woulfe's Apparatus improved.*—The following form of Woulfe's apparatus is due to the Marquess Ridolfi. The bottles have three apertures as usual, and the middle one intended for the purpose of cleaning the bottle, or the introduction of materials, is closed either by a cork or a stopper; tubes descend by the other two into the bottles, as is usual, one a little way in the other nearly to the bottom; these tubes are small, they are fastened into the neck of the bottle and do not rise far above, perhaps three inches, before they terminate: each of these tubes is surrounded on the exterior of the bottle by a considerably larger tube as high as themselves, and fastened by cement on to the top of the necks or tubulars, so as to form a little vessel to receive mercury round the outside of the smaller tube. The connexion is then easily made between one or more of these bottles by glass tubes bent twice at right angles, and of such size as easily to slip in between the two tubes before described: when the lower ends are immersed in the mercury all is tight, and the apparatus may be set to work. This contrivance allows a little motion to the bottles without endangering them; they are instantly connected or unconnected at pleasure, and they act to a certain extent as tubes of safety.

11. *Prize Question.*—The Society of Apothecaries of Paris have offered a prize of 600 francs for—1. The best determination in what manner charcoal acts in discoloration, and what are the changes it undergoes during the action. 2. What is the influence exercised during the operation by any foreign substances which the charcoal may contain. And, 3. To establish whether the texture of animal charcoal is not one of the essential causes

of its more marked action on colouring substances. A prize of 300 francs will also be given for the best vegetable analysis, such analysis to be made on a substance used in medicine, or in the arts. The time is limited to April 1, 1822.

12. *Laws of the Propagation of Heat.*—In consequence of the investigation of M. Bellevue into the nature of meteorolites, M. Emer has been led to examine the manner in which intense heat is suddenly propagated in solid bodies. He is engaged in drawing up a memoir on the movement of heat in solid bodies, in which he considers the points of equal heat, and the points of equal motion of heat in bodies that split and break by the action of fire, and are uniform in all parts. He deduces the law by which the ruptured surfaces of these detonations and separations caused by violent dilatation are formed, and concludes—

1. That it is at protuberances, and particularly at corners and edges that this sort of mutilation should commence.
2. That the fragments should generally affect mamillary forms, or those of pyramids complete or truncated, or prisms.
3. That the bases of the pyramids, and one face of the prisms are the surfaces of the fracture, and are always convex towards their middle, and over the larger part of their surface.
4. That the other faces are often concave, as if for the most part they were the fractured surfaces of the previous fragments.
5. That in general the fracture surface of a principal fragment is its largest face, and that it is only the smaller fragments which are occasioned by the breaking up of these that present anomalies.
6. That each fragment separated is impelled by the result of the forces of dilatation, which result is perpendicular to the surface of the fracture.

M. Emer has made many useful applications of his theory to the arts and sciences.—*Journ. de Phys.* xcii. p. 158.

13. *Phosphorescence of Wounds.*—It is known that light is emitted from organized bodies, when putrefaction takes place under certain circumstances: the same phenomenon sometimes occurs in wounds, and doubtless a greater number of instances would be recorded, were they often dressed in the dark. Baron Percy who, during twenty-five years of war, has had under his care more than a million wounded, has often been deprived of the advantage of light. It was thus that he observed in a young soldier the phosphorescence of a slight wound in the leg, for more than fifteen days. In this case it might perhaps be attributed to the man's having applied compresses dipped in urine to the wound: but sometime afterwards at the siege of Manheim a vivid light, a true ignis fatuus existed, for more than six days,

over the wound of an officer, who had been dressed with compresses wetted with pure water only. Baron Percy has since had frequent opportunities of observing similar facts.

14. *On the Phosphorescence of luminous Insects.*—This subject is treated by M. Macair in a Memoir published in the *Bibl. Univers.* 1821. The insects on which his experiments were principally made, were the *Lampyrus noctiluca and splendidula,* known by the common name of glow-worms. The following are some of the general and striking results.

Solar light appears to have a constant influence on these animals. Some were put into boxes, from which the light was shut out, and when the boxes were opened in the evening, they rarely gave any light; but the same worms, in the same boxes with glass tops, and placed in the sunshine, shone brilliantly in the ensuing evening.

Heat caused these animals to become luminous, and they remained so as long as the heat was continued; it began at 22° R. was brightest at 33° R., the insect then soon died, but the light continued; at 46° it ceased. When the animals were thrown into water of 35° or 40°, they died instantly, but the light continued brilliant: at 10° higher all light was extinguished, and could not be restored. Cold, on the contrary, destroys the luminousness of the insect.

When the luminous part of the animal is cut off, the light diminishes, and in four or five minutes is gone. In a few minutes the rings move and the light re-appears, but more weakly, and then fades away. This continues two or three days, but the light is faint; if the part be warmed, then the light is brilliant, and by renewing it may be restored for two or three days together as often as is desired.

When the abdomen of a worm is opened the luminous matter is found within, formed by a particular organization. It appears as a yellowish white matter on the last three rings semi-transparent, which in the microscope appears organized, and consisting of grains confined in a ramifying fibrile structure, shining brightly in the dark. The interior surface of the ring is very transparent, but not phosphorescent. The substance is translucid, becomes opaque by drying, and then ceases to shine. It is heavier than water. Preserved openly in water it shines with a yellowish-green light for two or three hours, and then ceases shining. Heat and galvanism reproduce the light as long as the substance is not quite opaque. Up to 33° R. the light increases; at 42° it ceases, and the substance is then white and opaque like albumen. In a vacuum it ceases to shine, but reshines with air. It shines more in oxygen than in other gases. When burned it gives ammonical results. Concentrated acids extinguish the light and coagulate the substance. It is not soluble in oils. Ether and alcohol destroy the light, and coagulate the substance. Potassa dissolves the substance. It is not soluble in boiling water, but becomes more consistent in it. From the chemical character M. Macair concludes the substance to be albumen principally, and the cause of the cessation of light to be the coagulation of the albumen and its consequent opacity.

The general conclusions are—1. that a certain degree of heat is necessary to the voluntary light of glow-worms. 2. That a slightly increased heat increases the light, but much more heat destroys it. 3. That all bodies capable of coagulating albumen destroy the phosphorescence of this matter. 4. That the light does not appear except in gases containing oxygen. 5. That the pile excites it, but common electricity does not. 6. That the luminous matter is principally albumen.

15. *Relation of a remarkable electrical Phenomenon.*—The following relation is made by M. Allemand of Fleuvier Neuchatel, to M. Pictet, and is published in the *Bib. Univer.*, June 1821. M. Allemand, on the 3d of May, about ten o'clock in the evening, was caught in a violent storm of wind and rain. The thunder becoming frequent and strong, he thought it proper to close an umbrella he had with him, and hold the upper metallic point in his hand, lest it should attract the lightning. The night, dark of itself, was made more so by the great rain. Suddenly he perceived a light from above, and looking upwards found the edge of his hat luminous. Supposing at the moment the hat was on fire, he, without reflection, passed his hand over the light to extinguish it. It however only shone more strongly, a circumstance which caused some confused ideas on the nature of the light. The hand being filled with water from the hat, on shaking it, M. Allemand saw that the interior of it shone as if it were a polished metal reflecting a strong light.

Being at this time near the farm of Chaux, about ten or twelve minutes' walk from Fleurin, and fifteen or twenty from Motiers, M. Allemand, considered for a moment what he had best do, and concluded on continuing his progress. Having once filled his hand with the electrified water with impunity, he ventured to repeat the experiment, and did it fifteen or twenty times, endeavouring to ascertain whether it had odour, or produced any decrepitation or sound; but nothing of this kind could be perceived, nothing but the bright light which seemed like a brilliant varnish on the hand. The light remained for an instant only. At a few hundred paces farther on, the light on the hat still continuing, M. Allemand was surprised by the appearance of another light less bright than the former, on the smooth surface of the umbrella-handle, at the place where generally a plate of metal is placed for the name, but which plate had been removed from this umbrella. At first the finger was passed over it to extinguish it, but the phenomena were just as before, and both the rubbing and rubbed surface shone brightly. Afraid of the metal about the umbrella, it was thrown down, and M. Allemand went on his way, rubbing his hat on the sleeve of his coat; but in this way only rendering the light brighter. The thunder was more frequent than before, but still at some little distance. The crown of light continued until M. Allemand arrived near Motiers, and he attributed its cessation to the high poplar trees in the neighbourhood of that place.

Stopping at Motiers only a short time, he took a guide with a lantern to find the umbrella. Having done so he sent back the man, and went on himself towards Fleurin.

As the tempest had diminished, he used the umbrella; and as soon as the light of the lantern was sufficiently removed, he again remarked luminous appearances. These occurred at each end of the whalebone ribs, on the metal point which terminates them; the light was not so bright as the electric star, but were brilliant points like a yellow red metal, highly polished, and would, M. A. remarks, have appeared very beautiful if he had been collected enough to admire them.

M. Allemand explains these effects by supposing the atmosphere sursaturated with electricity, and that a portion of it was continually passing to the ground, through his hat, umbrella, and himself.

16. *On Voltaic Electricity*, by M. Pictet.—The following thoughts on Voltaic Electricity are by M. Pictet of Geneva.

In reflecting on this subject, we have been led to a few considerations that tend to explain the well-known fact of the great heating power, and weak chemical action of the voltaic apparatus, with few plates of large surface, compared to the apparatus of many small plates which produces little heat, but has much decomposing power.

Two very different actions may be distinguished in the voltaic apparatus. 1. The chemical action of the fluid which, in attacking the plates, disengages the electro-caloric, and in quantity greater as the action is more rapid. 2. An electro-motive force resulting from the reciprocal action of the two metals of each pair of plates, and by which the electro-caloric is put in movement in a direction according to the position of the plates.

This electro-motive force is probably exerted in a series of actions separated by very short periods of time, or time prescribed, and equal in each apparatus for a time given. The existence of these vibrations is indicated by the characteristic trembling produced by the voltaic action on the nervous and muscular system by the galvanic shock. Let us suppose 100 of these vibrations in a second. A little more or less is of no consequence.

In the voltaic apparatus, with few plates and large surface, an immense number of points in the same pair are acted on at once by the fluid; hence rapid and considerable disengagement of electro-caloric which is immediately urged forward by the electro-motive force, and conveyed by the conductors. These large plates without being numerous, ought to produce a great calorific effect, because each voltaic element or pair, produces in a tempuscule given, a quantity of electro-caloric at least proportionate to its surface, and perhaps even in a higher ratio.

It is not the same with the apparatus of many small plates. The quantity of electro-caloric disengaged in the tempuscule given, being proportionate to the surface of the plates will be inconsiderable, because of the smallness of that surface. It is urged forward by the electro-motive force at the instant of its production, and it traverses the series of elements without ever becoming considerable, because the initial disengagement was small, and cooling would diminish its quantity whilst passing the numerous metallic communications from one couple to another.

On the other hand, this apparatus will be far more effectual in producing chemical decompositions than the other, because the chemical action is probably dependant on the electro-motive action, and it increases with it, and this in a given interval of time; it is much more frequent, and consequently more productive in an apparatus with many plates than in one with few: thus, for instance, suppose 100 of the electro-motive and decomposing vibrations in a second of time, an apparatus with six pair of large plates would only produce 600 of these active vibrations in a second, whilst an apparatus with 500 pair of small plates would produce 500 × 100 = 50,000 similar vibrations in the same time.

It may be also that the extreme abundance, and so to speak, violence of the electro-caloric current in the apparatus with large plates, injures its chemical effect by the mechanical impulsion which the current exerts on its entrance into the fluid to be decomposed; whilst in the many small plated apparatus, the current of electro-caloric being less powerful, and the vibrations more numerous, the polar decomposing effect is more energetic.

These *thoughts*, M. Pictet judiciously adds, are merely given as *conjectures!* they wait the result of experiments, to be either confirmed or rejected.—*Bib. Univer.* xvi. p. 293.

17. *Electro-Magnetic Instruments invented by* M. de la Rive.—Two excellent little instruments have been invented by M. de la Rive, of Geneva, to illustrate the various phenomena of electro-magnetism, with very little trouble, and at a very trifling expense. The first consists of a small voltaic combination attached to a cork; the plate of zinc is nearly half an inch wide, and extends about an inch and a half below the cork, its

upper end passing through the cork to the upper surface; the slip of copper is the same width as the zinc, but passes round the zinc, being opposed to both its surfaces as in Dr. Wollaston's construction, its upper end also appears through the cork. A piece of copper wire, covered with silk thread, is coiled five or six times, and tied together, so as to form a ring about an inch in diameter, and the ends of the wire are connected, one with the zinc, and the other with copper slip above the cork. When this small apparatus is placed in water slightly acidulated by sulphuric acid, the voltaic apparatus is active enough to make the ring highly magnetic; and by presenting a magnet to it in different directions, it may be attracted or repelled, and presents all the phenomena of a mobile conducting wire.

The other apparatus is also a little voltaic combination hung from a cork, but the plates are connected together, not by a ring of wire, but by a helix. The helix is made of similar wire to the ring, it is about one-third of an inch in diameter, and the two ends of the wire are returned through the helix till near the middle, when they are made to pass to the outside between the spirals, then being connected with the upper ends of the plates, the helix lies on the cork, with its two ends equally distant from the centre, the course of the electricity being along the wire, from one end of the helix to the other, and then back to the plates. When placed on acidulated water the helix becomes magnetic, and its extremities become opposite magnetic poles, being attracted and repelled by the poles of a magnet, just as another magnet would be in the same situation.—*Bib. Univer.* xvi. p. 201.

These little instruments took their rise from the floating needle of M. Naef. This was composed of a strip of silver and one of zinc connected together, bent, and floated on cork. These, when placed on acid, were attracted and repelled by the magnetic pole.—*Bib. Univer.* xvi. p. 120.

18. *Contact in Voltaic Electricity.*—In making experiments in voltaic electricity and electro-magnetism, where numerous repetitions of contact between wires are required, it is extremely useful, if these wires are copper, to rub the ends over with a little nitrate of mercury; an amalgam is thus formed on the surface of the copper, which does not oxidate or become dirty as copper itself does, but remains bright, and fit for voltaic contact for a long time. M. F.

19. *Magnetism by Electricity.*—M. Van Beek has remarked, that a very slight degree of common electricity is sufficient to produce magnetism in a needle. A helix was made, and an unmagnetized needle placed in its interior; then one end being held in the hand, the other was brought near the conductor of

an electrical machine, and sparks passed to it. After a number of these had been taken, the needle, on examination, proved to be magnetic.—*Bib. Univer.* xvii. p. 23.

20. *New Electro-Magnetic Apparatus.*—Since the paper in the preceding pages has been printed, I have had an apparatus made by Mr. Newman, of Lisle-street, for the revolutions of the wire round the pole, and a pole round the wire. When Hare's calorimeter was used to connect with it, the wire revolved so rapidly round the pole, that the eye could scarcely follow the motion, and a single galvanic trough, containing ten pair of plates, on Dr. Wollaston's construction, had power enough to move the wire and the pole with considerable rapidity. It consists of a stand, about three inches by six, from one end of which a brass pillar rises about six inches high, and is then continued horizontally by a copper rod over the stand; at the other end of the stand a copper-plate is fixed with a wire for communication, brought out to one side; in the middle is a similar plate and wire; these are both fixed. A small shallow glass cup, supported on a hollow foot of glass, has a plate of metal cemented to the bottom, so as to close the aperture, and form a connexion with the plate on the stand; the hollow foot is a socket, into which a small cylindrical bar magnet can be placed, so that the upper pole shall be a little above the edge of the glass; mercury is then poured in until the glass is nearly full; a rod of metal descends from the horizontal arm perpendicularly over this cup; a little cavity is hollowed at the end and amalgamated, and a piece of stiff copper wire is also amalgamated, and placed in it as described in the paper, except that it is attached by a piece of thread in the manner of a ligament, passing from the end of the wire to the inner surface of the cup; the lower end of the wire is amalgamated, and furnished with a small roller, which dips so as to be under the surface of the mercury in the cup beneath it.

The other plate on the stand has also its cup, which is nearly cylindrical, a metal pin passes through the bottom of it, to connect by contact with the plate below, and to the inner end of the pin a small round bar magnet is attached at one pole by thread, so as to allow the other to be above the surface of the mercury when the cup is filled, and have freedom of motion there; a thick wire passes from the rod above down perpendicularly, so as to dip a little way into the mercury of the cup; it forms the connecting wire, and the pole can move in any direction round it. When the connexions are made with the pillar, and either of the wires from the stand plates, the revolution of the wire, or pole above, takes place; or if the wires be connected with the two coming from the plates, motion takes place in both cups at once, and in accordance with the law stated in the paper. This apparatus may be much reduced in size, and made very much more delicate and sensible.

M. F.

III. NATURAL HISTORY.

§ MEDICINE, &c.

1. *On the Use of Iodine in Medicine, by* Dr. Coindet.—Since the first discovery of the use of this substance, in cases of bronchocele, particular notice has been taken in this Journal of the results of its application, and the conclusions to be drawn from them. See x. 191, and xi. 407. In another memoir, published by Dr. Coindet, and of which the principal points will be condensed below, a new, and in many cases better mode, of administering it is pointed out, and its good effect in schrofulous cases detailed, with the advantages which will probably result from its use in this disease.

It appears that many of the dangerous symptoms caused by iodine, may be attributed, some to its local action upon the mucous membrane of the stomach, which with some persons, cannot bear the remedy uninterruptedly administered, or in increased doses, with impunity; and others to its particular action on the lymphatic system.

The symptoms belonging to these two actions differ essentially. The second action is that which, when properly directed, gives the remedy its usefulness. In order to avoid the first, Dr. Coindet endeavoured to introduce it into the system by other ways than the stomach. An ointment was made, of 1½ ounce of hogs'-lard, and half a drachm of hydriodate of potassa. A piece of the size of a nut was prescribed night and morning, to be rubbed in on the goître, or the enlarged scrophulous glands, or those of the neck. In the first case of its administration in this way, the tumour softened after eight days of friction; in fifteen days the diminution was much greater, and the tumour had divided into many small lobes, distinct one from another; at the end of a month it had entirely disappeared, without any other effect being produced by the iodine.

After that twenty-two persons were treated in the same way. Above half the number were cured in between four and six weeks, and the others to a greater or less degree.

Iodine, thus introduced into the absorbent system, presents the same results as when administered internally. The duration of treatment, the thickening and softening of the skin, the softening of the goître, its diminution and separation into many cists, tumours, or lobes, the cessation of oppressed respiration, and alteration of voice, are exactly the same.

In most cases this mode of treatment sufficed alone to remove the tumour; when it did not, a small quantity administered internally completed the cure. In one case, when the remedy had been applied by the stomach, but only with partial success, external application of it almost entirely removed the goître.

Though none of the dangerous symptoms, sometimes attending the former mode of administration had been observed accompanying the latter treatment, yet Dr. Coindet took all his usual precautions, and when the action on the goître was well established, suspended the application of iodine for eight or ten days; it was then resumed, and thus any evil resulting from a saturation of the system with iodine, or a too rapid action on the lymphatics avoided.

In all these cases the utmost attention was paid to the local treatment of the goître, inasmuch as it contributes powerfully towards the cure. It consists principally in the application of leeches, and sometimes emollient fomentations. The leeches frequently removed pain, and sometimes when iodine alone would not act on the goître, the application of leeches decided the cure. It is not sufficient, therefore, in cases of the goître, scrophula, or other disturbed states of the lymphatic system, to prescribe iodine in a careless or common way, but the other indications must be attended to, to ensure the success to be expected from so powerful a remedy.

From the power of this remedy on the absorbent system, Dr. Coindet was induced to try its effects in cases of scrophula without fever, or indolent swellings of the glands of the neck, and with great success. It was given in solution, in smaller doses than for the goître, and combined with bitters or some aromatic syrup. The course of the cure was the same as for the goître. The tumours became loose, mobile, small, and soft, but the enlargement of the cellular structure feeling like an empty cist, was sometime before it entirely disappeared. In two cases mentioned the cures were decided and rapid.

The application of the iodine externally in these cases was almost as effectual as internally, but the latter mode was generally preferred, because of the tonic effect of the remedy in small doses. Sometimes iodine would have no effect at all, a result which occurs now and then with the goître.

Dr. Coindet, led by the powerful action of this remedy on the absorbent system, then extends his views to some further applications of it. The use of it alternately with mercury, or the iodide of mercury, is proposed in complicated cases of scrophula and syphilis, and in those cases where the syphilitic virus of the parent causes the devolopement of scrophula in the child. From the analogy existing between diseased ovaries, and the affections of the thyroid glands, the probable use in the former cases is pointed out, and as in some goîtres which appeared to be serous cists, a cure was effected, it is also suggested that iodine may be useful in cases of dropsy.

In proposing these various views of the probable use of iodine, Dr. Coindet disclaims any idea of making it an universal medicine. He remarks that all these diseases have something in common, and that the action of the remedy on the absorbent system, is the principle on which he founds his hope of its utility in them.—*Bib. Univer.* xvi. p. 320.

2. *Use of Nitrate of Silver in Medicine.*—The *Giornale di Fisica*, tom. xi., contains at p. 355, a paper by Il C. Sementini, on the use of nitrate of silver in cases of epilepsy. After remarking on the difficulty which occurs in treating such cases, and the good effects that have been observed in using the nitrate of silver, and its superiority in this respect over all other remedies, both as to the effect it produces, and the little inconvenience it causes; the Cavalier states, that to secure the good effects belonging to it, the nitrate of silver should be well triturated with the vegetable extract, in combination with which it is given; that the first doses should be small, and the quantity gradually increased to six or eight grains, or even more, in a day: that the use should not be continued very long together; and that the patient should keep out of the action of light. The latter precaution is necessary, to prevent the discoloration of the skin, which sometimes happens after a long and copious use of this remedy. The precaution, however, only regards avoiding the meridian sun-light.

It frequently happens, in the use of this medicine, that a species of cutaneous eruption, consisting of small pustules, occurs. This may be regarded as a certain proof of the good effects of the medicine.

In the early part of this paper, Il C. Sementini, in endeavouring to remove the impression existing against nitrate of silver, because of its poisonous qualities, remarks, that being mixed with vegetable extract, it is not really the salt, but the oxide, that is given; and, therefore, the observations of M. Orfila, on the nitrate as a poison, have nothing to do with the power of the remedy. At the same time, as an argument for using the nitrate in place of the oxide, it is remarked, that at the moment of decomposition a combination is, probably, effected between the extract and the oxide; and that actually the salt is found most efficacious.

Being assured of the use of nitrate of silver in epileptic affections, and reasoning upon its tonic effect, Il C. Sementini was induced to try its powers as a remedy in cases of paralysis. The first instance quoted is of a gilder, who, probably from the fumes of mercury, had become very paralytic. An eighth of a grain of nitrate of silver was prescribed at first, but the dose was increased every other day; by the time that three grains were taken the good effects were evident, and in twenty days more the man was perfectly restored. In another instance every part of the body and limbs were paralyzed but the head. A small quantity was given at first, but it was increased to eight grains per day, and it effected a cure.

Three other instances are then adduced, in all of which cures were effected: and the Cavalier expresses his hopes that in the hands of other medical men, it will be found as effective and as important as in his own.

3. *Use of Chlorine in Hydrophobia.*—In a book published by Doctor Previtali, of which notice is taken in the *Giornale di Fisica*, tom. iii., p. 357, an extended account is given of the successful administration of chlorine in cases of hydrophobia. Several persons were bitten by a dog, some of these died with all the symptoms of hydrophobia, others were treated with the chlorine, and though the symptoms returned once or twice on the early cessation of the remedy, yet they were vanquished by its continuance, and a perfect cure effected. The medicine consists of four scruples of saturated solution of chlorine, with four ounces of aromatic water and half an ounce of syrup of lemons, to be taken daily. The cases seem perfectly authenticated, for the authorities being earnest to obtain all possible knowledge upon the subject, a correspondence took place in consequence, which is published, and contains the progress of the cures. The symptoms which occurred when the remedy was intermitted; and the immediate effect of the remedy, on administration, were of the most decided nature.

Some account is given in the *Gazetta di Milano* of September 13, 1820, of the application of solution of chlorine as a remedy in cases of spotted fever, by Dr. Sacco, of Milan. Reasoning from its effect on infectious matter, and finding that two, or even three, ounces of the solution might be drank at once without injury, he administered it in cases of spotted fever, and found that in two or three days the effect was to reduce the disease to a simple fever, to shorten the period of its duration, and to lessen the diminution of strength, and other bad consequences, that remain after the fever is removed. And from the constant good effect produced in these, and similar cases, Dr. Sacco states his conviction that it will be of the utmost service in the putrid fever, yellow fever, plague, and all other contagious disorders.

The dose prescribed by Dr. Sacco is one ounce of the solution in three ounces of water, repeated four times a day; a spoonful of honey being taken after each dose. He also directs that the body should be washed three times a day with the same solution.

4. *Medical Prize Question.*—The Academic Society of Nantes

have proposed a prize of 300 francs for the best answer to the queries, "What are the origin, the causes, and the nature, of the yellow fever?" "What are the means to prevent it?"

§ II. Mineralogy, Geology, Meteorology, &c.

1. *New Mineral Substance.*—Mr. J. Deuchar found, a few weeks ago, a new mineral substance imbedded in limestone. It melts at a candle, and burns on a wick, or on paper. In the cold it is insoluble in alcohol, potash, or oil of turpentine, nor is it acted upon in the cold after five days' exposure to sulphuric, muriatic, or nitric acids.—*Annals of Philosophy*, ii. p. 236.

2. *Remains of Art in a Limestone Formation.*—The following geological fact is stated by Professor Silliman, as translated from Count Bournon's *Mineralogy:*

During the years 1786, 7, and 8, they were occupied near Aix, in Provence, in France, in quarrying stone for the rebuilding, upon a vast scale, of the Palace of Justice. The stone was a deep grey limestone, and of that kind which are tender when they come out of the quarry, but harden by exposure to the air. The strata were separated from one another by a bed of sand, mixed with clay, more or less calcareous. The first which were wrought presented no appearances of any foreign bodies, but after the workmen had removed the first ten beds, they were astonished when, taking away the eleventh, to find its inferior surface, at the depth of forty or fifty feet, covered with shells. The stone of this bed having been removed, as they were taking away the sand which separated the eleventh bed from the twelfth, they found stumps of columns and fragments of stones half wrought, and the stone was exactly similar to that of the quarry. They found moreover coins, handles of hammers, and other tools, or fragments of tools, of wood. But that which principally commanded their attention was a board, about an inch thick, and seven or eight feet long; it was broken into many pieces, of which none were missing, and it was possible to join them again one to another, and to restore its original form, which was that of the boards of the same kind used by the masons and quarrymen; it was worn in the same manner, rounded, and waving on the edges.

The stones, which were completely or partly wrought, had not at all changed in their nature, but the fragments of the board and the instruments, and the pieces of instruments of wood, had been changed into agates, which were very fine. and agreeably coloured. Here then (observes Count Bournon) we have the traces of a work executed by the hand of man, placed at the depth of fifty feet, and covered with eleven beds of compact limestone; every thing tended to prove that this work had been executed upon the spot where the traces existed. The presence of man had then preceded the formation of this stone, and that very considerably, since he was already at such a degree of civilization that the arts were known to him, and that he wrought the stone, and formed columns out of it.

3. *On Meteorolites, by* M. Fleuriau de Bellevue.—A paper, by M. Fleuriau de Bellevue, was read to the Academy of Sciences last year, on meteoric stones, and particularly on those which fell near Jonzac, in the department of Charente. This paper is long, and contains much minute investigation of those appearances, which, accompanying these phenomena, afford the only means of ascertaining their real nature. We shall, probably, take an opportunity of abstracting and condensing this paper; in the mean time the following conclusions are presented as those drawn by M. Bellevue.

1. The appearances presented by the crust of meteorolites seem to prove that their surface has been fused whilst rapidly traversing the flame of the meteor, and rapidly solidified into a vitreous state on leaving that flame.

2. They prove that in the first moments the movement of the meteorolites was simple, that is, that they did not turn round on their own axis whilst those two effects took place.

3. That the impulse each meteorolite has received has almost always been perpendicular to its largest face.

4. That the largest face is almost always more or less convex.

5. Our meteorolites (those of Jonzac) offer new proofs of the pre-existence of a solid nucleus to bolides or meteors.

6. This nucleus could not contain the combustible matter which produces the inflammation of the meteor.

7. It cannot have suffered fusion during the appearance of the phenomena.

8. The gaseous matter which surrounds this nucleus is dissipated without producing any solid residuum. No trace of this matter appears ever to exist in the crust of the meteorolites.

9. Meteorolites are fragments of those nuclei which have not been altered in their nature, but simply vitrified at their surfaces.

10. Many of the irregular forms which these fragments present may be referred to determinate geometric forms.

11. These latter forms are the consequence of the rapid action of a violent fire, according to a law of the movement of heat in solid bodies, discovered by M. Emer.—*Journal de Physique*, xcii. p. 159.

4. *Skull found in a Tree.*—A very precise description, ac-

companied by wood-cuts, is given in the *Monthly Magazine* for September, p. 102, of a skull found in a tree. The tree was an ash, and was cut down about two years since in Penley Abbey Farm, near Warwick. It was supposed to be about eighty years old. The skull was closely imbedded in the solid part of the trunk, about nine feet from the ground, and was discovered when the tree was sawn up into rafters. The piece in which the skull lay being cut out a section of it has been made. The wood around the bone was every where perfectly sound, except in one small place, where it is decayed, but this had no communication with the exterior at the time the tree was cut. The grain of the wood was completely deranged, and seemed to embrace the bone round which it had found. The skull is supposed to have belonged to a deer. It is now in the possession of the Rev. Thomas Cottle, of Warwick Borough.

IV. GENERAL LITERATURE.

1. *Ancient MSS.*—M. Maio still continues to be successful in his search after lost works of ancient writers; he has lately found several parts of the books of Polybius, Diodorus, Dion Cassius, some fragments of Aristotle of Ephorus, of Timeus, of Hyperides, of Demetrius, of Phalaris, &c., some parts of the unknown writings of Eunapius, Menander of Byzantium, Prescius, and of Peter the Protector. They were discovered in a MS. containing the harangues of the rhetorician Aristides, from a large collection of ancient writings made by order of Constantius Porphyrogentes, of which only a small part are known to be extant; the writing appears to be of the eleventh century. M. Maio has also found writings of the Greek and Latin fathers prior to St. Jerome, with other valuable works.

2. *Statuary Marble.*—Some remarkably fine statuary and other marble quarries have lately been discovered at Scravazza, in Tuscany, much superior to any thing of the kind at Coanara, which threaten to rival and lower the pride of the latter-mentioned place. His Royal Highness the Grand Duke of Tuscany gives great encouragement and protection both to commerce and the fine arts within his dominions.

3. *Inventor of the Steam Engine.*—A letter was published in the *Gentleman's Magazine* for 1811, which Mr. Tilloch has again brought into notice. The writer of it refers to the Harleian MSS. for proof that the real inventor of the Steam Engine was Samuel Morland, master of the works to Charles II. Morland wrote a book upon the subject, in which he not only shewed the practicability of his plan, but even calculated the power of different

cylinders. The book is extant in the above collection. It was presented to the French king in 1683, and copies were then shewn at St. Germains. The author dates his invention in 1682, seventeen years prior to Savary's patent. The description of the MS., in which Morland explains his invention, will be found in the improved Harleian Catalogue, Vol. iii., No. 5771, and it is also pointed out in the preface to that volume, Sect. 22.

4. *Medico-Chirurgical Society of Edinburgh.*—It is with pleasure that we announce the formation of a Medico-Chirurgical Society in Edinburgh. The Society is formed upon the model of the Medico-Chirurgical Society of London, and has in view precisely similar objects. Most of medical professors in the university, and many of the most respectable practitioners in the city, have co-operated in its formation. Dr. Duncan, senior, has been elected its first president. Its sittings commence in the approaching winter session.

In addition to ordinary and honorary members, provision is made for the admission of corresponding members; and it is hoped that many, in almost every part of the world, and such especially as retain a grateful recollection of the advantages they derived from their alma-mater, will not be backward in supplying interesting communications.

Communications may be transmitted to the president of the Society, or to either of the secretaries, according to the following addresses:

Dr. W. P. ALISON, 44, Heriot-row, Edinburgh.
Dr ROBT. HAMILTON, 3, Northumberland-street, Edinr.

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THE

QUARTERLY JOURNAL,

January, 1822.

Art. I. *Comparative Analysis of Black and Green Tea.*

The following experiments, made in the laboratory of the Royal Institution, were chiefly undertaken with a view of ascertaining whether the different effects usually attributed to black and green tea, are referable to any peculiar principle existing in the one which is not to be found in the other. They will also tend to throw some light upon the relative composition of teas of different prices. To ascertain the nature and extent of adulterations of any kind is not our present object: the various specimens submitted to examination were obtained from most respectable sources, and undoubtedly genuine as imported; what processes the tea may be submitted to in China, or what mixtures and additions it may there receive, are curious and interesting matters of inquiry, and deserve further investigation than they have as yet received.

Experiments with Black Tea.

A

One hundred parts of the finest black tea sold in the shops at 12*s.* per lb., were digested in repeated portions of boiling water until it entirely ceased to act upon the residue; the leaves were then dried, and were found to have lost thirty-five per cent. in weight; they retained their original colour. The infusion was evaporated, and yielded a dark brown transparent extract, very astringent, and of a nauseous bitter flavour.

B

The leaves, exhausted of all their matter soluble in water, were digested in alcohol, (sp. gr. 820°), to which they imparted a deep brown colour, and considerable odour of tea. The alcohol being evaporated, yielded a resinous extract of a more agreeable smell and flavour than that obtained by water. The leaves were now colourless, and without the smallest remaining taste; they were dried and had sustained a further loss of twelve per cent.

One hundred parts, therefore, of the finest black tea contain forty-seven parts of soluble matter, thirty-five of which are taken up by water, and twelve by alcohol.

C

A solution of isinglass was carefully added to the aqueous infusion of one hundred grains of the same black tea, as long as it caused a precipitate, which, being dried at a temperature not exceeding 212°, weighed twenty-eight grains.

D

The above experiments were repeated with a sample of the commonest black tea sold at 6*s*. per lb. The weight of the soluble matter imparted to water was precisely similar, nearly 35 grains from 100: but the leaves, having been exhausted by water, only imparted six grains of soluble matter to alcohol. The flavour of the aqueous extract was nearly the same as that of the former two.

E

A variety of samples of black tea were submitted to distillation with water, but the distilled water had acquired in all cases a very slight vegetable flavour only; it contained no appreciable quantity of vegetable matter, and was not obviously different from tea of different degrees of excellence.

Experiments with Green Tea.

A

One hundred parts of fine green tea digested in repeated portions of water, sustained a loss amounting to forty-one parts; the leaves being separated and dried, still retained a greenish brown colour. The infusion, carefully evaporated, afforded a brown transparent extract, highly astringent and bitter, and having a peculiar flavour unlike that of the original tea.

B

The residuary leaves of the last experiment were transferred to alcohol, with which they formed a green tincture; when the whole of their soluble matter was thus withdrawn, they were dried, and were then of a pale straw colour, brittle, and quite insipid. They had sustained a further loss of ten parts.

The alcoholic solution being evaporated to dryness, yielded a highly fragrant olive-coloured resinous extract, scarcely acted upon by cold water, but perfectly redissoluble in alcohol. Its solution diluted with water became turbid, and deposited a pale olive-green precipitate of a slightly bitter flavour, and smelling very strongly of green tea.

One hundred parts, therefore, of the best green tea contain fifty-one parts of soluble matter, forty-one of which, having the properties of tan and extractive, are imparted to water; and ten subsequently abstracted by alcohol, of a resinous nature.

C

An aqueous infusion of one-hundred grains of the same tea was mixed with solution of isinglass; the precipitate, when rendered as dry as possible by a temperature not exceeding 212°, weighed thirty-one grains.

D

A series of similar experiments were made upon a very inferior sample of green tea, sold at 7*s.* per lb. This only imparted to water thirty-six per cent. of soluble matter; but the leaves, subsequently digested in alcohol, lost eleven grains; so that the entire soluble contents of the good and bad tea, are to each other as 51 to 47; but as far as the mere agency of water is concerned as 41 to 36.

E

Green tea was mixed with water and submitted to slow distillation; the liquid which passed over had acquired a little of the fragrancy of the tea, especially of the finer samples, but not the smallest portion of essential oil, or other vegetable matter could be detected in it*.

The above experiments shew that the quantity of astringent matter precipitable by gelatine is somewhat greater in green than in black tea, though the excess is by no means so great as the comparative flavours of the two would lead one to expect. It also appears that the entire quantity of soluble matter is greater in green than in black tea, and that the proportion of extractive matter not precipitable by gelatine is greatest in the latter.

Sulphuric, muriatic, and acetic acids, but especially the first, occasion precipitates in infusions both of black and green tea, which have the properties of combinations of those acids with tan. Both the infusions also yield, as might be expected, abundant black precipitates with solutions of iron; and when mixed with acetate, or more especially with sub-acetate of lead, a bulky buff-coloured matter is separated, leaving the remaining fluid entirely tasteless and colourless. This precipitate was diffused through water, and decomposed by sulphuretted hydrogen; it afforded a solution of tan and extract, but not any trace of any peculiar principle to which certain medical effects of tea, especially of green tea, could be attributed.

One property of strong infusions of tea, belonging equally to black and green, seemed to announce in them the presence of a

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* "I distilled half-a-pound of the best and most fragrant green tea, with simple water, and drew off an ounce of very odorous and pellucid water, free from oil, and which, on trial, shewed no signs of astringency." LETTSOM's *Natural History of the Tea Tree*, London, 1799, 4to.

Some of Dr. Lettsom's experiments would seem to shew that the noxious effects of tea are referable to the volatile and odorous principle which thus passes off in distillation; and he thinks that those who suffer from them, but yet cannot omit this favourite beverage, might take it with more safety if previously boiled for a few minutes to dissipate the fragrant principle. In all the forms which Du Halde relates for administering tea as a stomachic medicine among the Chinese, it is either ordered to be boiled or otherwise so prepared as to dissipate its fragrancy.

distinct vegetable principle; namely, that they deposit, as they cool, a brown pulverulent precipitate, which passes through ordinary filters, and can only be collected by deposition and decantation; this precipitate is very slightly soluble in cold water of the temperature of from 50° downwards, but it dissolves with the utmost facility in water of 100° and upwards, forming a pale brown transparent liquid, which furnished abundant precipitate in solutions of isinglass, of sulphate of iron, of muriate of tin, and of acetate of lead; whence it may be inferred to consist of tannin, gallic acid, and extractive matter.

When tea leaves have been exhausted by water repeatedly affused, the above experiments shew that alcohol is still capable of extracting a considerable quantity of difficultly soluble matter; this substance, again infused in boiling water dissolves with difficulty, furnishing a liquid which smells and tastes strongly of tea, and which, were it not for the expense of the solvent and the trouble attending its separation, might perhaps be profitably employed.

Though the above experiments shew that tea contains, upon an average, from 30 to 40 per cent. of matter soluble in boiling water, it is not to be supposed that so large a proportion is taken up in the ordinary process of making tea; on the contrary, from tea leaves in the state in which they are usually thrown away, there is still contained from 10 to 14 per cent. of soluble matter, capable of affording a sufficiently pleasant beverage, though it must be granted that the most agreeable portion of the tea, consisting probably of the purer tannin, or astringent matter, and of the whole of the aroma, is taken up by the first affusion of hot water; and that, subsequently, the bitter and less soluble extractive matter are dissolved, furnishing what is usually called *strong tea*, but infinitely less agreeable than the earlier infusion. Hence it is that the real epicure in this article imitates, in some measure, the Chinese process of infusion; and only drinks the first made tea, using a fresh, but small proportion of the leaves for each successive cup.

The following table shews the respective quantities of soluble matter in water and in alcohol, the weight of the precipitate by isinglass, and the proportion of inert woody fibre in green and black tea of various prices; it is given, not as throwing any important light upon the cause of the different qualities of tea, but as containing the results of actual experiments; it may also perhaps save some trouble to future inquirers.

One Hundred parts of Tea.		Soluble in Water.	Soluble in Alcohol.	Precipitate with gelly.	Inert residue.
Green Hyson	14*s*. per lb.	41.	44.	31.	56.
Ditto	12*s*.	34.	43.	29.	57.
Ditto	10*s*.	36.	43.	26.	57.
Ditto	8*s*.	36.	42.	25.	58.
Ditto	7*s*.	31.	41.	24.	59.
Black Souchong	12*s*.	35.	36.	28.	64.
Ditto	10*s*.	34.	37.	28.	63.
Ditto	8*s*.	37.	35.	28.	63.
Ditto	7*s*.	36.	35.	24.	64.
Ditto	6*s*.	35.	31.	23.	65.

ART. II.—*On the Chart of Shetland.* *By* J. M'CULLOCH, M.D., F.R.S.

HAVING been lately engaged in a geological survey of the Shetland islands, I had occasion to lament the deficiency which British Geography labours under in the want of a map of this district; a want which was productive of considerable labour and much uncertainty, and which eventually rendered it impossible to deduce any satisfactory conclusions respecting the general direction of the strata, or the dependency and connexion of those of a similar nature which were separated by wide intervals. Had even the maritime outline been tolerably correct, these difficulties would have been easily overcome, as the interior country is so little distant from the sea in any part, that a map capable of answering the ends in view could have been constructed with very little labour. The sea-chart of Shetland is, on the contrary, not only grossly incorrect in its general geological details, but, with a very few exceptions, utterly unfit for the purposes of navigation. It would be better, indeed, if no such chart existed; as, except in the very few instances which I shall presently point out, a reliance on it is in danger of tempting a vessel to its destruction. It has not therefore even the negative quality of being useless.

The deficiency of the chart is not so generally known as it ought to be. The name of Captain Preston, which is attached to it, is no less likely to mislead those who are unacquainted with its incorrectness, than the apparent decision with which the rocks, soundings, and anchorage, are laid down; and it is too late to ascertain the position of a rock when a vessel is on it, or the badness of an anchorage when she is embayed on a lee shore. In this respect, indeed, there is a striking constrast between the chart of Orkney and that of Shetland; while the excellence of the former may also have the bad effect of tempting those who have navigated by it, to place the same reliance on the latter; unaware that the survey of Orkney was conducted by M'Kenzie with the greatest care and anxiety, and that the chart of Shetland is little better than a map-seller's compilation, supplying the want of documents with conjectures.

To render these deficiencies better known through the medium of this Journal, is an act of justice which public benefit claims; criticism is not always employed in so pure and good a cause, nor can our defects be remedied till they are pointed out; and it is by no means generally known that this part of British hydrography is in so imperfect a state. If it were known, it is certain that the department of our government which takes charge of these matters, would have long ago found a remedy; as is proved by the laudable anxiety it has always shown to improve the art of navigation. The recent establishment of a light-house on Sumburgh-head is, indeed, an earnest of a desire to render the navigation of Shetland more safe than it has yet been; and it is to be hoped that it will, ere long, be followed by a nautical survey of all these islands*.

The present chart is stated to have been drawn up from the observations of Captain Preston, and the latitudes and longitudes of Admiral Lowenorn. A recent addition has been made to it, of a very accurate survey of Brassa Sound by Captain Ramage, who has also published a correct chart of Balta Sound, which, however, is not appended to the general chart. Some alterations have also been made in the original chart, down to 1820, which are said, in the work, to be "considerable improvements." These alterations are, however, trifling, and relate to little else than some of the latitudes, and to a correction of the longitude of Brassa; a new scale having been fitted on the old plates, and a rock added, conjecturally, to the east of Fetlar, as the cause of the loss of the Hound sloop, of which the place is, nevertheless, at this moment unknown.

It is, however, not possible to discover from what authorities the chart has actually been drawn up. It is understood in Shetland that Captain Preston did not survey the eastern coast, having been drowned while engaged on the west side of the island. It is certain, from internal evidence, that he could not have examined the west side throughout, as the coast from Papa Stour toward the north is exceedingly incorrect. His

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* The Commissioners under whom this elegant building has been erected have doubtless proceeded on the best evidence in choosing this place, but the situation is unfortunately too high. In the thick weather so common in Shetland, particularly with easterly winds, this light-house must always be involved in mists and fogs; and, even in the ordinary westerly winds, it arrests the flight of the clouds. It is to be feared that the light will frequently be invisible, and particularly when it is most wanted; and that it will thus become an additional source of danger rather than of security. There is unfortunately no lower point at the southern extremity of these islands where it could have been built, so as to serve the purposes of ships coming from the eastward as well as the westward; but many seamen give a preference to Moussa Island, as the chief want is that of a night-mark for vessels intending to make Lerwick. Such a light-house, together with a small light on the Nab, would render that harbour accessible even in the darkest night.

labours were probably limited to Valley Sound and the Voes as far as Scalloway; although, even these are so incorrectly surveyed, that it is probable the compiler had no access to his documents in a complete state. Neither has the compiler taken advantage of Collins's survey; which, limited as it is, is far more accurate than the present chart is in the same places; as any one may see who will take the trouble of comparing his chart of the two Burras with that which is given, and with the actual form of these islands*.

It is not my intention to go through all the details of this chart; nor would it indeed serve any purpose, unless I could have accompanied it by a large copy, which the dimensions of this Journal do not admit. I shall therefore limit myself to a few observations on some of the most remarkable points, for the purpose of establishing the justice of this general censure; those who may be possessed of a copy, may follow these observations without difficulty. The enumeration of even the few particulars which I have selected for remark, will amply justify this general criticism; and it is hoped that it will have the effect which is intended, of exciting the attention of some individual, or that of the government, to the subject; the object being, not to censure error or neglect, but to instigate to a remedy. Having entered almost every harbour under an excellent pilot, I am the more easily enabled to point out where the defects lie, but shall limit myself to the most prominent†.

One of the most important circumstances in the navigation of all the groups of islands which beset the coast of Shetland, is

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* Although not an advocate for monopoly, the publication of maps, or at least of sea-charts, on the correctness of which so many lives, and so much property depend, ought to be restrained to the hands of government. The temptation arising from a little profit, is too often an inducement to publishers to construct these charts from imperfect and imaginary documents, careless of the consequences which must result from their incorrectness, and, it is to be hoped, not aware of the fatal results which are so often produced by their inaccuracy.

† The accurate and universal knowledge of this pilot, whose name is Peter Anderson, would afford great facility to any one intending to construct a chart of these islands: and it is an opportunity which, once lost,

the nature of the tides: their strength, or velocity, their direction, the times of change, their interference, and the currents which are formed among them. Without an accurate knowledge of this nature, the best geographical survey would be utterly useless. If a vessel is not acquainted with the time of change in all these intricate channels, it will often be found impossible to reach the destined harbour, or to effect the intended passage; as the periods of ebb and flood are so materially influenced by the forms of the land in many places, by the collisions of two floods or ebbs from different quarters, or by the interference, perhaps, of the flood of one channel with the ebb of another. Not unfrequently also, it becomes necessary, in shifting from one harbour to another, so to time matters as to secure a portion both of ebb and flood; since, in consequence of the currents, or depths of water in such harbours, or the existence of bars and shoals, the object may as effectually be defeated by taking too large or too little a portion of the one, as by miscalculating the the time required to effect the passage. With a leading wind, or a favourable breeze, it is true, an accurate attention to the tides is often of little consequence; but cases are constantly occurring, in which, if the passage is not effected by the tide, it will not be effected at all; or the vessel may be caught at sea in a dark night, an event which is always a source of great peril on coasts of this nature, or else be embayed on a lee shore subject to any casual change of adverse wind, which, in these regions, often rise with great violence and incredible rapidity. Very often, security is to be obtained by taking shelter in an eddy, or in the still water which is often found to the lee of the current that sets on some island; but it is unnecessary to detail all the cases, too well known to seamen, and particularly to those acquainted with the Scottish islands, in which an accurate knowledge of the tides is as indispensable as that of the coasts and harbours themselves.

is not easily replaced; as he is the only person in the islands who is acquainted with every rock and harbour in them. I need not point out to surveyors how much time is saved in investigations of this nature by such a coadjutor.

In this important part of every sea-chart, that of Shetland is lamentably deficient; and it affords a strong contrast with that of Orkney, where all these particulars are recorded with the greatest fidelity and accuracy. There is, in fact, in the chart of Shetland, not the slightest indication even of the direction of the flood, except on the west coast, far from land, and for a small space on the eastern; in both of which it is little better than conjectural, and serves no useful purpose. This most essential circumstance is not only entirely omitted in the channels and near the shores, where a knowledge of it is most wanted, but it is not even noticed on the northern extremity of these islands, where the violence of the tide is such as materially to affect the plan of a vessel attempting that passage, and is extremely dangerous to boats. I shall content myself with enumerating a few of the channels where the tides are most troublesome, or where the defect of this part of the chart is most likely to be felt. In all these cases, it must be presupposed that there is not a leading wind, or a sufficient breeze; and when it is stated that the velocity of these tides often reaches to five or six knots an hour, or even much more, it will easily be understood that such a wind may often be wanting, particularly to deep and slow vessels.

Passing over the navigation of Brassa Sound, since a pilot may always be obtained there, I shall first remark, that neither the anchorage of Simbister Bay in Whalsey, nor that of the Out-skerries, can be taken without an accurate knowledge of the time and nature of the tides; owing to the shallowness and intricacy of the channels leading to them. The same remark may be made on the sound of Uyea and that of Balta; particularly if a vessel attempts to take the latter harbour by the northern passage. In shifting from the former harbour to the latter, it is necessary to make use of the latter part of the flood and the first of the ebb; since, without the first of these, it is difficult to beat out of the harbour of Uyea; and, without the last, equally inconvenient to beat into that of Balta. If a vessel, again, is desirous of going from Balta Sound to Cloup Voe, or the western parts of Yell, or from Uyea harbour to the same places, the chart does not indicate that the pasage round the Scaw in the former case, or that of Blumel Sound in the latter, are inexpedient, or even, in certain circumstances, dangerous; and that it is far preferable to take the southern passage through Yell Sound. The strength of the current through Colgrave Sound, though far less than that of the stream which runs with such velocity through Blumel Sound, renders it also necessary to be well acquainted with the times of ebb and flow through that passage; no less in merely sailing through it, than in attempting to make the harbour of Basta Voe. As, in leaving a harbour, every vessel has an opportunity of ascertaining the state of the tide, it is unnecessary to point out the peculiar situations in any of these islands, in which it is necessary to possess this knowledge for that purpose; but I may remark, generally, that with respect to the making a great proportion of them, a vessel may often fail in its object unless that circumstance is previously well known, and thus be compelled to stand out to sea again.

In passing round the Skaw, or Papel-ness in Yell or Fedaland Point in North Maven, it is equally necessary to be accurately acquainted with the times of change; as the strength of the current is considerable in all these situations, and it is requisite to take advantage of the ebb and flood both, for doubling these headlands; but on this subject the chart gives no information. The whole of the flood or ebb is equally required for making the passage from Fedaland to Hillswick, or through Swarbuck's Min; or from Papa Stour to the southern harbours of Æthsting, or the reverse; and, in all these cases, a miscalculation of the time or velocity of the current, if there is a short wind or a head sea, or both, may be productive of the most serious inconveniences. The same remarks might be extended to all the remainder of the western shore; but it is unnecessary to enter into minute details of cases which have occurred in my own experience, and which must occur to all vessels attempting this navigation without a well-informed pilot.

To a stranger, attempting to make any harbour, it is essentially necessary to have some physical marks, or picturesque appearances, if that term may be used, by which the spot can be recognised under different positions, or under that position, at least, in which it is most likely to be seen. The picturesque representations of coasts and headlands in sea charts are not often accurately or characteristically given; and the necessity that every surveyor should have a facility in drawing landscape, is but too obvious in many better charts than that of Shetland.

A great aid to the judgment is afforded, in all these cases, by the mode of expressing the shores on the geographic outline; whether it be low and sandy, or skirted with low rocks, or consisting of cliffs of different degrees of elevation. In the chart of Shetland these circumstances seem often to have been placed at random; while equal value is often given to rocky shores of a few feet in elevation, and to cliffs reaching to many hundreds. To quote examples is but too easy. The west and east sides of Foula are laid down as if they were of the same elevation; whereas, on the former side, the cliffs exceed a thousand feet in height, and the latter is almost uniformly low, and, in some places, indeed, quite level with the sea. The same error is found in many parts of Yell, Unst, and Fetlar; where low shores, and even sandy bays, are laid down as if they consisted of lofty cliffs. Trestra Bay in Fetlar, and Uyea Sound in Unst, are remarkable examples of this error. The island of Balta is another; in which the high cliffs of the eastern shore, and the low and often sandy outline of the western, are represented by the same hieroglyphic. I need not enumerate other striking instances of the same inaccuracy; as, to go over them in detail, would be to give an analysis of the whole lengthened outline of this intricate and indented coast.

To the mere geographer, the most gross inaccuracy of the chart of Shetland, consists, not only in the displacement, but in the absolute omission, of many of the smaller islands; some of which are far from being of trifling dimensions. I shall content myself, as before, in pointing out some of the most remarkable of these; as the want of an accompanying chart on a large scale would render the detail of trifling particulars as unintelligible as it would be tiresome. It will be easily understood, that, independently of the mere geographical defects, these errors are of important consequence in this chart, as far as it is intended for a guide to navigators; not only tending to mislead them respecting their position with regard to any given point of the coast, but further endangering their safety, in thick weather, by the unexpected occurrence of land where they expected to find sea.

An island which lies off Scant Ness is entirely omitted; and this is the more unpardonable, as it is the southernmost point of all Shetland, and therefore sufficiently remarkable. Two small islands to the north of Rovie Head, near Grumister, have also been forgotten. The same occurs at the entrance of Catfirth Voe; where one of the two Glitness Islands is left out of the chart. The How Stack, near them, which is a green island, is also laid down as a naked rock. In Oure Voe there is also an island omitted; and another has been forgotten near Hog Island, not far off from its entrance. At the north end of Whalsey are two islands, only one of which is noticed; but the confusion of small islands on the eastern coast of this spot is utterly unaccountable. There are four islands where only one is marked: three of them called the Holms of Ibister, and another, detached, of which I have lost the name. The Rumble and the Grief Skerry are also utterly misplaced; the former being a mile or more out of its true position, to the southward, and the latter appearing to have been transposed from the north to the south side of East Linga, over a space of more than three miles.

The Out Skerries are represented in nearly as incorrect a manner; the three larger islands being either displaced or omitted, so as to produce the most inextricable confusion. In consequence of this confusion, it would be quite impossible for any vessel to recognise these islands, or to attempt to make the anchorage. The geologist is equally puzzled in attempting to reconcile the physical geography of the stratification which he is examining, with the political geography which the map-maker has thought fit to assign to the remote tenants of this melancholy and stormy spot.

To compensate for the loss of an island in one quarter, the same artist has conferred on Unst one which has no existence;

namely, that of Houna, near Norwick Bay; treating as a rock the real Houna, which lies at the entrance of Balta Sound. It would be equally difficult to account for the assigned place of Hascosea, an island more than two miles in length, lying in a much frequented passage, and forming the very important breakwater to Basta Voe; it is moved more than a mile to the southward, so as no longer to perform that office.

In Yell Sound, the proportions, or relative positions of nearly all the islands are entirely perverted, so as to render it difficult, in navigating them, to know which is meant by any one of those laid down in the chart. This misplacement is most remarkable in the Brother Holm; while one of considerable size to the northward, called Little Holm, is omitted. Similar irregularities occur in the position of the Ramna Stacks, and in that of Greenholm, off Fedaland Point, while one of the former is also omitted. As the passage round this Point of North Maven, which forms the northernmost point of the mainland, lies between Greenholm and the land, this error is the more inconvenient and censurable.

To pass over errors of less consequence in the position of Little Wya and Ossa Skerry, in the dimensions and position of that Linga which lies at the mouth of Olna Firth Voe, and in the omission of a small island at the end of Selie Ness, I may remark, that in Wisdale Voe, an island containing not less than a square mile, is converted into a rock. The position assigned to the rocks and islands which crowd the bay, intercepted between Skelda Ness and Burra, are also incorrect; but the inconvenience that might arise from this is, in a great measure, obviated by the channels which lead into Scalloway having been laid down. It is for this part of the chart that the compiler appears to have been indebted to Captain Preston's survey; and it is certainly the least exceptionable part of the work; though I must in justice remark, that an appended chart of Valley Sound and Grueting Voe, on a larger scale, is also very correctly laid down.

Where such errors exist in the positions of islands of such dimensions, and where so many are omitted, it cannot be expected that there is more accuracy in the places of rocks, whether visible or sunk, in which so many parts of the coasts of Shetland abound. It is not too much to say, that, with very trifling exceptions, the whole of them are incorrect; either by reason of omissions, misplacement, or characters wrongly expressed. It will suffice here to mention a very few of the most remarkable cases, as some of them will again come under notice in pointing out the errors in laying down the harbours. How important it is to be accurate in this part of every nautical chart, it is quite unnecessary to remark. If there is one circumstance more than another which is a source of perpetual anxiety and distrust to a vessel attempting to make a harbour, or navigate a channel, it is a doubt respecting the place, existence, and character of rocks; and, more particularly, of those which are not always visible. It does not, indeed, fall to the lot of many mariners to experience the anxiety that is felt by him whose fate it is to navigate coasts of this nature; but those whose business it is to be often engaged among islands and channels like these, know well the hourly risk to which they are subject, where the space of one day alone exposes them to greater hazard than could be crowded into a whole life spent in navigating the ocean. It is not too much to say, that the circumnavigation of the Shetland Islands is attended with more hazard than that of the globe. A correct chart would reduce that hazard to little or nothing; yet that is not only wanting, but the seaman is exposed to the additional risk which must ever arise from the necessity of placing confidence in one which is grossly erroneous.

A number of sunk rocks are marked in this chart as lying near to the Noull of Eswick; whereas there is deep water here close in shore, with a safe passage for ships of any draught between the How Stack and the land.

Respecting the very intricate and dangerous navigation between Whalsey and the main land, or that on the east side of this island, it is only necessary to say, that not a single rock is rightly indicated; the whole being such a scene of confusion in the chart, that it is vain to attempt to specify the errors in words. That which was remarked respecting the islands at the Out Skerries, may also be observed of the rocks, which are equally incorrect in their positions.

In entering Oure Voe, it is not indicated that there is a rocky shoal between Ballasetter Holm and the southern shore, respecting which I had the disagreeable experience of having struck on it in nine feet water.

A rock is laid down off Fetlar, on which the Hound sloop, as formerly remarked, was said to have been lost in 1800. Now the true place of this rock is not ascertained, important as it is; nor did our pilot believe that the place indicated was at all near to that in which that vessel was wrecked; certainly, at least, none of the fishermen, who are perpetually on this coast, were acquainted with the spot. Hence no vessel can approach Fetlar from the east with any confidence, and those steering north or south must either keep a very wide offing, or sail close in shore.

There is an utter confusion respecting the rocks which lie to the southward of Yell, of which the knowledge is exceedingly important, as they lie in the way of vessels intending to pass from the north through Yell Sound. As to the Ramna Stacks, which were already noticed in mentioning the misplacement of islands, the omissions are of less consequence, as the principal are sufficiently conspicuous to form marks for themselves.

There is a passage between Papa Stour and the main land, which would often be very convenient, as enabling ships to save a tide in going for St. Magnus's Bay; but it is so ill laid down that no vessel can venture to take it. Not to prolong a part of the subject, however, on which it would be useless to insist further, I shall lastly remark, that there are great doubts in the minds of the pilots and fishermen, respecting the true place of the Have de Grind rocks, which lie to the eastward of Foula. Such a want of confidence, indeed, was felt by our pilot, experienced as he was, that he declined taking charge of the vessel, and those who have been in similar situations may judge of the uneasiness experienced in standing off and on, in a heavy sea, during a whole night on this coast, with a view of making Foula in the morning.

The profusion of excellent harbours in Shetland is such, that every seaman who has experienced the want of similar refuge in the channels of England, is inclined to express a childish regret at the waste of a commodity, as he is inclined to view it, which, if properly distributed, might almost supply the whole of Europe with anchorages. Even in this profusion, however, is the bounty of nature shewn, as, without the refuge which they afford at almost every point, it would be impossible, at least in the short days of winter, to navigate these coasts at all. It is not possible for those who have not experienced this kind of navigation, to conceive the anxiety which the coming on of darkness or thick weather produces in such intricate channels, amid rocks and on lee-shores, and among currents and tides, which prevent the pilot from forming any estimate of the vessel's place. In such cases even all the harbours of Shetland are not too many; and yet of these there is a large proportion in which the compiler of the chart has placed no anchor, while in others he has marked stocked anchorages, where no vessel would even venture to stop a tide, unless in fine and summer weather. I shall enumerate these errors and omissions in somewhat greater detail, as it is a species of information which can more advantageously be communicated by mere words, than that which was attempted in most of the preceding remarks.

The anchorage at West Voe, near Sumburgh Head, may often be very convenient with an east wind, or with a wind from the west, when Quendal Bay would be too open. The ground is clean and good, and there is no difficulty in beating out, unless the wind were to shift to the southward. It is far more spacious than is represented in the chart, owing to the western promontory having been laid down of more than double its actual breadth, and from the omission of the island off Scant Ness.

The harbours immediately north of Sumburgh Head, namely, East Voe and the Pool, are both exposed and shoal, nor are they safe, even for the smallest class of fishing vessels, although in the chart a stocked anchor is laid down in each. Levenwick, Sandiwick, and Æth's Voe, are almost equally bad; but any incorrectness in these is of less moment, as no vessel would incline to stop in them when equally able to reach Brassa Sound. On the subject of that sound, the minute survey of Captain Ramage, lately appended to the general chart, leaves nothing to be desired. There is nothing to object to the several anchorages laid down in Catfirth, Wadbester, Laxfirth, and Dale's Voes, nor to those in Oune Voe, since an anchor may be let go in almost any part of these inlets.

But in Whalsey the chart of the harbour is so incorrect, that the real anchorage could not be discovered by it without the aid of a pilot; so that in this respect the chart is, to say the least of it, useless. If the anchorage at the Out Skerries, which may often be very convenient, had been properly laid down, there would have been no difficulty whatever in taking it, by attending to the tides. For want of any direction respecting these, from the absence of marked soundings, and from the extremely incorrect position given to these islands and the intermediate channels, no vessel would now dare to enter them; although, so far from the harbour being fit for small vessels only, as the chart says, ships drawing twelve feet water and upwards may lie in it with the greatest safety, and may quit it with any wind, as there are two entrances.

The entrance to Vidlon Voe is perfectly simple; and here the chart has very properly laid down anchors, as it has, with much less propriety, in Burra Voe, at the southern point of Yell, since that harbour is superseded by the much better and neighbouring one of Hamna Voe, although in neither have any soundings been laid down. From this part of Yell to Refirth Voe there is no anchorage, and, although there is fortunately no difficulty in entering this harbour, excepting that arising from its narrowness, it is extremely ill delineated in the chart, nor are any soundings placed in it, so that it cannot be entered without the lead. Basta Voe forms one of the finest harbours in Shetland, or perhaps in the world; but no indication of its nature is given in the chart, and, as usual, it is deficient in the essential circumstance of soundings; so that in thick weather, in which it was my fate to enter it, it is necessary to keep the lead always going. In speaking formerly of the incorrectness of the islands, I remarked that this harbour was principally formed by the island of Hascosea, which covers it, while, according to the chart, no vessel would venture to run for it in a south-east wind, to which, according to the draught, it appears to be open.

Fetlar contains no harbour, yet, for want of soundings in the chart, vessels might be inclined to take Trestra Bay, where the shoal water of this very bad place ought to have been noticed. Uyea Sound is properly marked as an anchorage, but the anchor is laid down too near to the shore of Unst.

Captain Ramage's chart of the important harbour of Balta Sound has supplied the deficiency of the general chart in this place, but a reduced copy ought to have been added to it, as it is impossible otherwise to venture on this harbour, particularly by the northern passage, where the relative position of Balta and Unst is extremely erroneous. It has been long enough before the public to leave the proprietors of the Shetland chart no excuse for not having appended it to the editions sold in 1820.

There is no other harbour in Unst, and although an anchor is laid down in Cloup Voe, at the north end of Yell, it is not frequented. The difficulty of beating out of Whalfirth Voe, against the western swell, also renders that an inconvenient harbour, a circumstance of which notice ought to have been taken in the chart. To the southward of this, near Sandiwick, are two harbours where vessels may lie securely, but in neither of these is an anchor marked.

It is scarcely necessary to notice the omission of anchorage marks in the voes to the south of Waterholm, as these harbours are not wanted in a channel where so many others are at hand; but it ought to have been remarked that there is a very good and a very convenient anchorage in Urha Voe, for vessels which are either employed in Yell Sound, or have not the good fortune of wind or tide to effect their passage through it. This omission is particularly censurable, as the depth of Hagraster Voe makes it inconvenient, and as there is a scarcity of good harbours, compared with the necessity for them, which may often be felt in this channel, on the east side of North Maven.

In Hagraster Voe no anchor at all is laid down, although, for a space of near eight miles, vessels may anchor almost any where in this very secure and quiet place, in clean and good ground.

Colifirth Voe also forms a very convenient harbour for vessels losing the ebb tide in the channel, yet no anchor is laid in it. The draught of this harbour is indeed so incorrect, that no vessel would be aware of its containing a smaller bay within, where there is always smooth water, and excellent soft holding ground.

No anchorage is marked in Burra Voe, to the northward of this, or, as it is sometimes called in the country, North Ru. Yet it is peculiarly convenient for vessels intending to make the passage round North Maven, as, by taking the end of the ebb from it, they may ensure their passage round Fedaland Point, with a whole flood, to reach any of the harbours in St. Magnus's Bay. Independently of this, it is laid down in such a manner that no vessel would expect to find a harbour in it, while a sunk rock in the entrance, which is not easily seen in the smooth and dark water of a bay, receiving so much fresh water from the hills, is entirely omitted. I may as well add here, that which more properly belongs to the examination of the coast outline, that there is here laid down in the chart a bay called Husater Voe, which has no existence, the whole coast from Colifirth Voe to Bura Voe being nearly straight, instead of being deeply indented.

Although an anchorage is laid down in Sand Voe, on the west side of North Maven, no vessel can possibly take this harbour, unless under extreme distress. From its narrow entrance it is utterly impossible to beat out of it, and, from the prevalence of westerly winds, and the almost unceasing heavy swell from that quarter, a vessel once at anchor here might be detained for months. The same reasoning applies, and almost in the same degree, to Ronas Voe, although the entrance is much wider. The difficulty of getting out of these harbours is much increased, and indeed often rendered extremely formidable, by the squalls which blow from the surrounding high land, and by which a vessel is so often baffled in her attempts to stay, while the want of room to wear renders the missing of stays a very dangerous accident, as I have more than once experienced. To avoid future repetition, I may here remark, that Ronas Voe has a deep inlet nearly at right angles to the entrance, which, in the chart, is entirely omitted. Of Hamna Voe, on this shore, where an anchor is also laid down, it is only necessary to observe that, owing to its breadth, it might be safer for the purpose of stopping a tide in, with a wind from the eastward, but that in a westerly wind it affords no shelter whatever.

Although an anchor is laid down at Hillswick, no vessel, looking at the draught on the chart, would attempt to take it, as the line of the coast is made nearly straight where there is a considerable bay. Thus, as happened to myself on getting into this bay, a seaman is tempted to run for Hamer's Voe; a most secure harbour it is true, but out of which it is scarcely possible to beat against a strong west wind, from the narrowness of the entrance, while that attempt is also attended with a hazard which nearly cost the loss of the vessel in which I sailed. I ought here also to remark, that there is an inlet from Hillswick northwards of two miles in depth, which is forgotten in the chart. This, however, is a mere question of terrestrial geography, as the water is too shoal for vessels. As ships may anchor by the lead almost any where to the west of Muckle Roo, the chart is here sufficiently correct, as are the directions to enter by Swarbucks Min. But there are two serious deficiencies in the sailing directions for St. Magnus's Bay, as well as in the chart of that place, which require notice, more particularly as, for want of proper information, a Russian vessel was lost here not long ago, when she might probably have escaped without much difficulty. The tallow, which formed part of the cargo, is still picked up on the shores, being mistaken by the natives for spermaceti, and having undergone some changes from the action of the sea water, which are not uninteresting to chemists. Vessels caught in St. Magnus's Bay with a westerly wind, are directed to run for Swarbucks Min. Now, if the wind is to the southward of west, a vessel, in attempting to weather Muckle Roo, may fail in this object, and become so deeply embayed as inevitably to go on shore on Eglisha, the Longhead, or Isle-burgh-ness, as that above-mentioned did. The direction in this case should have been, to make for Hillswick or Hamer's Voe, as more to leeward, and where the entrance is attended with no difficulty. If once to leeward of the Longhead, it would be too late, with such a sea as the westerly swell sets in here in a gale of wind, to attempt Hillswick, and equally impossible to weather Muckle Roo. It ought also to have been noticed in the chart, that there is no water, except for boats, in Rose Sound, which is, on the contrary, laid down as a wide channel, and without soundings, since a vessel, despairing of weathering the land, might make for this opening, where she would infallibly be lost.

In Papa Stour no anchor is laid down in House Voe, which is not only a good anchorage for ships of moderate burden, but is absolutely necessary for the purpose of waiting the tide to the southward, supposing a vessel to have left the harbours within Swarbucks Min with the ebb. Two or three anchors, on the contrary, are placed in harbours at the north of Walls, where no vessel enters.

The separate survey of Valley Sound and Grueting Voe are correct, although, in a geographical view, the latter inlet is very improperly contracted in its dimensions. It ought also to have been noticed, that no vessel can beat out of the east sound with a swell from the south-west, and that the attempt is attended with the greatest hazard, from the narrowness of the channel, and from the height of the land, which produces baffling squalls, and from there being no room to wear in the passage, nor ground to let go an anchor if a vessel should miss stays. Of the perilous nature of this attempt I can also speak from experience. There is a stocked anchor laid down in Selie Voe which ought not to be there, as it is almost an open harbour. In Frixeter

Voe one anchor is placed; the whole is, however, one immense harbour, capable of containing all the fleets of Europe, but the chart has neglected to notice the shoal water at the entrance, which renders it nearly as impracticable as it is useless on this coast. In Wisdale Voe no anchor is marked, although there is an excellent anchorage near Sand, with a clean channel. Of both these latter bays I may further remark, that the outline is very incorrect; in Wisdale Voe, in particular, a deep bay being gratuitously placed where the shore forms a straight line. It is from errors of this nature that the opinion was formerly given, that Captain Preston's documents could not have been applied to the construction of even this part of the Shetland chart, as such mistakes could not possibly have existed in any real survey, had it even been executed by the most ordinary fisherman.

No anchorage being marked in Stromness Voe, it is almost superfluous to say that the entrance is too narrow, as well as too shoal, for any vessel; but a singular omission of a geographical nature is here deserving of notice. This is the total omission of the prolongation of this voe, which reaches for nearly three miles into the interior country, being connected with the more open bay by a narrow channel, over which there is a bridge. As a part of the sea outline, this should have been inserted, although other inland lakes and objects were neglected, as appertaining rather to the terrestrial geography. The harbour of Scalloway is sufficiently well laid down, but there is a very singular mistake in representing the two lakes of Tingwall as one, and in connecting them with the sea, as if they were salt lakes, whereas the southernmost is separated from it for more than a mile by a tract of meadow-land, which certainly has not been formed since the chart was constructed. Anchors are laid down in several parts of Cliff Sound, where no seaman would think of anchoring, owing to the depth of water, and the squalls from the high land, and where, in fact, there is no occasion for any vessel to stay. A similar remark may be made on an anchorage marked between the two Burras, which can only be required by the smallest class of fishing vessels, and is safe for no other, while the extremely incorrect way in which these two islands are drawn, renders it nearly impossible for any stranger to conjecture where the channels are, or where his position is.

Near St. Ninian's Isle are laid down three anchors, and it would be difficult to say which of these places is the worst harbour, if indeed they at all deserve the name of harbours. In an east-wind, a vessel might stop a tide under St. Ninian's ; but, with a wind from the west, no seaman in his senses would make such an attempt. I may also here observe, that the bar which connects St. Ninian's Island with the main land, is laid down in a wrong place ; being nearer to the middle of the island.

The last harbour in the circuit is Quendal Bay. Here Collins's chart is more correct than that which is here reviewed ; although deficient with respect to the point that covers the proper anchorage under Quendal house. This is a wide and excellent bay, even in winds from the west ; nor would there be any difficulty in getting to sea, should the wind shift to the southward, unless there was a very heavy swell ; as it is sufficiently wide to enable a vessel to beat out without difficulty.

It would be an endless task to go over the subject of soundings, and I shall therefore be content with remarking, in a general way, their incorrectness and deficiency. Where these were of most importance, they have already been noticed as far as is necessary for the purposes of this criticism, in the preceding remarks on the harbours.

Neither is it necessary to make any detailed remarks on the very few sailing directions which are appended to the chart ; that which relates to St. Magnus's Bay having already been examined. I shall only observe in addition, that the passage through Cliff Sound, either into, or out of, Scalloway, is always inexpedient, owing to the squally nature of this bay and narrow channel, and the nicety required in passing the bar of Trondray, on which there are only twelve feet at high water.

With respect to the general outline of Shetland as given in the chart, I must limit myself to a few cursory remarks on some of the most prominent errors which are to be seen by the eye alone ; as I had no opportunity of making any accurate observations by the aid of instruments. Nor indeed can any correct notion of the heap of errors which it displays, be given in words. In a nautical view, this species of error is certainly of far less importance than those already pointed out; yet it is not the less discreditable, that so large a portion of the British dominions, remote as it may be, should have remained so long neglected as it is, but as it cannot now continue much longer. The interior geography is, in fact, not merely little better, but somewhat worse than a blank; as that which is given is incorrect, and as the record of any one object only tends to mislead, by causing the traveller to suppose, that where nothing is laid down, nothing is therefore present. Near Sumburgh Head, it is not indicated that the connexion between this promontory and the main land is so slight that it is almost insulated by a tract of loose sand scarcely higher than the level of the sea; nor would the near approach of the voes on the east side to Quendal Bay, be supposed, from the outline in the chart. There is no incorrectness of much moment, as far as the objects of the chart are concerned, from this point to the Naull of Eswick, where a small but deep inlet is entirely omitted. I may remark indeed, once for all, to avoid tedious repetitions, that throughout the chart in general, the voes are rarely carried to a sufficient depth within the land, and are often equally contracted in their lateral dimensions.

The incorrectness of the outlines of Whalsey Island, and of the Out Skerries, were already mentioned in speaking of the anchorage in these places; but it is fair to remark, in extenuation of these and other errors, that Oure Voe and Vidlon Voe are, on the whole, very well delineated. In Yell there is a deep bay at Quyon which is very slightly marked in the chart; but as it is not used as an anchorage, on account of some rocks at the entrance, the error is of the less moment. The extraordinary incorrectness of the coast and the harbours immediately to the northward of this, was already mentioned in speaking of these harbours, and of the misplacement of Hascosea Island.

The outline of Balta is extremely faulty, but there is a much less pardonable error here in representing the northern passage into the sound as a wide and clear opening, whereas it is very much narrowed both by the position of Balta, and by a boundary of low rocks.

Of Yell, it is sufficient to say that the general outline is no less incorrect throughout, than are the draughts of the harbours; and, on the whole of Yell Sound, the incorrectness of which was already noticed in speaking of the smaller islands, it is proper to remark, that the want of an indicated tract renders the chart in this place nearly useless.

Having already observed upon the incorrect outline of the east side of North Maven, I need only here add that a singular omission will be found near Fedaland Point, which is indeed quite unaccountable, if ever any survey of this shore was made. The extremity of North Maven here forms a peninsula of considerable dimensions, separated from the main land by a beach of shingle and rocks; the indentation on the east side forming a cove for boats, and containing a very important fishing station. The omission of two fresh-water lakes in this neighbourhood, will perhaps not be considered of any moment; but, where some of these are laid down, it is a natural expectation that the whole should equally be found.

On the west-side of North Maven, the land under Ronas Hill is carried too far to the westward; and the same sort of incorrectness prevades the whole of this shore. Independently of the omissions of the two bays formerly noticed at Hillswick, there is a third sandy inlet on the west side of that peninsula, where a boundary of rocky cliffs is indicated in the chart. In the same manner, there is not due value given, either to the dimensions or form of Hamers Voe, which, as I formerly observed, is a very important object to vessels embayed in St. Magnus's Bay. Respecting the outlines from this part as far as Scalloway, the chief remarks, as far as the use of the chart is concerned, have already been anticipated in speaking of the various anchorages; and it would be superfluous to enter into more details respecting so erroneous a specimen of geography as it exhibits. Yet it may be remarked, that the very incorrect outline of the two Burras is particularly unpardonable; as the survey of Collins, to which the compiler might have had access, gives a far more correct view of them. It is indeed difficult to conceive how any one drawing them at all, could have contrived to do it so incorrectly; as the most superficial examination by the eye, is sufficient to correct the glaring errors which occur in the chart. The bridge which connects the two is not only omitted, but its place is indicated much too far to the southward. A deep bay on the east side of East Burra, which is reduced to almost nothing in the chart, is so situated and of such dimensions in nature, as to tempt a boat into it from Cliff Sound, with the expectation of finding the passage between the two islands. At the southern extremity of each of the islands, there is also a peninsula separated by a low rock, in one case indeed nearly insulated; but these are totally omitted in the chart.

But it is unnecessary to proceed further, and I shall therefore conclude these remarks on the general outlines of the Shetland Chart, by noticing the incorrectness of the draught of Foula, which the most superficial view might have given in a better form, and which it would require no great expense of time to survey with sufficient accuracy for all general purposes.

In terminating this communication, I shall only add, that a set of the most common astronomical observations on the latitudes and longitudes of a few leading points, would materially improve this chart; and that, with the assistance of the pilot before-mentioned, the industry of an active person would, in one or two summers, supply most of the chief desiderata in the present imperfect documents, until a real survey of all the islands can be effected. It is in vain to attempt to construct such a chart by the ordinary operation of boat surveying; as the rapidity and uncertainty of the currents, render it absolutely impossible to determine points in this manner with any tolerable accuracy. I shall therefore trespass no longer on the patience of the readers of this Journal, to most of whom the navigation of Shetland is, perhaps, as little interesting as that of the Celebes or Loo Choo. It is sufficient to have justified, by the details already given, the general censure with which I commenced this communication; and its object will be fully attained if it shall either induce those who may with authority undertake it, to commence this necessary work; or point out to unemployed officers in these times of peace, a subject, from which, if they do not reap profit or fame, they will assuredly acquire the thanks of many who, like myself, have hourly hazarded their lives during a stormy summer in a dangerous and anxious navigation.

Shetland, August, 1821.

Art. III. *Account of the Method of illuminating the Clock Dial on the Steeple of the Tron Church in Glasgow.*

[We are surprised that no attempt has yet been made to illuminate the dials of the London steeples, more especially as gas has been conducted into several of our churches, and is almost always abundantly laid on in the vicinity, for the purpose of lighting up church-yards, and thus preventing the unhallowed visitations of a class of people, who, under powerful patronage disgrace this metropolis and its suburbs, commonly called *resurrection men.* Several of our city clocks are, as it were, constructed for the purpose; such as those of St. Dunstan's in Fleet-street, of Bow church in Cheapside, and others which project after the like fashion. The Parish Church of St. James is also very advantageously situated for nocturnal illumination, one of its four sides being seen from a number of streets at the west end of the town.

The following is a description of an ingenious plan successfully adopted at Glasgow, under the superintendence of Messrs. John and Robert Hart. We understand that the hours on the clock-dial, which fronts a long line of streets, are legible at night to as great a distance as when the sun is directly shining upon the tower.]

The Tron steeple of Glasgow is of the Gothic order, consisting of a square tower surmounted with a pyramidal top; the square tower terminates with a rail, or balcony, beneath which the clock-dial is placed.

The gas is introduced into the bottom of the steeple and conveyed up the wall by an iron pipe of one inch bore: this pipe passes within eighteen inches of the wheel-work of one of the dials, and a wheel, of double the diameter of the hour-wheel, is so placed as to be driven by it; this, of course makes one revolution in twenty-four hours. The hours are engraved on the rim of this wheel, and a moveable arm attached to it, which can be fixed at any hour or half-hour by means of a clamp screw upon the centre, and a steady pin and holes in the rim; this arm serves to disengage a catch that holds up the hand of the main stopcock, so that by this means the clock shuts off the gas in the morning. A painted board with the respective months and hours serves to direct the person who has the charge of it, to shift the arm as the season alters. The pipe afterwards ascends and branches off to the respective dials.

The lamp is formed of a copper bowl, eighteen inches diameter, and of a parabolic figure, of three inches and a half focus: it is lined in the inside with small pieces of looking-glass, imbedded in white paint and putty, similar to some of the reflectors used for light-houses; the front is enclosed with glass. The burner used is a No. 3, Argand, with each alternate hole shut up, and equal to a No. 1, *Glasgow* Argand.

The lamp, properly inclined so as to illuminate the dial, is suspended about seven feet from the building, above the centre of the dial, the bottom of the lamp is on a line with the top of the dial plate. From the situation of the dial it was necessary to fix the lamp from the balcony; and that it might be easily got at, the branch is jointed so that it can be drawn up to the person within the rail, to be cleaned when necessary. It is lighted from the inside of the steeple by a *flash pipe*, or separate tube, pierced along one of its sides with holes, and partially covered to protect it from the weather; so that when the gas is admitted and a light applied at one end, each hole lights the next till the flame reaches the farthest extremity, and thus kindles the gas issuing from the burner; the *flash pipe* is then shut. The exterior of the lamp represents the Eagle of Jupiter armed with lightning, and the whole is surmounted with the city arms. From the cheerful appearance of this dial, it is likely that other clocks may likewise be lighted, where the building will admit: a simpler and neater method than that which the peculiarities of the Tron church steeple rendered necessary, might be adopted; namely, to cut a small circular window about two feet in diameter above the dial, from which the lamp might be put up and cleaned when necessary, and when lighted pushed forward: here a straight rod only would be required to suspend it, which might be run out over a wheel having a balance weight in the inside to return it again to the window in the morning. The back of the lamp might be made to represent the city arms, or any other appropriate device. When the clock disengaged the gas stopcock, it might also liberate the catch that retains the lamp, and allow it to run in, so that the person who lighted it, would only have to hook up the stopcock, light the lamp, and push it forward into its proper situation for illuminating the dial: a simple folding-piece of three joints, like a foot-rule, would connect the lamp or lamps to the centre or main gas pipe.

Description of the Plate.

Fig. 1. is a section of part of the steeple—A, the dial and hands—B, the gas pipe, with C, the flash pipe branching off—DD, Joint on which the branch turns when drawn up—E, stays for its support.

Fig. 2. shews the means of shutting off the gas—A, the ascending pipe and main stopcock open, with its hand upon the detent—B, the discharging wheel; 1, the moveable arm, 2, the detent—C, the hour-hand work of the dial.

Fig. 3. Section of the reflector and burner—A, glazed cover.

ART. IV. *On the Difference of the Functions in certain Nerves of the Face, illustrated by their Anatomy in the Inferior Animals, and by a comparison of their Uses in Man and Brutes. By* JOHN SHAW, Esq.

IN a paper published in the last Volume of the *Transactions of the Royal Society*, Mr. Charles Bell has shewn that two sets of nerves, differing in structure and function, go to the face in man. One of these has the property of controlling the actions of the muscles of the mouth and nostrils in breathing, and of giving expression to the features in emotion; while the other appears to be for the purpose of regulating the muscles in mastication, and those actions which are independent of respiration and expression, and for bestowing sensibility on the skin.

Having been engaged with Mr. Bell in making the experiments by which these facts were proved, my attention was naturally called to a further observation of the condition of the muscles of the face in the different kinds of paralysis. The cases which I have examined during this inquiry, have induced me to hope, that by following up the observations which led to the discovery of the difference between the functions of the two systems of nerves, we shall eventually be able to ascertain, the cause of the variety of symptoms in paralysis.

In one man, I could see no symptom of palsy until he laughed or sneezed; while in another, the features distorted by paralysis regained their proper balance when the actions of laughing or sneezing were excited. In a third, the paralysis was apparent, not only while the features were at rest, as in the case of common palsy, but the distortion of the countenance, instead of being diminished, was increased to a most unusual degree, when the patient laughed or sneezed.

Before attempting to account for the variety of the symptoms in these cases, I propose to give a description of the nerves of the face, and of the changes produced on the expression in different animals, by cutting a branch of either set of nerves.

Two nerves which arise from distinct parts of the brain, and which differ from each other in structure and function, are distributed on the face in man, and on the corresponding parts in the greater number of mammalia.

The one hitherto called *Portio Dura* of the VIIth. belongs to the system of *Additional*, or *Superadded Nerves*, and passes to every muscle that is in any way connected with respiration or expression; while the other, called the *Vth*, or *Trigeminus*, is one of the original, or symmetrical system, and goes not only to the same muscles, but also to the skin, and to the deeper muscles, whose action is principally that of mastication.

The proportion of the Facial Respiratory Nerve to the Vth. is greater in man, than in any other animal. If we descend to the next link in the chain of beings (the monkey,) we shall find the proportion of it to be much diminished, and that of the Vth increased. The distribution of the nerve is more complicated in the monkey than in the dog, its intricacy being apparently in proportion to the number of the muscles of expression. From the lion, the dog, and cat, we descend to the horse, ass, and cow; in these animals, there is a marked difference in the distribution of the nerve, from that of either the monkey or the dog, for, excepting a few branches, which pass to the muscles of the external ear, and to the eyelid, the whole of the respiratory nerve is confined to the muscles of the nostrils and side of the mouth, while in the carnivorous tribes it is spread in great profusion overthe cheeks and side of the neck.

There are, however, some varieties in the classes of graminivorous animals. In the gazelle, sheep, and deer, the distribution of the nerve is still more simple than in the horse; while in the camel it is more profuse, and is, in this respect, intermediate between that of the carnivorous and the graminivorous animals. The expression of the enraged camel is sufficiently ferocious, and the manner in which he shews his tusks, when dying, is very similar to that of a carnivorous creature*.

We are told by those who have seen an elephant in a rage, that he is most sublime and terrific; but the anatomy of the

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* The anatomy of the nervous system of the camel is very different from that of the greater number of quadrupeds.

We had an excellent opportunity of examining it last spring, in the courier camel, or maherry, which was brought from the interior of Africa, by Captain Lyon, as a present to his Majesty. In the dissection of this animal we noticed many interesting facts, which have been overlooked by comparative anatomists, and particularly the difference of the distribution of the nerves of the neck and stomach, from that of the same nerve in the horse and ass, and from that in the greater number of ruminating animals.

We found that the nerves of the neck, in their number and distribution, resembled those of a large bird, as the swan, &c., much more than those of a horse or bullock, and particularly in the spinal accessory, or superior respiratory of the trunk, being either deficient altogether, as in birds, or quite different to what it is in the greater number of quadrupeds. This anatomical fact is a strong proof of Mr. Bell being correct in the opinion which he has formed of the use of this nerve. While in Paris last September, I was told that there was a specimen of the brain of a camel, in

Portio Dura leads me to suspect that the expression of rage, however terrible, must be quite different from that of the ferocious snarl of the lion. In the face it must be in a great measure confined to the contortions of the proboscis, and to the eye, for, excepting a few branches to the eyelids, the distribution of the nerve of respiration and expression is confined almost entirely to the proboscis*.

M. Cuvier's collection, in which the spinal accessory nerve might be seen; but on examining the preparation, I could find no trace of the nerve. There were some little filaments pointed out to me, as the origin of the nerve, but to this I could not assent. If these fibres, (which, however, in the present state of the preparation are very obscure,) be compared with the origin of the spinal accessory, even in the sheep, we shall be forced to conclude that, if there be a nerve of this kind in the camel, it is so small that the truth of the opinion, as to the use of the nerve in other animals, cannot be affected.

* During the last winter I often visited the Menagerie in Exeter Change, to study the motions and uses of the proboscis of the elephant, and this I had a good opportunity of doing, as the small elephant there, was so gentle, that he permitted me to handle his trunk freely. From the great power which the elephant has over his trunk, as a machine, I was certain that there must be large nerves running to it, similar to those which supply the fingers in man; but as the proboscis forms an important part in the respiratory system of this animal, I thought that, in the dissection of it, there would be the most distinct proof of the accuracy or fallacy of Mr. Bell's opinions on the subject of the *Portio Dura*. The animal died in the month of May, and, through the kindness of my friend Mr. Mayo, I was enabled to make an examination of the nerves of the trunk. The dissection was most satisfactory, for the trunk was found to be supplied, not only by branches of the Vth. pair, as described by Cuvier, but also by a very large branch from the *Portio Dura*.

The *Portio Dura* in this elephant was found emerging from the parotid gland, as in other mammalia. It gave off some descending branches to the neck, but passed from behind the jaw to the proboscis, almost as an entire nerve, and of the size of the sciatic nerve in man: in its course it had only given some small branches to the muscles of the eye, to those of the ear, and to a small muscle which corresponds with the platysma. Before it passed into the substance of the proboscis, it united with the second division of the Vth. pair, which comes forward from the infra orbital hole, in two large branches. The two nerves being then closely united, passed between the layers of the muscles, which form the greater

If we compare the anatomy of the facial respiratory nerve, in the various classes of birds, we shall find its distribution to be analogous to that of the same nerve in the different tribes of quadrupeds. In the game-cock, a few branches of the nerve pass to the loose skin under the jaw, which is dilated in crowing, the greater number being distributed on the muscles of the neck, which causes the elevation of the feathers when he puts himself in an attitude for fighting. But in the duck, which, when enraged, has little or no power of expression, the same nerve is not larger than a cambric thread, and passes only to the skin under the jaw.

The effect upon the muscles of the face, produced by cutting the facial respiratory nerve, is in the ratio of the intricacy of the distribution of the branches, and according to the proximity of the part cut, to the origin of the nerve from the brain.

I cannot detail any examples of the effect of cutting this nerve in man, for, though I have witnessed operations where it was done, yet, as they occurred before I was acquainted with the facts now established, I did not take notes of the consequences which ensued. However, I shall presently detail cases where, by disease or accident, the whole nerve in some instances, and part of it in others, has been injured. In those cases the symptoms very nearly corresponded with the phenomena presented, when the whole, or only a part, of the nerve has been divided in animals.

About four months ago, I divided the left facial respiratory nerve of the most expressive monkey I could find in the

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mass of the trunk. The *Portio Dura* became quickly diminished in size, as it gave off its branches in great profusion to the muscles: but the Vth. was continued down, as a very large nerve, to nearly the extremity of the trunk; in this respect resembling the nerves to the fingers in man. On making sections of the proboscis, near its extremity, a great number of these nerves were seen in its substance.

A few branches of the *Portio Dura* ran to the valvular apparatus in the upper part of the trunk; but this peculiar structure was supplied principally by a branch from the Vth. pair, which winded round under the orbit.

Menagerie in Exeter Change. The effect was immediate; his power of expression being destroyed on one side. When he was irritated, he snarled and showed his teeth on the right side only. During the first month, he could not shut the left eye, but of late, though he is able to close the eye, he has so little power over the motions of the eyelid, that when he is attacked with a stick, the orbicularis muscle appears to become so convulsed as to render the eye useless. He then seems to avoid winking with the other eye, that he may be on his guard.

The effect of injury to this nerve is more distinctly shewn on the human face. In the case of a little girl, which will be particularly related in another paper, the consequence of disease of the right *Portio Dura* is very striking. When she laughs heartily, the right cheek and the same side of the mouth are unmoved, while the muscles of the left side are convulsed with laughter.

If told to endeavour to laugh with the right side, she raises the angle of the mouth, but by an action which is evidently regulated by the branches of the Vth. nerve. This attempt to laugh gives a peculiarly droll expression to her face, and I think it is the same action which amuses us so much in the face of the famous mimick, who invites the public to see him *at home* every spring.

After having made the experiments on the *Portio Dura* of several animals, and observed the effect upon the human countenance, where the nerve had been injured, I was so much struck with the resemblance in the action of some of the muscles, to those of the actor alluded to, that I went to the Theatre to observe the expression of his face. Although there were evident marks of paralysis of the *Portio Dura,* there was a considerable degree of expression exhibited on the same side, for which I could not at the time account. It, however, appears to be now explained by the state of the little girl's cheek, for when she attempts to laugh with the right side, the expression is so similar, that it almost amounts to a proof that this performer has, by practice, gained such a power over the actions regulated by the Vth., as to be able to bring them into a state similar to that in natural laughter. But as this is done through the Vth., instead of the VIIth., it makes the expression on one side quite different from that on the other, and consequently gives a peculiarly ludicrous appearance to the whole countenance.

The experiment of cutting the facial respiratory nerve was performed on a dog. The following is the note made a few days after the nerve was cut: The dog is now quite well, having suffered very little from the operation; when he fawns, the right side of his face is completely motionless; (the nerve of the right side was cut.) When I threaten to strike him, although there is a tremulous motion expressive of fear in all the muscles of the left side of the face, the other is perfectly still; he cannot even close the eyelid, and instead of winking when he expects to be struck, the eyeball itself is turned up. When he is excited, there is an expression of alacrity in all the muscles of the left side of the face, and a brilliancy in the left eye, while the right is perfectly inanimate. This is shewn in an extraordinary degree when he is fighting with another dog.

The difference between the two ears is not so distinctly marked, for though the left ear be more elevated than the right, still there is an expression of alacrity in the erection of both ears. However, at this I was not surprised, as I had found, in a dissection which I had made of a dog, previous to performing the experiment, that the principal branches, which pass to the muscles of the ear, were so deeply situated, that to have cut them, would have probably endangered the animal's life. The effect upon the respiratory muscles of the right nostril, was not so distinct as in the experiment upon the ass,(which will be detailed presently,) but the power of giving that peculiar twist to the nose, which is so distinctly seen in a pointer setting, was destroyed.

This dog, in the course of two months, perfectly recovered the use of all the muscles which had been paralyzed; but this circumstance involves the question of the re-union of nerves, upon which I shall not at present enter.

I repeated the experiment on another dog in the month of May, and at the same time cut the infra orbital nerve of the opposite side. This dog is still in very good health, and now affords as striking an example of the effect of cutting the respiratory nerve of one side, and a branch of the Vth. of the other, as he did on the day, after the operation was performed. The effects produced by cutting the Vth. will be detailed presently.

I cut the same nerve of a cat, as near its exit from the stylo-mastoid foramen as I could. When she was irritated, she afforded an excellent example of paralysis of the action of the muscles regulated by this respiratory nerve. She spate with that side only, on which the nerve was entire. Here I succeeded in paralyzing the muscles of the ear; for, while she spate, the ear of the side on which the nerve was entire, was pulled back, while the other stood erect and motionless.

The same experiment is easily made on the ass, or it may at any time be done on an ox in the slaughter-house, for while the animal is insensible, and dying from loss of blood, we have only to divide the nerve, as it passes from before the ear; the convulsed motion of the nostril of the same side will immediately cease.

The effect upon the nostril is the most obvious symptom, when the nerve is cut in the ass*. If, after having cut the right

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* I performed this experiment on a horse at Charenton, at the request of M. Majendie, and with the assistance of M. Dupuy, Professor of the Veterinary College there. As I was anxious to execute the experiment quickly, and avoid the hemorrhage which is generally a consequence of seeking for the nerve in the middle of the parotid gland, I cut through the skin anterior to the jugum, in the hopes of immediately finding the nerve. A large branch, which I supposed to be the *Portio Dura*, was exposed, but on cutting it through, no paralysis of the nostril ensued.

There was naturally a degree of incredulity on the part of those who were spectators, as to the fact of the actions of the nostril being paralyzed by cutting the *Portio Dura*. But on saying to the gentlemen present, among whom was Dr. Spurzheim, that this being the first time I had made the experiment on a horse, I was afraid I had not cut the principal branch of the *Portio Dura*, they, with great liberality, afforded me every assistance. On examining the wound more particularly, I discovered another large nerve, and on cutting this the nostril was immediately paralyzed.

As I had some difficulty in understanding why the result of this experinerve, we hold the nostrils for a short time, so as to prevent the animal from breathing, he will, when freed, begin to snort, but with the left nostril only. If we hold carbonate of ammonia to the paralyzed nostril, he will not be affected; but if it be

ment was at first so different from those which we had made in London on the ass, and in which the nerves are so similar to those of the horse, I took the first opportunity of again examining the anatomy of the nerve. I then discovered a good reason for my failure, as a large branch which passes from the second division of the Vth. pair after running for a short way, parallel to to the *Portio Dura*, joins with it. It was this branch I had cut in the first part of the experiment.

I have since, with the assistance of my friend Mr. Cæsar Hawkins, repeated the experiment on the horse, and which, I am happy to say, can be done without giving the animal any pain, for the actions of the nostrils, and the irritability of the nerves, especially of the respiratory class, continue so long after the animal is insensible, that, by cutting the *Portio Dura* we may stop the convulsions of the nostrils, and afterwards, by exciting the nerve with the galvanic forceps, exhibit the peculiar set of actions regulated by it.

The experiment was made in the following manner:

As soon as the animal became insensible from loss of blood, I cut through the skin over the jugum, so as to expose the nerves passing down to the cheek and nostrils The nostrils, during this time, were in convulsive action, and corresponded with that of the chest. On cutting across the superior branch (which though principally of the Vth., had some branches of the VIIth. intermingled with it at the part cut,) scarcely any change was perceived; but on cutting the lower branch, which is almost entirely formed by the *Portio Dura,* the convulsive actions of the muscles moving the nostril immediately ceased. When this was repeated on the other side of the face, the same consequences followed. On afterwards irritating the extremities of the nerves with the galvanic forceps, the actions of the muscles of the nostrils were excited when the inferior branch was touched, but little or no effect was produced on touching the upper (the branch of the Vth.)

These circumstances afford a strong proof of the necessity of paying particular attention to the anatomy of the nerves, before we draw any conclusions from experiments upon them.

I may, however, observe that, although there was in this instance a degree of culpability, all the experiments which have been made in the present investigation have been founded on views taken from comparative anatomy, and from observations made on the habits of the various classes of animals; by following this plan the experiments have been seldom or never attended with conflicting results.

held to the other, he will snuff it up, and then curl the nostril, and have an expression in the whole of this side of the face, as if he were going to sneeze, while the right will remain quite unmoved.

After discovering that the plexus of the *Portio Dura* passed to the muscle which moves the feathers of the neck in some birds, I cut the nerve of one side in a game-cock. On opposing him to another cock, there was a marked difference in the erection of the feathers of the two sides; but as the action of the muscles of the entire side appeared to operate on the feathers of the other, I attempted to cut the corresponding nerve, but while doing this, the hook, by a sudden struggle of the bird, became entangled with the branches of the par vagum, and other nerves of the neck, which lie close upon the *Portio Dura*, and the animal immediately expired. I have not since repeated the experiment.

Though I have generally used the old nomenclature, *Portio Dura of the Vth*, yet I believe it to be quite an error to consider this nerve to be it in any way connectedwith the auditory nerve. In the duck, the distinction between the two is most complete, there not being the slightest connexion between them. Comparative anatomy would induce us rather to consider the *Portio Dura* as connected with the VIIIth., than with the *Portio Mollis.* Perhaps the name of "Respiratory Nerve, to the muscles of the face," as given by Mr. Bell, is the most appropriate, as it is indicative of its principal functions.

Comparative Anatomy of the Vth *Pair.*

This nerve is, in every respect, different from the facial respiratory nerve. The observations which have been made on its minute anatomy in man, and in the lower classes of animals, and the result of experiments, warrant the opinion formed by Mr. Bell; *viz.*, that the Vth pair is similar to those nerves which arise from the spinal marrow, and which, in their origin and distribntion, are so essentially different from the class of respiratory nerves.

The rudiments of the Vth may be discovered in the lowest classes of animals. If a feeler of any kind project from the head of an animal, be it the antenna of a lobster, the *moustache* of a phoca, or the trunk of an elephant, it is a branch of the Vth which supplies sensibility to the member, and regulates the voluntary actions of its muscles. Thus it may be compared to those nerves in man, which pass to the muscles of the arm, and to the tips of the fingers.

But this nerve is also, in the greater number of animals, connected with the organ of taste—and consequently it is very large; its magnitude being in proportion to the size of the apparatus connected with mastication and taste. Thus, in the lower scale of animals, the nerve is much larger in proportion than in man; indeed, its size, compared to that of the *Portio Dura,* may give us a better estimate of the comparative degree of the power of expression, than can be deduced from any other fact of anatomy. A good example is afforded in the goose or duck. In the latter bird, the six branches of the Vth, when laid together, form a mass equal in size to that of the largest nerve in a man's arm; while all the branches of the *Portio Dura* would not form a nerve larger than a common sewing thread.

In the cat, and in the hare, the branches of the Vth pass not only to the muscles, but also into the whiskers; while the branches of the facial respiratory nerve go past the hairs, and enter into the muscles, moving the tip of the nostril. It is rather difficult to demonstrate the nerves going into the bulb of the hair in these smaller animals, but it is easily done in the phoca. A preparation illustrative of this fact was shewn to me some years ago in Amsterdam, by Professor Vrolich; and in the first number of the *Journal de Physiologie Experimentale,* by M. Magendie, there is an account of "les Nerfs qui se portent aux moustaches du Phoque," by M. Audral. This fact of anatomy, which has been denied by some, is farther demonstrated by the dissection of those animals which have tufts of hair, or whiskers, over the eye. In the American squirrel I have traced branches of the first division of the Vth into the bulbs of the hairs over its eye-brow.

It will be unnecessary to give more proofs from anatomy of the distribution and *use* of this nerve being different from that of the *Portio Dura.* I shall now corroborate them by mentioning the result of two experiments.

In the same ass, of which the *Portio Dura,* or facial respiratory nerve of the right side, had been cut, the infra orbital branch of the Vth of the left side was divided.

Upon cutting this nerve no change was produced in the action of the muscles moving the left nostril in respiration, and this could be easily observed, as the other nostril had been paralysed, by cutting the *Portio Dura* of the right side. The effect, however, upon the muscles of the lip during feeding was very distinct, for although the same muscles were not paralysed in the act of respiration, still they were rendered incapable of mastication and voluntary motion, as the animal could no longer use this side of his lip in gathering his food *. The sensibility of this side was destroyed, for the animal did not start when it was pricked with a needle, as he did when the other was touched. I performed a similar experiment on a dog, which is still alive. When he is quiet, the nose is twisted to the side upon which the Vth is entire, and the *Portio Dura* deficient, but the moment he is excited, the nose is pulled to the other side.

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* In an account which M. Magendie has given of some experiments similar to those detailed in this paper, which he repeated at Charenton, he says, " Le résultat que nous avons obtenu s'accorde parfaitement avec celui que nous venons de rapporter, à l'exception toutefois, de l'influence de la section du sous orbitaire sur la mastication, influence qui n'a pas été évidente pour moi."

The idea that we had, in our experiments in England, found the power of mastication destroyed, must have arisen from the difficulty I found of expressing myself correctly in French, when discussing the results of the experiments with M. Magendie.—There is *one act* of mastication destroyed by cutting the infra orbital nerve, but to destroy the power altogether, it would be necessary to cut through the *trunk* of the Vth nerve of each side.

The difference of the degree of sensibility in the two sides corresponds with that found in the experiment on the ass.

A description of several different kinds of palsy will be given in my next communication, to prove that the changes produced on the human countenance in palsies, depend on the particular system of nerves diseased, and that the two systems are seldom or never affected at the same time. It will also be shewn, that, by exciting the actions regulated by the one system of nerves, the distortion caused by paralysis of the other will disappear.

By a knowledge of these facts, it may be expected we shall not only be able to form a more correct diagnosis of the nature and seat of palsy than heretofore, but also to estimate the degree of danger attending each class of symptoms.

Albany, London, Dec. 1, 1821.

ART. V. *Letter from* DR. COINDET, *of Geneva, to the Editor of the Journal of Science, on the administration of Iodine in Scrofula**.

WHEN I published my second Memoir, I already possessed a great number of good observations on what I regard to be the complement of my discovery, that is, the employment of iodine by friction in strumous swellings, and other scrofulous affections. An experience, much extended and varied since, has confirmed me fully in the belief that iodine is the specific of this class of maladies, against which we possessed only secondary remedies, namely, those which act indirectly, whilst I find that this excites directly and exclusively the lymphatic system, whence we obtain surprising success in the above cases.

I prescribe a *pommade*, consisting of hog's-lard an ounce and

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* Though most of the facts communicated in this letter have already appeared in our Journal, we have no doubt that our medico-chemical readers will peruse it with interest.

a-half, white wax two drachms, fused together, and united by trituration with 36 grains of hydriodate of potash, or of soda. The size of a hazel-nut of this ointment to be rubbed in every morning and evening, on the goitre, gland, swelled joint, chronic tumour, &c. Various effects result from this practice. One of these is a local irritation, or chapping of the skin, an inconvenience to be avoided. If there be a chronic phlegmasia in the scrofula of the neck, or in any of these strumous swellings, characterized by hardness, lancinating pains, we must stop the above treatment till, by leeches, emollient cataplasms, ipecacuan, and saline purgatives, we have paved the way for the success of the iodic frictions. But it happens pretty frequently that, in their turn, these frictions produce a sort of phlegmasia in the same swellings, that is, after a certain number of applications the ointment developes an excitement of the lymphatics, characterized by hardness, pain, and extreme sensibility of the scrofulous glands. We then must immediately suspend all friction, and pursue the antiphlogistic plan, sometimes very actively. Whether it be that the nature of the inflammation has been modified, or that the absorption alone has been powerfully augmented, the fact is, that the pains give way, and, after a few days, we are surprised to find that the obstructed glands are softened, smaller, and that the cure has been accelerated by that accidental disorder.

It happens also, but much more rarely, that when the iodine has been prescribed inwardly, there arises a constitutional action (agreeably to what I have stated in one of my Memoirs,) which we must regard, as an iodic super-saturation of the system. The patient becomes pale, he takes a complexion peculiar to the effects of the iodine, he turns lean, first of all where the frictions are applied, especially if that be the neck, and then the emaciation extends over the whole body, if the frictions be continued. The pulse becomes occasionally frequent, but at the same time the goitres, or scrofulous glands, dissolve during this action with great rapidity, so that in some cases the cure excites astonishment. Yet, by these means, we make the patient suffer unnecessary inconvenience and risk, injuring the reputation of this powerful medicine to no purpose. As soon as we perceive the glands, tumours, &c. soften and subside, we must suspend the frictions, and resume them after the interval of a few days.

I have ascertained, in a precise and certain manner, that the action of iodine continues some time after its administration is stopped. It is therefore absurd merely to say, as I have heard some "You have the goitre, take iodine." These scrofulous obstructions demand a careful treatment, which must be observed for some time, both internally and externally, with regard to this remedy. It, in fact, requires attention somewhat similar to those which we give to corrosive sublimate, when prescribed in syphilitic cases.

I continue always to prescribe iodine internally, but less frequently than I formerly did, because the frictions give the same results, without running the hazard of injuring the stomach; and it is a singular fact, that half a drachm, or 36 grains, of hydriodate of potash, under the form of inunction, cures or dissipates a goitre, or scrofulous tumours in the neck, in the very same space of time that the following solution does: Hydriodate of potash or soda 36 grains, iodine 10 grains, distilled water 1 ounce, which are united together. Of this solution 10 drops are taken three times a-day at first, and then gradually augmented in quantity.

I shall be particularly obliged to you, Sir, for communicating this letter to my medical brethren in Great Britain; and I remain your most obedient servant,

Z. Coindet, M.D.

Geneva, September, 1821.

ART. VI. *Process for procuring pure Platinum, Palladium, Rhodium, Iridium, and Osmium, from the Ores of Platinum. By* M. BARUEL, *Chemical Operator in the School of Medicine at Paris.*

Communicated by the Author*.—With Supplementary Remarks by the Translator.

1. Two sorts of platinum ore occur in commerce, one of which is white and brilliant, the other is blackish coloured. The latter contains much more iron † than the preceding; both ores exist always in the form of small spangles, which vary in size; platinum ore is one of the most compound known; besides the five metals above noted, several others are found in it, especially two kinds of ferruginous sand, one of them attractible by the magnet, the other not, and which is a combination of the oxides of titanium and iron; there is besides chromate of iron, some copper, particles of gold alloyed with silver, with copper, and mercury. It contains, moreover, some sulphuret of lead and copper. We may hence judge of the singular complexity of this mineral, and be ready to acknowledge that its exact analysis, in regard to the proportion of its constituents, is nearly impossible. In order to separate the platinum, palladium, rhodium, iridium, and osmium, from each other, and the rest of the bodies, the following method is the one which long experience has proved most successful.

2. The ore is triturated in a cast-iron mortar for a considerable time, during which a stream of water is constantly passed over it, to wash away the ferriferous sand, the titanite, and chromate of iron, reduced to an impalpable powder. When the ore is very brilliant it is left to settle for an instant; the

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* This valuable memoir derives peculiar interest from the large importation of the above ore, daily expected from South America, in consequence of the negociation between M. Zea and some London merchants.

† Rather the fine black powder, or ore of iridium and osmium, noticed in paragraph 8.—TR.

water is decanted off, and it is then exposed in a crucible to a red heat during a quarter of an hour. The whole mercury is thus volatilized, when we can readily distinguish the spangles of alloy of gold and copper by their colours.

3. The calcined ore being introduced into a tubulated retort, we pour over it half its weight of nitro-muriatic acid (aqua regia) composed of one part of nitric acid, at 25° Baumé (1.210 sp. gr.) and three parts of muriatic acid, at 18° (1.14), and heat the mixture for half an hour. Such acid dissolves all the gold, all the lead, the greater part of the copper, and a very small quantity of platinum, palladium, and iron, while the silver is converted into a chloride, which remains mingled with the ore not attacked. After decanting the acid liquor, the ore is thrown on a filter, and washed with a sufficient quantity of water. The filter-funnel being transferred to another vessel, the filter is to be washed with a very weak water of ammonia. By this means we dissolve all the chloride of silver, which is recovered by saturating the filtered liquor with muriatic acid.

4. The solution which contains the gold, lead, copper, and iron, with a small quantity of palladium and platinum, being added to the water which has served for the washings, the whole is now evaporated to the consistence of syrup, which is diluted with thrice its volume of water, and treated with sulphuric acid, drop by drop, to precipitate the lead in the state of sulphate, to be afterwards separated by the filter.

5. Into the filtered liquor a solution of proto-sulphate of iron must be poured, which throws down the gold and palladium in the metallic state. We decant the liquor, wash and dry the precipitated metals. The platinum remains in the liquor with the iron and copper. We concentrate this liquor by evaporation, then pour into it a sufficient quantity of a saturated solution of muriate of ammonia, which throws down the platinum in the state of ammonio-muriate. This must be washed on a filter and dried.

6. The gold may be very easily separated from the palladium by melting these metals with four times their weight of silver, and acting on the alloy with concentrated nitric acid, which

dissolves the palladium and silver, but leaves the gold in the form of a brown powder, which may fused into a button in a crucible. Into the nitric solution of silver and palladium we pour muriatic acid, which throws down all the silver in the state of chloride. The liquid freed by the filter from the chloride contains only palladium. We add to it a few drops of solution of sal-ammoniac, then saturate the redundant acid by ammonia; the whole palladium is thus precipitated in the state of an ammonia proto-submuriate of palladium, which exhibits small needles, of a delicate rose colour. This salt is to be washed on the filter, and dried.

7. The ore of platinum which has been successively treated with weak nitro-muriatic acid, and then with ammoniacal water, to carry off the chloride of silver, is to be strongly desiccated. Having replaced it in the retort, we pour over it a weight equal to its own of nitro-muriatic acid, made in the same proportion as the above, but with this difference, that the acids ought to be as concentrated as possible. I employ for this purpose nitric acid, at 40° (1.387 sp. gr.) and muriatic acid, at 23½° (1.195). The retort is placed on a sand-bath, with a tubulated receiver adapted to its neck, and it is heated moderately. A brisk effervescence soon arises, owing to the disengagement of much nitrous vapour, and a little chlorine. The action of the heat must be so modified as to produce the most beneficial effect on the solution, without volatilizing the acid. Finally, when the effervescence ceases, the fire is to be augmented till the liquid boils, and till no more orange nitrous fumes are disengaged.

When the action of the acid is quite exhausted, we decant the liquid into a matrass, and pour on the portion of the ore not attacked the same nitro-muriatic acid, equal in quantity to the first.

The mixture is to be heated anew, observing the same precautions as for the preceding solution. Finally, we treat the ore five times in succession with the compound acid. By this process six parts of this acid are sufficient to dissolve the whole platinum, palladium, and rhodium contained in the ore.

8. After the last digestion, which yields only a slightly

reddish-coloured solution, there remains a residuum, under the form of a brilliant blackish powder, which consists of an alloy of iridium and osmium. One part of this is a fine powder (see Note to paragraph 1,) and the other forms brilliant spangles. We shall return, in the sequel, to the residuum; let us employ ourselves at present on the solution.

9. We have said that all the platinum, rhodium, and palladium, were dissolved; but the acid also dissolves a little iridium and osmium, as well as the iron alloyed with the platinum grains. During the action of the acid on the ore, at the same time that the nitrous gas and chlorine are evolved, there is volatilized a little water and muriatic acid, which carry over with them a notable quantity of oxide of osmium, which is condensed in the receiver.

10. All the successive solutions of the ore of platinum are united and introduced into a retort of proper capacity, to which the receiver containing the former condensed vapours is attached. The retort is now heated on a sand-bath, till its contents acquire the consistence of syrup. By this means we drive off all the excess of the acid, which carries along with it into the receiver the whole oxide of osmium which that solution contained.

11. The product of the last distillation being saturated with lime, we distil over to one-half the volume. The product of this new distillation has an extremely penetrating odour, on account of the large proportion of oxide of osmium which it contains. It must be preserved in glass bottles, furnished with well-ground stoppers.

12. The concentrated solution of platinum is to be diluted with from five to six times its weight of water, then filtered.

13. The black powder which was not acted on by the nitro-muriatic acid, is also to be washed with water, dried, and kept in a phial; we shall distinguish it by the name of the *black powder.*

14. Into the filtered solution we pour a saturated solution of muriate of ammonia, till this ceases to occasion any precipitate. In this operation there are formed ammonio-muriates of platinum, iridium, rhodium, and palladium. These two

last salts being very soluble, remain in the liquid with the iron, but the ammonio-muriates of platinum and iridium being very sparingly soluble form the precipitate, which has a tawny or reddish-yellow colour, of more or less depth, according as the proportion of the salt of iridium is more or less considerable. When the further addition of the muriate of ammonia produces no more precipitate, the whole is to be thrown on a filter of cotton, and washed with water of as great coldness as possible, which is conveniently procured by putting a bit of ice into the water intended for the washings. When the precipitate is sufficiently washed, which is recognised by the water that passes having merely a faint yellowish hue, it is to be dried. This precipitate, as we have remarked, is an ammonia-muriate of platinum, the pure yellow of which is altered by its mixture with the ammonio-muriate of iridium, which is red.

15. This impure ammoniacal salt of platinum is calcined in a crucible, observing to heat the crucible at first in its upper part, in order to avoid the volatilization of a portion of the salt, without its being decomposed. The heat is to be pushed to redness, at which temperature it must be kept up for an hour. By this means the salts are decomposed, and there remains in the crucible only the platinum and iridium. To separate these two metals we put them into a retort, and dissolve them anew in the nitro-muriatic acid; but in this case the nitric acid must be only at 28° (1.24), and the muriatic acid at 19° (1.15). Two and a-half parts of this acid suffice to dissolve one of platinum thus reduced, without affecting the iridium. This metal remains at the bottom of the liquor (which is of a fine orange-yellow colour,) under the form of a grey powder. On filtering, pure iridium remains above, which is to be washed and dried.

16. The solution of platinum must be precipitated once more by muriate of ammonia; and the fine yellow ammonio-muriate of platinum thus obtained, is to be reduced by strong calcination in a crucible, observing the precautions already indicated. The pure platinum remains in the crucible, under the form of a greyish-coloured spongy mass, which acquires metallic lustre by friction against any hard body.

17. As platinum can be fused only in small masses at a time, and at a flame supplied with oxygen gas, or the compound flame of oxygen and hydrogen, it cannot be melted on the large scale like most others. However, chemists have succeeded in forming this metal into ingots of a very considerable weight, by uniting the particles with strong pressure at a very high temperature. For this purpose, a certain quantity of platinum, resulting from the calcination of the triple ammoniacal salt, is compressed in a crucible; then more is successively introduced, even to the amount of 20 or 30 pounds. The crucible is then covered, and heated to whiteness. The platinum is now transferred as speedily as possible into a square steel matrix, (a strong hoop of steel, jointed, would answer equally well) and capable of opening into two pieces by means of hinges. On the top of the ignited mass, a steel mandril, adapted to the cavity of the matrix, is to be applied, which is to be rapidly driven home, by three or four blows of a strong coining screw-press. By this powerful pressure, which the spongy platinum experiences at a white heat, it diminishes greatly in bulk, and its particles already acquire a pretty strong cohesion. The matrix, or collar, is opened, the mass of platinum is removed to be heated anew in a crucible to a red-white heat, at a fire acted on by two good bellows. It is again introduced with the utmost celerity into the matrix, where it receives five or six blows of the fly-press. In the second operation, all the particles of the platinum are sufficiently approximated to form a homogenous mass, which may be thenceforth heated, without inconvenience, among naked charcoal, giving it the greatest possible heat, and condensing, with two blows of the press, each face of the ingot. In thus transferring the mass of platinum successively, from the forge to the press about thirty times, we obtain an ingot perfectly sound, possessed of great malleability and ductility. Platinum thus made into ingots, is delivered to the workmen, who fashion it like gold and silver; that is to say, all the pieces are stretched at first under the rolling-press, and then fashioned by the hammer, taking care to anneal it from time to time. Thus are prepared, in France, the great masses of platinum, with

which are fabricated the large alembics, destined for the concentration of sulphuric acid.

18. The mother-water, from which have been precipitated the ammonio-muriates of platinum and iridium by pouring muriate of ammonia into the solution of crude platinum, has a reddish-brown colour, and contains all the ammonio-muriates of palladium and rhodium, as well as a certain quantity of the ammonio-muriates of platinum and iridium; because, as we have observed, these salts are not completely insoluble. It contains, moreover, all the iron which was alloyed with the platinum, and sometimes a little copper, which has escaped the action of the first portion of nitro-muriatic acid which was poured on the ore to dissolve the gold. This mother-liquor is put into matrasses, and plates of iron are plunged into it. The iron precipitates all the metals (except the oxide of iron) under the form of a black powder. When the whole metallic matter is thrown down, which is known by the liquor assuming a green colour, the plates of iron are removed, after detaching from their surfaces the adhering powder. The liquor is decanted off, and thrown away. The black precipitate must be washed several times, till the water employed passes off tasteless. The powder is then treated with weak nitric acid, which dissolves the greatest part of the iron, which, by the effect of the precipitation, had been alloyed with these metals*, and which takes up also whatever copper may remain. The residuum is washed anew, and treated with nitro-muriatic acid, which dissolves all the platinum, palladium, rhodium, and remains of the iron; but does not affect the iridium, which remains pure at the bottom of the solution in the form of a black powder, or metallic spangles. The iridium, being separated by the filter, is then washed, dried, and united to that formerly obtained (15).

19. The liquors are now to be united, and evaporated to the consistence of syrup, to drive off the greater part of the

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* Or, *during the precipitation had fallen down in alloy with these metals.* The original words are, " fer, qui par l'effet de la précipitation, s'étoit allié avec ces métaux."

acid excess; then this is to be diluted with four or five times its weight of water, as cold as possible. Into this a solution of muriate of ammonia is to be poured, till it ceases to occasion a precipitate. What falls is an ammonio-muriate of platinum, which must be separated by filtration. The solution is then concentrated, and allowed to cool several times in succession, to separate all the ammoniacal salt of platinum which it may contain. When the liquid is completely deprived of platinum, or when it yields no longer the yellow precipitate, we dilute it with five or six parts of cold water; and it ought to have a sensible excess of acid. This, if wanting, may be supplied by adding a little of the muriatic. We then pour into it water of ammonia, drop by drop, but not so much as entirely to saturate the acid-excess. Immediately there is formed, in the liquid, a precipitate in the shape of small needles, delicate and shining, possessing a beautiful pale rose-colour. This crystalline precipitate is an ammonio-subprotomuriate of palladium. Since this salt is insoluble, there can remain none of it in the liquid. It may be separated by the filter, and washed with very cold water. By heating this salt to redness in a crucible, the palladium remains pure. It may be afterwards melted in a cavity of ignited charcoal, on which a stream of oxygen gas is made to play.

20. The liquid freed from the salt of palladium, possesses a fine currant-red colour, derived from the ammonio-muriate of rhodium, which it holds in solution, and which is very soluble. It contains, moreover, a little muriate of iron, and occasionally a little muriate of copper, when this metal has not been entirely dissolved by the first portion of nitro-muriatic acid, which was made to act on the ore, as has béen stated above. There are two modes of treating this salt, to obtain pure rhodium. The first consists in evaporating this liquid, at a gentle heat, to dryness; and boiling the residuum several times along with absolute alcohol. The spirit dissolves all the muriate of iron and copper, with the excess of sal ammoniac, and does not affect the ammonio-muriate of rhodium, which remains in the form of a saline powder of a fine carmine-red colour By calcining this

salt to redness in a crucible, we decompose it, and the rhodium remains pure and perfectly metallic. The second means of obtaining the rhodium from the above iquid, consists in plunging into it plates of iron. The rhodium and the copper are precipitated, carrying down with them a little iron. When every thing is fallen down, the liquor is decanted, the precipitate is washed, and boiled with an excess of strong muriatic acid, which dissolves all the iron. The liquid is now poured off, the residuum is washed with a sufficient quantity of water, and is next boiled several times with concentrated nitric acid, which dissolves all the copper. The rhodium being completely insoluble in each of these acids separately, remains under the form of shining pellicles, which must be washed and dried. Rhodium being the most infusible of metals, cannot be melted but in small pieces, by the aid of a flame fed with oxygen gas, or by the compound flame of hydrogen and oxygen. (See Annotations *infra.*)

21. Let us return to the black powder separated from the platinum ore, by treating it with nitro-muriatic acid. We have said that this black powder was an alloy of osmium and iridium. It is scarcely affected by any nitro-muriatic acid. It requires, indeed, an enormous quantity of this acid to dissolve a minute particle of it. The only means of attacking this alloy, is to calcine it with nitrate of potash. With this view, we triturate the black powder with twice its weight of a mixture of three parts of nitre and one of caustic potash, and introduce the whole into a silver crucible, which is to be kept at a cherry-red heat for half an hour. In consequence of the affinity of the potash for the oxides of osmium and iridium, the nitric acid of the nitre is decomposed, and oxidizes these metals. The crucible is to be withdrawn from the fire, allowed to cool, and cold water is then poured on the materials. This dissolves the potash, the whole oxide of osmium, and a little of the oxide of iridium. The whole being thrown on a filter, the oxide of iridium remains above, which is to be washed and dried.

22. The filtered liquor which contains the combination of potash and oxide of osmium, as well as a little oxide of iridium, is

put into a flask, and saturated with nitric acid. The liquid is then put into a retort, to which is fitted a tubulated globe, surrounded with moistened cloths. On distilling, the water which rises in vapour carries with it all the oxide of osmium. When the liquid is two-thirds drawn over, the whole osmium is usually volatilized. The liquid remaining in the retort contains the nitrate of potash, and a trace of iridium. The aqueous solution of osmium is as colourless and limpid as distilled water. It has a strong and peculiar odour, extremely irritating to the nostrils, and which it is dangerous to inhale for any length of time. In order to obtain the osmium from this solution, it is put into a matrass, and we add a little muriatic acid to acidulate it slightly, and then insert a plate of pure zinc. The oxide of osmium is decomposed by the zinc, which is dissolved in the muriatic acid, and the osmium is precipitated to the bottom of the liquor in the form of a blackish-blue powder. When the oxide of osmium is completely decomposed, which may be recognised by the liquid losing its odour, we decant the fluid, pour the powder of osmium on a filter, wash it copiously with water, dry it, and put it immediately up in a well-stopped phial.

23. The oxide of iridium, proceeding from the calcination of the black powder with nitre and potash, which remained on the filter, is by no means pure. It is a mixture of oxide of iridium, of a certain quantity of the black powder, or alloy of osmium and iridium, which has not been affected by the nitre, and a little oxide of silver, derived from the crucible. This mixture is to be treated with nitro-muriatic acid, which dissolves only the oxide of iridium, converts the oxide of silver into a chloride, and does not act on the alloy. We next filter and wash. The unattacked alloy, and the chloride of silver, remain on the filter. This residuum is to be washed with water containing a little ammonia, which dissolves the chloride of silver, while the alloy of osmium and iridium remains pure. This may be again calcined with the mixture of nitre and potash, to decompose it completely.

24. Into the solution of iridium, which is of a very deep reddish-brown colour, muriate of ammonia is to be poured, and the

liquid is to be evaporated to dryness, at a gentle heat. The residuum is to be then treated with alcohol very highly rectified, which takes up the excess of sal ammoniac, and occasionally a little muriate of iron; because the alloy sometimes contains a little of this metal. When the alcohol is no longer coloured, the ammonio-muriate of iridium remains pure. It is necessary merely to calcine it strongly in a crucible to have pure iridium. This metal, being more infusible than rhodium, can be melted only in very small quantities by the oxygen on charcoal, or hydrogen blow-pipe.

Remarks by the Translator.

The preceding process was obligingly drawn up by M. Baruel, at the request of a chemical gentleman from this country, who wished to know the actual method now practised at Paris, for preparing the great masses of platinum of which Coturière forms his beautiful alembics. Permission was given to publish the process in a work on practical chemistry, now in preparation; but as the period of its appearance is a little uncertain, it has been thought fit to give it to the world in the present form. Though it be fundamentally the same with that published by M. Vauquelin in 1813, in the 88th volume of the *Annales de Chimie;* yet it is much superior in the clearness, method, and definiteness of the operations. These indeed are so explicit, that one of the most delicate processes of chemistry, is thus brought within the compass of almost every operative chemist. The paragraphs are here numbered, for reference in annotation.

The preliminary observations in § 2, are very useful; they are not given by M. Vauquelin. M. Baruel is judicious in prescribing, with M. Laugier, a retort and receiver for conducting the aqua-regia solutions. In all cases where nitric or nitromuriatic acid is concerned, a great waste and annoyance are occasioned by the escape of acid vapours. These may be effectually saved, by connecting, with a somewhat long and wide tube, the first balloon to a second containing a very little water. The first balloon should have three apertures; one connected with the retort, a second in the same line connected with the receiver, and a third on the top, into which a small glass tube proceeding from a gasometer, filled with common air, is fixed. The tube which joins the two balloons, should have a slight declivity from the retort, and should dip at its end, into the water of the second. The outlet of this last must be closed with a Welter's tube of safety. When nitrous vapours are coming over open the stop-cock of the gasometer, and transmit into the first balloon, a moderate current of air. Its oxygen will immediately re-acidify the nitrous gas, which will condense in the form of nitric or nitrous acid, in the second receiver; while the azote will pass off. By M. Baruel's arrangement, § 7 and § 9, much of the osmium is preserved; which by following M. Vauquelin's directions is lost sight of, and dissipated.

In § 12, M. Baruel desires us to dilute with 5 or 6 parts of water, the concentrated platinum solution. M. Vauquelin, says that " ten parts of water and one of the solution in a state of great concentration, appear to me to be the best proportion." " Without the precaution of dilution," he observes, " it would be very difficult to wash the precipitate, and it would remain mixed with iron, and with the other metals, that happen to be present. It is better that the whole platinum should not be precipitated, than that the precipitate should be impure; because the platinum remaining in solution is separated in the subsequent processes." " Ammonio-muriate of platinum is not pure," he subjoins, " unless it has a lemon colour, does not become brown on drying, and is easily reduced to powder."

In a subsequent part of his interesting Memoir, M. Vauquelin states, that on treating the black metallic precipitate, (obtained by immersion of iron plates into the solution freed from platinum by sal-ammoniac,) successively with cold nitric, and muriatic acids, washing and drying the residuum, very acrid white vapours rose, which he ascertained, by heating a portion of the residuum in a crucible, to be a mixture of calomel and muriate of copper. " The sublimate," adds he, " contained likewise globules of mercury, and a black matter, which I suppose, from the smell which it exhaled, to be osmium." The fact is, that the chief part of the acrid vapours, was oxide of osmium, which was unnecessarily wasted.

The note at paragraph 18, has its commentary in the following observation of M. Vauquelin. "The muriatic acid employed after the nitric, contained likewise a great deal of iron, some copper and palladium, and even platinum and rhodium. This shews that a portion of these last three metals is precipitated by the iron in the state of oxide; otherwise muriatic acid would not have dissolved them. This seems to prove likewise that these metals in precipitating combine with iron and copper, and prevent them from being attacked by nitric acid, even when employed in considerable quantity." The great quantity of oxide of iron which precipitates along with the platinum, palladium, and rhodium, is very remarkable."

The process in § 15 and § 16, is a refinement on the former methods. M. Vauquelin contents himself with the first precipitate of platinum by sal-ammoniac, regarding it as quite pure. A great master of chemical analysis, to whom this process of M. Baruel was shewn, remarked, that his aqua-regia contained too much nitric acid; that if the suitable proportion of muriatic acid be employed, then the platinum may be thrown down pure, at first, by sal-ammoniac; and that M. Baruel dissolves by his menstruum, a portion of iridium.

The separation of palladium, under the form of an ammonia-submuriate in § 19, is precisely Vauquelin's method. Of this plan, M. Vauquelin speaks with complacency. "This process, more simple, and more exact than that of Wollaston, depends 1. On the insolubility of ammonio-muriate of palladium, even in water slightly acidulous. 2. On the solubility of the muriates of copper and iron in alcohol, and the insolubility of ammonia-muriate of rhodium in the same liquid." Yet the following extract from the same elaborate Memoir, seems to invalidate the above certainty of separation. "Now to obtain the rhodium, I concentrate the liquids from which the palladium has been separated, till they crystallize totally on cooling. I put the crystals aside to drain; they are *frequently* of two kinds and of two colours. Some have the form of hexahedral plates, and a fine ruby-red colour; while others, fewer in number, are square prisms of a yellowish-green colour. These last are ammonio-muriate of palladium." Thus we see that the ammonio-muriate of palladium has remained in solution, contrary to the statement above.

Dr. Wollaston's original method of separating rhodium from palladium and platinum is peculiarly elegant and economical, while it is perfectly exact. Into the solution deprived of the greater part of its platinum by sal-ammoniac, he immerses a piece of clean zinc. This throws down all the metals in the state of a black powder, except the iron, which remains in the solution. The copper and lead of the precipitate, weighing from 40 to 50 grains, being removed by very dilute nitric-acid, the remainder after being washed, was digested in dilute nitro-muriatic acid, which dissolved the greater part. "To this solution were added 20 grains of common salt, equal to one-fifth of the ore employed: and when the whole had been evaporated to dryness with a very gentle heat, the residuum, which I had found from prior experiments would consist of the soda-muriates of platina, palladium, and of rhodium, was washed repeatedly with small quantities of alcohol, till it came off nearly colourless. There remained a triple salt of rhodium, which by these means is freed from all impurities*." From the solution of this soda-muriate in water, zinc throws down the metal in a black powder, which may be agglutinated into a somewhat spongy metallic mass, by the heat of a very powerful furnace. Dr. Clarke, of Cambridge, is the only chemist, I believe, who has succeeded in giving to rhodium a high density, by fusion before his oxy-hydrogen blow-pipe. In an obliging communication to the present annotator, this gentleman states, that "Dr. Wollaston's own rhodium, taken from the pure specimen he gave me, is rendered perfectly malleable before the gas blow-pipe, and in this malleable state, after being hammered, its specific gravity, (estimated in distilled water, at the temperature of 60° of Farenheit,) equals 20.03, being as high as that of pure hammered platinum." Dr. Wollaston's rhodium, fused in his own air-furnace, has a specific gravity of only 9.74 by our experiments.

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* *Phil. Trans.* for 1804, page 423.

The separation of the salt of rhodium from those of platinum and palladium, as performed by Dr. Wollaston for the instruction of his scientific friends, is one of the most striking phenomena of chemical analysis. He mixes the three triple salts with a little water, in a watch glass, and evaporates to such a degree as leaves the mass apparently dry, yet with as much combined water as would give it the watery fusion at a higher heat. He pours on this a little alcohol, about specific gravity 0.850, and applies a gentle heat. Instantly after fusion, two very distinct and dissimilar fluid strata are formed; a red-coloured liquid above, and an oily-consistenced liquid below, of a dark-brown colour. The former is to be immediately poured off. The latter after being washed with a little more alcohol, yields a pure soda-muriate of rhodium.

The plan of separating palladium from its metallic associates in the platinum ore, originally invented by Dr. Wollaston, is very beautiful, and certainly in no respect inferior to that prescribed by M. Vauquelin. "To a solution of crude platina, whether rendered neutral by evaporation of redundant acid, or saturated by addition of potash, of soda, or ammonia, by lime or magnesia, by mercury, by copper or by iron, and also whether the platina has, or has not, been precipitated from the solution by sal-ammoniac, it is merely necessary to add a solution of prussiate of mercury, for the precipitation of the palladium. Generally for a few seconds, and sometimes for a few minutes, there will be no appearance of any precipitate; but in a short time, the whole solution becomes slightly turbid, and a flocculent precipitate is gradually formed, of a pale yellowish-white colour. This precipitate consists wholly of prussiate of palladium, and when heated will be found to yield that metal in a pure state, amounting to about four or five tenths per cent. upon the quantity of ore dissolved.

"The prussiate of mercury is peculiarly adapted to the precipitation of palladium, exclusive of all other metals, on account of the great affinity of mercury for the prussic acid, which in this case prevents the precipitation of iron or copper; but the proportion of mercury does not by any means influence the quantity of palladium, for I have in vain endeavoured, in the above experiment on crude platina, to obtain a larger quantity of palladium than I have stated, by using more of the prussiate of mercury, or to procure any precipitate by the same means from a solution of pure platina. The prussiate of mercury is consequently a test by which the presence of palladium may be detected in any of its solutions*."

In paragraph 22, M. Baruel directs to saturate the potash, in the alkaline compound of oxide of osmium, with nitric acid, and then to distil; but as the slightest excess of this acid would cause it to come over with the volatile oxide in the subsequent distillation, it seems preferable to employ sulphuric acid, as enjoined by Mr. Tennant. This excellent chemist also suggested the collection of the oxide that rises during the solution of the iridium ore. "As a certain quantity," he says, "of this oxide is extricated during the solution of the iridium in marine acid, that part may also be obtained by distillation†." M. Thenard does not seem to have been aware of this fact, since he lately ascribes to M. Laugier the merit of the same suggestion. "Il a reconnu que, dans le traitement de la mine de platine par l'acide nitro-muriatique, une partie d'osmium était attaquée, et que cette partie se vaporisait avec une certaine quantité d'acide‡."

It is hardly necessary to allude to the coarse method of the Marquis Ridolfi, described in the first volume of this *Journal*, page 259. To fuse crude platinum with half its weight of lead, to reduce the alloy to powder, to mix it with sulphur, to expose it to a strong heat in a covered crucible; to re-melt, with a little lead, the brittle button first obtained, and to hammer it at a white heat, upon a hot anvil, to extrude the lead, are directions nearly impracticable; and useless if they could be practised; for the foreign metals cannot be thus separated from the platinum.

The practice in Paris of alloying the pulverulent pure platinum with one eighth of its weight of arsenic; and of exposing the ingot of alloy to an open heat, progressively raised to white-

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* *Phil. Tran.* 1805, page 326.

† *Phil. Tran.* 1804, page 416.

‡ *Traité de Chimie*, vol. ii, page 690, first edition.

ness, in order to expel the oxidized arsenic, is, I believe, generally abandoned. Uncertain portions of the arsenic used to lurk in the platinum, and to deteriorate its valuable qualities. The platinum of England and France seems to be at present equally pure, malleable, and ductile; and the price is nearly the same. There is, therefore, no ground for those jealous preferences which the narrow-minded of both nations are apt to give to their labours in this respect. As they form the most enlightened portion of Europe, so they should unite in the most cordial and generous co-operation, for the improvement of Science and the Arts. These reflections have been suggested by the following handsome remarks of M. Vauquelin, written during a period of national hostility :—" Though Dr. Wollaston operated only on 1000 grains of the ore of platinum, and of course had only six or seven grains of the new metals at his disposal, yet he determined their principal properties, which does infinite honour to his sagacity; for the thing appears at first view incredible. For my part, though I employed 60 marcs (about 39⅓ troy pounds) of crude platinum, I found it very difficult to separate exactly the palladium and rhodium from the platinum and the other metals, which exist in that ore, and especially to obtain them in a state of purity*." Indeed, the most eminent men of the one nation are most ready to allow due merit to their rivals in the other. Jealous detraction is cultivated only by the subalterns, to lessen the sense of their own inferiority, or to gratify the prejudices of the great and little vulgar, whose suffrages they court.

ART. VII. *Contributions towards the Chemical Knowledge of Mineral Substances. By the late* MARTIN HENRY KLAPROTH.

[Continued from Page 40, of Vol. XII.]

Analysis of Cererit. (Cerite.)

I RECEIVED the specimens employed in the following Analysis from M. Geyer, of Stockholm, in the year 1788.

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* *Annales de Chimie*, for Nov. 1813.

A.

1. A piece of cererit was heated to redness in a platinum crucible; it lost 2 *per cent.* of its weight, and its original red colour became brown: its form remained unchanged.

2. 100 grains in fine powder, exposed for half an hour to a strong red heat, lost 5 grains, and acquired a darker colour.

B.

100 grains mixed with 200 of carbonate of potassa, and ignited in a platinum crucible, shewed no tendency to fusion, but assumed the appearance of a light brittle mass of an ash-grey colour. Being saturated and washed, the filtered alcaline liquor was colourless. It remained transparent when neutralized with nitric acid, and therefore contained no oxide of tungsten; nor was any other acid present, since the solution was not affected by the addition of solution of silver, mercury, lead, iron, baryta, *&c.*

The lixiviated residue was decomposed by repeated boiling in nitric acid. After the separation of the silica, the nitric solution was supersaturated with caustic potassa, boiled, filtered, neutralised with muriatic acid, and mixed with carbonate of potassa; but nothing was thrown down, nor did it become turbid.

C.

a. 400 grains of the levigated fossil were digested in 4 ounces of boiling muriatic acid, to which 1½ ounces of nitric acid were afterwards added. When every thing except the *silica* appeared to be dissolved, the liquid was filtered off, and the residue, washed and ignited, weighed 138 grains.

b. The excess of acid in the straw-coloured solution was then neutralized with liquid ammonia, taking care that nothing was thrown down; succinate of ammonia was then added, which caused a precipitate of succinate of iron, and which, after washing and ignition, afforded 14 grains of *oxide of iron.*

c. The solution, thus freed from iron, was precipitated by caustic ammonia; the *oxide of cerium* thus thrown down was edulcorated and ignited; it was of a brown colour, and weighed 218 grains.

d. The remaining fluid was boiled with carbonate of potassa; it afforded 9 grains of carbonate of lime, equal to 5 grains of pure *lime.* The fluid was then saturated with nitric acid, and tested by prussiate of potassa, which gave slight traces of the presence of copper. One hundred parts therefore of cererit contain,

Oxide of cerium	C *c*	54.50
Silica	— *a*	34.50
Oxide of iron	— *b*	3.50
Lime	— *d*	1.25
Water	A 2	5.
		98.75

Properties of the Oxide of Cerium.

a. Thrown down from its nitric solution by ammonia, oxide of cerium appears in the form of a muddy-red precipitate, which dries into a transparent and hard mass; when ignited, it acquires the form of a cinnamon-brown powder.

b. Carbonated alcalis separate a white carbonate, of which 100 grains (precipitated by carbonate of ammonia) lost 23 grains during solution in nitric acid; 100 grains of the same carbonate lost by ignition 35 grains; hence it is composed of

Oxide of cerium	65.
Carbonic acid	23.
Water	12.
	100

c. Before the blow-pipe, oxide of cerium becomes brilliantly luminous, without fusion. With phosphate of soda it produces a yellow bead which becomes colourless when cold. Borax produces the same appearance.

d. It undergoes no change by long-continued exposure to a bright red heat in a charcoal crucible.

e. Employed as an enamel, it gives a light-brown colour.

f. Neither caustic nor carbonated alcalis dissolve the recently precipitated oxide of cerium.

g. Carbonate of cerium is easily soluble in the acids, forming a sweetish astringent neutral solution; when diluted, the solutions are colourless, but become reddish-purple if concentrated.

h. Sulphate of cerium crystallizes in truncated octoëdra of a pale amethyst colour, and very difficultly soluble.

i. Nitrate of cerium is difficultly crystallizable and deliquescent.

k. Muriate of cerium forms prismatic crystals. Chlorine is evolved during the digestion of oxide of cerium in muriatic acid.

l. Acetic acid scarcely acts upon oxide of cerium, but it easily dissolves the carbonate, forming a very soluble white salt.

m. Sulphate of soda added to nitrate or muriate of cerium, forms a difficultly soluble white precipitate of subsulphate of cerium. This precipitate boiled in a solution of carbonate of soda, yields a perfectly pure carbonated oxide of cerium.

n. Sulphurous acid dissolves oxide of cerium, and the solution affords prismatic crystals of a pale amethyst colour.

o. The neutral phosphates, tartrates, and succinates form white precipitates in nitrate and muriate of cerium, which are soluble in acids.

p. Oxalic acid and the neutral oxalates occasion a precipitate in the solutions of cerium, which, unlike the former, is insoluble in nitric and muriatic acid.

q. Prussiate of potassa produces a white precipitate soluble in the acids.

r. Tincture of galls occasions no change.

s. Neither does sulphuretted hydrogen produce any change in the oxide of cerium; the precipitate occasioned in its solutions by hydro-sulphuret of ammonia, is of the same whitish-yellow colour as when pure ammonia is used.

Analysis of a compact Ore of Titanium from Arendal.

a. 300 grains of this ore in fine powder were fused with six times their weight of carbonate of potassa; when cold a grey porcellaneous mass was obtained, which by digestion in water afforded a carbonate of titanium, weighing, after having been washed and dried, 520 grains.

Prussiate of potassa and tincture of galls gave characteristic green and red precipitates in the muriatic solution of the above carbonate.

b. The alcaline washings of the carbonate did not become in the smallest degree turbid when neutralized by sulphuric acid.

This mineral therefore is to be regarded as pure oxide of titanium.

Analysis of the Topaz.

This mineral is especially characterized by the changes which it undergoes when subjected to a white heat; it then completely loses its original appearance, becomes soft, dull-white, and opaque, and *loses considerably in weight.*

[Here follows an account of the causes to which this loss of weight have been erroneously attributed, and to the experiments of Margraaf, Bergman, Wiegleb, Vauquelin, and Lowitz; the author also alludes to the probable existence of fluoric acid in the topaz, and then proceeds with his Analysis.]

Analysis of the Saxon Topaz. Sp. gr. 3.545.

A.

100 parts of coarsely-powdered topaz heated till the fragments began to fuse, in a coated retort, connected with the mercurio-pneumatic apparatus, afforded no gaseous matter, and lost little in weight.

The same portion, heated to whiteness in a blast-furnace for an hour, became white and pulverulent, and lost 22 per cent. in weight.

B.

300 grains of powdered topaz, distilled to dryness with sulphuric acid, produced a corrosion in the neck of the retort, resembling that of fluoric acid.

C.

200 grains of powdered topaz were projected into an ounce of nitre, fused in a platinum crucible; the mixture after a time concreted. The alcaline mass was then softened in water

saturated by nitric acid, and filtered. Lime-water, added to the filtered liquor, gave a precipitate having the characters of fluate of lime.

D.

100 parts of levigated topaz were fused in a platinum crucible, with 400 parts of nitrate of baryta; the fused mass was powdered, diffused through water, and mixed with excess of sulphuric acid; the precipitate being separated, excess of ammonia was added, and the liquor was filtered off and evaporated to dryness; the dry salt was wholly dissipated by heat, and gave no traces of a fixed alcali.

E.

a. 200 grains of topaz were bruised in a steel-mortar, and afterwards triturated as fine as possible in one of agate. The powder had sustained an increase of 4 grains. It was mixed in a silver crucible with 3 ounces of ley, (composed of equal parts of pure potassa and water,) evaporated to dryness, and ignited for an hour. The grey mass was then softened with water, and digested in muriatic acid, which entirely dissolved it, forming a clear yellow liquor. This was evaporated to dryness, water was poured upon it, and the insoluble siliceous residue thus obtained being completely washed, dried, and ignited, weighed 74 grains; deducting 4 grains derived from the mortar, the quantity of *silica* in 100 of the topaz is thus found to amount to 35 grains.

b. The muriatic solution, heated to its boiling point, was saturated by carbonate of potassa; the edulcorated precipitate weighed 218 grains.

c. 109 grains of this precipitate were re-dissolved in muriatic acid, and the solution tested by oxalate of potassa, which occasioned no change. It was then precipitated by caustic potassa, which being added in excess re-dissolved the whole of the precipitate by the aid of heat, with the exception of a scarcely perceptible portion of *oxide of iron.* The alcaline liquor, supersaturated by muriatic acid, was mixed with carbonate of potassa, and the precipitate thus obtained being dissolved in sulphuric acid, and mixed with acetate of potassa, afforded nothing but crystals of alum.

d. The other half of the precipitate *b* was digested in distilled vinegar; the solution saturated by carbonate of ammonia afforded a precipitate which, after due elutriation and ignition, weighed 59 grains, and had the proporties of pure *alumine.*

e. The liquid remaining, after the separation of the precipitate *b*, was evaporated to a small bulk, rendered neutral by muriatic acid, and mixed with lime-water: a precipitate was thus formed, having the properties of *fluate of lime*; acted upon by sulphuric acid, it evolved glass-corroding fumes.

The above experiments sufficiently demonstrate the existence of *fluoric acid* in the topaz, without accurately determining its relative proportion. Experience leads me to believe, that the loss in the earthy constituents does not exceed 1 per cent.; hence the deficiency of 5 per cent. may be referred to fluoric acid, and the components of the pale yellow Saxon topaz may be stated as follows:

Silica	*a*	35
Alumina	*d*	59
Fluoric acid	*e*	5
Oxide of iron	*c*	a trace.
Loss		1
		100

Analysis of Zoisit, from Carinthia.

The specific gravity of the crystals of this mineral, used in my analysis, was = 3.315.

a. 100 grains in fine powder were mixed with a solution of 250 grains of soda, and evaporated and fused in a silver crucible; the mass was softened with water, and dissolved in excess of muriatic acid, with which it formed a clear yellow solution; this was again evaporated to dryness, and the residue, treated by very dilute muriatic acid, left 45 grains of *silica.*

b. The liquid was mixed with caustic ammonia, and the precipitate separated by filtration; carbonate of soda was then added, by which 33.5 grains of carbonate of lime were thrown down equivalent to 18.5 grains of *lime.*

c. The precipitate thrown down by caustic ammonia was boiled in caustic potassa, which dissolved it, with the exception of some brown residuary matter. This residue, dissolved in nitric acid, and precipitated by ammonia, afforded 3 grains of oxide of iron; carbonate of ammonia, subsequently added to the washings, furnished an additional precipitate of carbonate of lime, equal to 2.5 of *lime.*

d. To obtain the ingredient dissolved by the caustic potassa, the solution was slightly supersaturated by sulphuric acid, and precipitated by carbonate of potassa; the precipitate was washed, and boiled in distilled vinegar, which being again saturated with ammonia, and the precipitate washed, dried and ignited, gave 29 grains of pure *alumina.*

Hence 100 parts of crystallized zoisit consist of

Silica	*a*		45
Alumina	*d*		29
Lime	*b*	18.50	21
——	*c*	2.50	
Oxide of iron	*c*		3
			98

Analysis of the foliated Augite of Carinthia.

A.

This mineral is difficultly fusible in small fragments; it runs before the blow-pipe into an opaque olive-coloured slag. Its powder is greenish grey; heated to redness, it becomes pale-brown, without loss of weight,

B.

a. 100 grains were mixed with a solution containing 200 of caustic soda, evaporated to dryness, and ignited. The resulting mass tinged the water which was affused of a pale-green; dissolved in muriatic acid, evaporated to dryness, and again treated with very dilute muriatic acid, *silica* remained, which after being heated red-hot, weighed 52.5 grains.

b. The muriatic solution was saturated by caustic ammonia, which caused a bulky brown precipitate; this being separated by filtration, a clear colourless solution was obtained, which, by the addition of carbonate of soda, yielded 16.5 grains of carbonate of lime, =9 grains of *lime.*

c. The precipitate by ammonia was transferred while yet moist, into a boiling solution of caustic potassa; a portion was dissolved, and the remainder collected upon a filter. Muriate of ammonia was added to the alcaline solution, which threw down 7.25 grains of *alumina.*

d. The remaining portion was dissolved in dilute nitromuriatic acid; to this solution carbonate of soda was added, to throw down oxide of iron, amounting to 16.25 grains.

e. The residuary liquid was boiled, and completely decomposed, with excess of carbonate of soda; carbonate of magnesia was thus thrown down, which yielded, on ignition, 12.5 grains of *magnesia.*

C.

100 grains of levigated augite were fused with 500 grains of nitrate of baryta; the resulting mass was powdered, mixed with water, neutralized by sulphuric acid, and the precipitate thus formed separated upon a filter. The clear liquid was precipitated by ammonia, the precipitate removed, evaporated to dryness, and the dry salt ignited; as this contained sulphate of magnesia, it was again dissolved and decomposed by acetate of baryta; the filtered liquid was evaporated, and the residue ignited; there remained a blackish matter, weighing 1.25 grains, which was washed, to separate the charcoal resulting from the combustion of the acetic acid; the washings afforded a portion of carbonate of potassa, equivalent to about half a grain of *potassa.*

100 parts, therefore, of this lamellar augite consist of

Silica	B *a*	52.50
Magnesia	— *e*	12.50
Lime	— *b*	9
Alumine	— *c*	7.25
Oxide of iron	— *d*	16.25
Potassa	C	0.50
		98

Analysis of Conchoidal Apatite, from Zillerthahl.

The specific gravity of this mineral is 3.190. After ignition it becomes colourless, without losing its transparency, or decrepitating, neither does it lose perceptibly in weight. Sprinkled upon hot coals, it exhibits no phosphorescence.

Having, by previous trials, ascertained the presence of phosphoric acid and lime, its decomposition was effected as follows :

a. 100 grains in powder were digested in muriatic acid, which soon formed a solution, a few particles of talc only remaining, and weighing 0.75 grain. This loss on the hundred parts was made good by the addition of a similar weight of the pure mineral.

b. The muriatic solution was nearly saturated with caustic ammonia, solution of oxalic acid was then added as long as it formed a precipitate of oxalate of lime, which was collected, washed, dried, and strongly heated in a platinum crucible ; it was then dissolved in muriatic acid, and thrown down by carbonate of soda, in the state of carbonate of lime, which weighed, when washed and dried, 91 grains, =50 grains of pure *lime.*

c. To separate the phosphoric acid from the liquid, from which the lime had been abstracted by oxalic acid ; it was evaporated to dryness, and the saline residue gradually ignited in a platinum crucible, during which operation muriate of ammonia passed off in white fumes. The remainder, when cold, appeared as a vitreous mass, blackened by a portion of charcoal derived from the decomposition of the oxalic acid. It weighed 47.5 grains. It was dissolved in boiling water, by which 1 grain of finely-divided charcoal was separated.

d. The acid liquid was neutralized with ammonia, which produced some turbidness, and a precipitate, weighing 7 grains, and composed of phosphate of lime, was deposited; it fused before the blow-pipe into a porcellaneous bead.

Deducting these 7 grains, and the 1 grain of coal, the weight of the phosphoric acid is 39.5. If we consider the 7 grains of undecomposed phosphate as composed of 3.75 of lime, and 3.25 of phosphoric acid, the components of the apatite obtained in this analysis are,

Lime	*b*	50	53.75
	d	3.75	
Phosphoric acid	*d*	39.50	42.75
		3.25	
			96.50

But a small portion of phosphoric acid was probably volatilized by heat, so that the loss, amounting to 3.5 grains, may be attributed to that source, and the components of this mineral will then stand thus:

Lime	53.75
Phosphoric acid	46.25
	100

A trace of iron and of manganese was also discovered in this apatite, but in quantity too small to be appreciated by weight.

The analysis of the Saxon apatite agrees with the above results: it consists of

Lime	55
Phosphoric acid	45*

Vauquelin's analysis of the chrysolite, or asparagus stone of Spain, also afforded nearly the same proportions, namely,

Lime	54.28
Phosphoric acid	46.72†

Analysis of the Columnar Brown-spar of Mexico.

This mineral is among the treasures imported from America by M. Von Humboldt.

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* *Berg. Journ.* 1788. 1 Band. S. 296.

† *Jour. des Mines,* xxxvii. p. 26.

A.

100 parts were heated in a covered crucible; it decrepitated into a coarse powder, losing 5 per cent. of weight, but remaining white. The heat was increased to redness, when it lost 11 per cent., and became of a blackish-grey colour.

B

Digested in cold and moderately strong nitric acid, it dissolved slowly, and with sparing effervescence, forming a transparent straw-coloured solution. If the acid be heated, it effervesces strongly forming a reddish-brown solution, and the undissolved portion of the fossil appears orange-yellow; when perfectly dissolved, the colour of the solution is pale reddish brown.

C.

100 grains of the coarsely-powdered mineral were dissolved in nitric acid, and the solution neutralized by caustic ammonia. A brown flocculent precipitate fell, weighing, when dry, 14 grains; this was re-dissolved in nitro-muriatic acid, the excess of acid was then neutralized by soda, and the iron separated by succinate of ammonia. The succinate of iron yielded, after ignition, 5.5 grains of magnetic oxide of iron. But, since the iron exists in this mineral in the state of carbonate, the above result must be assumed to indicate 7.5 grains of *carbonate of iron.* The liquid from which the iron had been thrown down was heated to its boiling point, and mixed with carbonate of soda, which gave a white precipitate, becoming black when heated, and weighing, after ignition, 4 grains. Digested in nitric acid, the greater portion of this precipitate was dissolved, but there remained a black residue, consisting of oxide of manganese, and weighing 1.25 grains, which I consider as equal to 2 grains of *carbonate of manganese.* Carbonate of soda threw down from the remaining nitric solution 3.5 grains of *carbonate of magnesia*

b. The excess of ammonia, in the original solution *a,* was supersaturated by nitric acid, and carbonate of soda added to the cold solution as long as it occasioned a precipitate, which, collected, edulcorated, and dried, weighed 49 grains, and consisted of *carbonate of lime.*

c. When the residuary liquor was boiled, it became turbid, and a white precipitate fell, the separation of which was completed by the addition of a little soda. Washed and dried, it weighed 31 grains. Dissolved in sulphuric acid and evaporated, it furnished crystals of sulphate of magnesia, which being re-dissolved, left 4 grains of sulphate of lime, =2.5 grains of *carbonate of lime*; this being deducted, there remained 28.5 grains of *carbonate of magnesia.*

100 parts, therefore, of this mineral consist of,

Carbonate of lime	C	*b*	49	51.50
		c	2.50	
Carbonate of magnesia		*a*	3.50	32
		c	28.50	
Carbonate of iron		*a*		7.50
Carbonate of manganese		*a*		2
Water	A			5
				98

Analysis of Dolomite.

[In these analyses, Klaproth dissolved the mineral in dilute nitric acid, and saturated with caustic ammonia, which threw down oxide of iron, when present; he then added carbonate of ammonia, by which carbonate of lime was precipitated; and lastly the magnesia was separated, either by boiling with carbonate of potassa; or by oxalate of potassa. The following are the results.]

	Compact Dolomite from St. Gothard.	Decomposing Dolomite from the Apennines.	Compact Dolomite from ditto.	Antique Dolomite from Tenedos.
Carbonate of Lime	52	59	65	51.50
Carbonate of Magnesia	46.50	40.50	35	48.
Oxide of Iron and of Manganese	0.75			
Loss	0.75	0.50		0.50

Analysis of Anhydrite.

A.

A fragment of blue anhydrite (from Sulz on the Neckar) weighing 300 grains, was kept red hot in a platinum crucible for half an hour; it became soft and yellowish-white, but lost no weight.

B.

Thirty grains of the powdered mineral were boiled in 12 ounces of water; 8 grains were dissolved, but the solution did not render nitrate of silver in the least turbid, so that it contained no muriatic acid.

C.

a. 200 grains of pulverized anhydrite were boiled with 400 grains of carbonate of potassa in 8 ounces of water for a quarter of an hour. The alcaline liquor was then filtered off, neutralized by muriatic acid, and mixed with muriate of baryta as long as a precipitate was produced. The dry sulphate of baryta weighed 345 grains = 114 grains of dry *sulphuric acid.*

b. The residue upon the filtre dissolved with effervescence in dilute nitric acid, excepting 0.5 grain of *silica*; supersaturated with caustic ammonia the solution gave no precipitate; it was therefore again rendered neutral by nitric acid, and tested with prussiate of potassa, and after a while it deposited a small portion of prussian blue indicating not more than 0.20 grain of *oxide of iron.*

c. This being separated the solution was saturated whilst boiling hot, with carbonate of potassa; carbonate of lime was thus thrown down, weighing when washed and dried in a gentle heat, 153 grains = 84 grains of *lime.* This carbonate was then neutralized with sulphuric acid, evaporated to dryness, and ignited; it thus afforded 198 grains of regenerated sulphate of lime.

100 parts therefore of this blue anhydrite were resolved into

Lime	42.
Sulphuric acid	57.
Oxide of iron	0.10
Silica (probably adventitious) . . .	0.25
	99.35

[Another specimen of anhydrite from Hall in the Tyrol, contained

Lime	41.75
Sulphuric acid	55.
Muriate of soda	1.
	97.75

The source of loss in this analysis is not adverted to. The composition of sulphate of lime, deduced fron the mean of Klaproth's *Analyses of the Anhydrite*, agrees exactly with the equivalents upon Dr. Wollaston's scale.]

Analysis of Bitter Spar from Hall in the Tyrol.

This spar accompanies the anhydrite described in the last paragraph. Its component parts are

Carbonate of lime	68.
Carbonate of magnesia	25.50
Carbonate of iron	1.
Water	2.
(Adhering clay)	2.
	98.50

Analysis of the Green Earth of Verona.

This substance, found at Monte Baldo in the Veronese, is much esteemed as a green pigment. After having been heated red hot it acquires a brown colour, and additional hardness, losing 6 per cent. of its weight. It is not soluble in acids, nor do they alter its colour.

A.

a. 100 grains were mixed with 200 of caustic potassa dissolved in water, and the mixture evaporated and ignited. In this case the colour remained unchanged till the mass became red hot, when it grew yellowish brown : it was dissolved in muriatic acid ; the solution was evaporated to dryness, and the residue being again digested in very dilute muriatic acid, 53 grains of *silica* were obtained.

b. Carbonate of soda was then added to the remaining cold liquid so as to saturate it, which threw down *oxide of iron*, weighing when washed and ignited, 28 grains.

c. After the separation of oxide of iron the residuary liquid was made boiling hot, and carbonate of soda added in excess, which threw down *magnesia* weighing after ignition 2 grains.

B.

One hundred grains were fused with 500 of nitrate of baryta, and the residuary mass supersaturated with dilute sulphuric acid. The precipitate being separated, the clear liquor was mixed with excess of carbonate of ammonia, filtered, evaporated to dryness, and the residuum ignited in a platinum crucible. After the escape of the sulphate of ammonia, there remained 19 grains of sulphate of potassa, equal to 10 grains of *potassa*.

The components therefore of this mineral are as follow:

Silica	53
Oxide of iron	28
Magnesia	2
Potassa	10
Water	6
	99

Analysis of the Alum Stone of Tolfa.

As this mineral affords alum by lixiviation, sulphuric acid, alumina, and potassa, were to be looked for among its components.

A.

One hundred grains were strongly heated in a coated glass retort; sulphurous acid, water, and sulphuric acid passed over, but there were no traces of uncombined sulphur. The loss of weight amounted to 29 grains.

B.

Two hundred grains were gently heated so as to drive off *water* only: the loss amounted to 6 grains.

C.

a. 200 grains in fine powder were fused with twice their weight of dry carbonate of soda; the cold mass was powdered, dissolved in dilute muriatic acid, evaporated to dryness, redissolved in water, and filtered; there remained upon the filter 113 grains of *silica.*

b. The muriatic solution was divided into two equal portions, one of which was mixed with solution of muriate of baryta; 50 grains of sulphate of baryta = 16.5 dry *sulphuric acid* were obtained.

c. The other half of the solution was precipitated by ammonia. The precipitate consisted of *alumina,* which when purified, edulcorated and ignited weighed 19 grains.

D.

One hundred grains were fused with 300 of nitrate of baryta; the resulting mass was softened with water, mixed with excess of sulphuric acid, evaporated nearly to dryness, and then boiled in water and filtered; the clear liquid was neutralized by ammonia, and after the separation of the precipitate was evaporated to dryness and ignited, there remained 7 grains of sulphate of potassa = 4 of pure *potassa.*

The components therefore are

Silica	C *a*	56.50
Alumina	c	19.
Sulphuric acid	*b*	16.50
Potassa	D	4.
Water	B	3.
		99.

Analysis of the Alum Slate of Freienwald.

A.

a. 1000 grains were boiled for half an hour with 20 ounces of distilled water; then filtered and the residue edulcorated. The filtered liquid was colourless, did not change vegetable blues, but had a slightly vitriolic taste.

b. Half of this solution was mixed with muriate of baryta by which 23 grains of sulphate of baryta were separated; and afterwards, prussiate of ammonia threw down 40 grains of prussiate of iron.

c. The other half was mixed with oxalate of potassa; it became pale yellow and moderately turbid, the colour probably resulting from a little oxalate of iron: the mixture became slowly clear, and the precipitate being collected weighed after ignition 2.5 grains, and had the properties of lime slightly contaminated by oxide of iron.

The matter therefore, soluble in water, consists of sulphate of lime and sulphate of iron, the relative proportions of which may be estimated as follows: 1000 parts of the ore gave 46 of sulphate of baryta = 15.18 dry sulphuric acid, of which 7 parts saturated the 5 of lime, producing 15 parts of crystallized gypsum The remaining 8.18 of acid combined with 8.5 of oxide of iron would produce 18 parts of effloresced sulphate of iron.

B.

Two hundred grains of the ore were boiled in water with 400 of dry carbonate of soda; the filtered liquid was of a dark brown colour; neutralized with muriatic acid, it afforded no trace of sulphuretted hydrogen: this neutral solution gradually deposited a bulky dark brown matter which, collected and dried, weighed 12 grains, and which, heated in a platinum crucible, burned away without any odour of sulphur, and left 1 grain of white alumina.

C.

Two hundred grains were digested in muriatic acid, during which not the smallest trace of sulphuretted hydrogen was perceived, nor was there much action; but during the gradual addition of nitric acid, nitrous gas was evolved and the black colour of the ore became brown; the filtered solution was yellow and mixed with muriate of baryta furnished 54 grains of sulphate of baryta (after ignition).

D.

a. 1000 grains of the ore recently from the mine, were heated in a retort, the recipient being connected with the pneumatic apparatus. 220 cubic inches of gas were evolved, having the properties of a mixture of sulphuretted and carburetted hydrogen.

b. The liquid produced weighed 133 grains; it was yellow and smelt of hydro-sulphuret of ammonia; it changed reddened litmus paper to blue, and produced fumes when brought to a glass rod dipped in muriatic acid; it occasioned a brown precipitate in solution of lead; became milky when neutralized by muriatic acid; filtered and evaporated, it left 2 grains of muriate of ammonia.

c. The black residue in the retort weighed 750 grains; its charcoal was burned off and there remained 660 grains of a reddish-brown matter, so that the *carbonaceous loss* was 90 grains.

d. 132 grains of this brown residuum, (one fifth,) mixed with twice its weight of caustic soda and fused, furnished a greenish-brown mass which, digested in water gave a pale green solution; supersaturated by muriatic acid, evaporated, and again digested with water acidulated by the same acid, *silica* remained, weighing, after ignition, 80 grains.

e. The remaining liquid was saturated with carbonate of potassa, and the washed precipitate thus obtained boiled in liquid caustic potassa to abstract the *alumina*, which, thrown down by muriate of ammonia, washed and heated, weighed 32 grains.

f. The brown residue, insoluble in the caustic alcaline solution, was dissolved in dilute sulphuric acid, and evaporated to dryness; during the evaporation *sulphate of lime* separated, which was carefully collected; it weighed rather more than 2 grains: the dry mass was ignited and washed, and the *oxide of iron*, which then remained, being collected and mixed with oil and ignited became magnetic, and weighed 14.5 grains. The washings saturated, when boiling hot, with carbonate of potassa, yielded a slight trace of carbonate of magnesia.

E.

a. 200 grains of the ore were slowly distilled in a sand-heat, so that no gas was evolved; 21.5 grains of water very slightly sulphurous, passed into the receiver, and an extremely thin coating of sulphur was remarked in the neck of the retort.

b. The dried ore was taken out of the retort and heated upon a tile; it exhaled a slight smell of sulphur, without either smoke or flame. The loss during the combustion amounted to 45 grains which may be estimated as charcoal and sulphur with perhaps a little water.

c. The residuum was mixed with 200 grains of sulphuric acid and 400 of water, evaporated, and strongly ignited for half an hour: it was then washed in water, and the filtered washings were saturated with ammonia which threw down half a grain of magnesia.

d. The liquid was then evaporated to dryness, and the saline mass heated till it no longer evolved fumes; the dry salt weighed 4.5 grains, and had the properties of a mixture of sulphate and muriate of potassa, the sulphate being to the muriate as 2 to 1. [It is here not very evident how the muriate of potassa escaped the action of the sulphuric acid, during the ignition in *c.*]

F.

It appears from these experiments,

1. That there is no bitumen, but only charcoal in this alum ore, since neither bituminous oil, nor smoke, nor flame, are produced during its distillation and combustion.

2. That the sulphur is not combined with iron in the state of pyrites, but in some peculiar combination with the carbonaceous matter; no pyrites is visible in the ore, and alcalis do not extract it, nor does muriatic acid evolve sulphuretted hydrogen.

G.

The estimation of the relative weights of the constituents of this ore is therefore attended with difficulty; the ingredients that distinguish it as an alum ore, are alumina and sulphur. Direct experiment gives the proportion of the former =160 in 1000. The proportion of sulphur may be deduced from the result of process C, which affords 270 grains of sulphate of baryta from one thousand of the ore. But of these 270 grains, 46 may be ascribed to the vitriol and gypsum, and 20 to the sulphate of potassa (supposing 15 parts of that salt to be contained in 1000 of the ore). We must therefore subtract 66 from 270, and there remain 204 parts of sulphate of baryta, as the equivalent of the sulphur of the ore. These 204 parts are equal to 90.75 of sulphuric acid, (specific gravity 1.850) or =28.5 of sulphur, which being deducted from the joint amount of the sulphur and charcoal, amounting to 225 grains (E*b*) leave 196.5 grains for the weight of the charcoal.

H.

Assuming that 1000 parts of crystallized alum yield upon average, when decomposed by muriate of baryta, 945 grains of sulphate of baryta, it follows that 1000 parts of the alum ore of Freienwald, containing 28.5 of sulphur, should produce sulphuric acid sufficient (with the due addition of potassa) to form 260 parts of alum, for which scarcely one-sixth part of the alumine in the ore is required. But the quantity of alum produced at the works, falls infinitely short of this estimate in consequence of the very imperfect acidification of the sulphur.

I.

The following view of the components of this alum ore is suggested by the above experiments.

Sulphur	G		28.50
Carbon	G		196.50
Alumina	D *e*		160.
Silica	D *d*		400.
Black oxide of iron (with a very slight trace of manganese	D *f*	72.5	64.
Of which deduct for the composition of sulphate of iron		8.5	
Sulphate of iron	A *c*		18.
Sulphate of lime	A *c*		15.
Magnesia	E *e*		2.50
Sulphate of potassa	E *d*		15.
Muriate of potassa	E *d*		5.
Water	E *a*		107.50
			1012.

Some of the above results, however, require further investigation, and the magnesia is probably in the state of sulphate in the ore. The excess of 1.5 per cent. in the analysis is insignificant in such experiments.

Art. VIII. *Description of an Electro-magnetical Apparatus for the Exhibition of Rotatory Motion.* *By* M. Faraday, *Chem. Assist. in the Royal Institution.*

The account given in the *Miscellanea* of the last Journal, of the apparatus invented in illustration of the paper in the body of that Number, being short and imperfect; a plate is given in the present Number, presenting a section of that apparatus, and a view of a smaller apparatus, illustrative of the motions of the wire and the pole round each other. The larger apparatus is delineated, Fig. 1. Plate VII. on a scale of one-half. It consists of two glass vessels, placed side by side with their appendages. In that on the left of the plate the motion of a magnetic pole round the connecting wire of the voltaic battery is produced. That a current of voltaic electricity may be established through this cup, a hole is drilled at the bottom, and into this a copper pin is ground tight, which projects upwards a little way into the cup, and below is rivetted to a small round plate of copper, forming part of the foot of the vessel. A similar plate of copper is fixed to the turned wooden base on which the cup is intended to stand, and a piece of strong copper wire, which is attached to it beneath, after proceeding downwards a little way, turns horizontally to the left hand, and forms one of the connexions. The surfaces of these two plates intended to come together, are tinned and amalgamated, that they may remain longer clean and bright, and afford better contact. A small cylindrical and powerful magnet has one of its poles fastened to a piece of thread, which, at the other end, is attached to the copper pin at the bottom of the cup; and the height of the magnet and length of the thread is so adjusted, that when the cup is nearly filled with clean mercury, the free pole shall float almost upright on its surface.

A small brass pillar rises from the stand behind the glass-vessels: an arm comes forward from the top of it, supporting at its extremity a cross wire, which at the place on the left hand, where it is perpendicularly over the cup just described, bends downwards, and is continued till it just dips into the centre of the mercurial surface. The wire is diminished in size for a short distance above the surface of the mercury, and its lower extremity amalgamated, for the purpose of ensuring good contact; and so also is the copper pin at the bottom of the cup. When the poles of a voltaic apparatus are connected with the brass pillar, and with the lateral copper wire, the upper pole of the magnet immediately rotates round the wire which dips into the mercury; and in one direction or the other, according as the connexions are made.

The other vessel is of the form delineated in the plate. The stem is hollow and tubular; but, instead of being filled by a plug, as is the aperture in the other vessel, a small copper socket is placed in it, and retained there by being fastened to a circular plate below, which is cemented to the glass foot, so that no mercury shall pass out by it. This plate is tinned and amalgamated on its lower surface, and stands on another plate and wire, just as in the former instance. A small circular bar magnet is placed in the socket, at any convenient height, and then mercury poured in until it rises so high that nothing but the projecting pole of the magnet is left above its surface at the centre. The forms and relative positions of the magnet, socket, plate, &c. are seen in fig. 2.

The cross wire supported by the brass pillar is also prolonged on the right hand, until over the centre of the vessel just described; it then turns downwards, and descends about half an inch: it has its lower extremity hollowed out into a cup, the inner surface of which is well amalgamated. A smaller piece of copper wire has a spherical head fixed on to it, of such a size that it may play in the cup in the manner of a ball and socket-joint, and being well amalgamated, it, when in the cup, retains sufficient fluid mercury by capillary attraction to form an excellent contact with freedom of motion. The ball is prevented from falling out of the socket by a piece of fine thread, which, being fastened to it at the top, passes through a small hole at the summit of the cup, and is made fast on the outside of the thick wire. This is more minutely explained by Fig. 3, and 4. The small wire is of such a length that it may dip a little way into the mercury, and its lower end is amalgamated. When the connexions are so made with the pillar and right hand wire, that the current of electricity shall pass through this moveable wire, it immediately revolves round the pole of the magnet, in a direction dependant on the pole used, and the manner in which the connexions are made.

Fig. 5, is the delineation of a small apparatus, the wire in which revolves rapidly, with very little voltaic power. It consists of a piece of glass tube, the bottom part of which is closed by a cork, through which a small piece of soft iron wire passes, so as to project above and below the cork. A little mercury is then poured in, to form a channel between the iron wire and the glass tube. The upper orifice is also closed by a cork, through which a piece of platinum wire passes, which is terminated within by a loop; another piece of wire hangs from this by a loop, and its lower end, which dips a very little way into the mercury, being amalgamated, it is preserved from adhering either to the iron wire or the glass. When a very minute voltaic combination is connected with the upper and lower ends of this apparatus, and the pole of a magnet is placed in contact with the external end of the iron wire, the moveable wire within rapidly rotates round the magnet thus formed at the moment; and by changing either the connexion, or the pole of the magnet in contact with the iron, the direction of the motion itself is changed.

The small apparatus in the plate is not drawn to any scale. It has been made so small as to produce rapid revolutions, by the action of two plates of zinc and copper, containing not more than a square inch of surface each.

In place of the ball and socket-joint, (fig. 3, and 4,) loops may be used; or the fixed wire may terminate in a small cup containing mercury, with its aperture upwards, and the moveable wire may be bent into the form of a hook, of which the extremity should be sharpened, and rest in the mercury on the bottom of the cup.

ART. IX. *On the Atomic Weight of Silver, and on the Constitution of Liquid Muriatic Acid at different Densities; by* ANDREW URE, M.D., F.R.S., *Professor of the Andersonian Institution at Glasgow,* M. G. S., *&c.*

[This paper forms the introduction to an elaborate Memoir on the nature and manufacture of Chloride (Oxymuriate) of Lime, and on the atomic weights of Silver and Manganese, with which we have been favoured by Dr. Ure, and which will appear in our next Number.]

UPWARDS of four years ago, I published an account of a set of experiments, which I had made on the saturating power of liquid muriatic acid of 1.102, with a table of its progressive densities, and corresponding acid strengths. This table was favourably received by the chemical world, and has since been adopted into most of our respectable treatises on Chemistry. Having had occasion last spring to subject muriatic acid in different states of dilution, to a very rigorous examination, I perceived small deviations in the new results, from my former tabular quantities, which induced me to revise the whole with the greatest possible care *. My first business, however, was to settle the prime equivalent or atomic weight of silver. For this purpose I revived from the washed chloride of this metal a portion of silver which I re-dissolved in pure nitric acid, filtered the solution, re-produced the chloride, and once more revived the silver by ignition with pure carbonate of potash. The metal being well boiled in distilled water, washed, and dried, I regarded as perfectly pure, and dissolved a known weight of it in a sufficient quantity of nitric acid. The solution was then diluted with water, so that the silver formed one-twentieth of the whole. I next dissolved, in some of the same double distilled water, 75 grains of pure sal gem, (native muriate of soda, which stood the tests of muriate of barytes and oxalate, and phosphate of ammonia,) and mixed with that solution one of the nitrate of silver, containing 137.5 grains of metal. Gentle

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* Subsequent researches have not enabled me to make any improvement on my tables of Sulphuric and Nitric Acids. I believe them to be very near the truth.

agitation was employed during the mixture. In 24 hours the supernatant liquid, having become quite limpid, was tested, and found to contain redundant muriate. Small quantities of a very dilute solution of nitrate of silver were progressively added with much agitation, till the clear supernatant liquid was found free from every vestige of chlorine. Nor did muriate of soda shew in it any trace of silver. The total quantity of silver expended to produce this effect was 138.75 grains. This experiment was repeated on different quantities, and with a variation in the mode of mixture, but still the same proportion obtained in the result. From the quantity of salt and of silver in their respective solutions, and from the delicacy of my balance, I could add either ingredient to the mixture within $\frac{1}{1000}$ of a grain. In three experiments, however, in which I used sea salt, recrystallized with care in regular cubes, I obtained anomalous results, in consequence of minute portions of adhering muriates of potash or lime. From such salt the prime equivalent of silver appeared to be at different times 13.73, 13.75, and 13.7. It is the tendency of the slightest admixture of these salts, or of sulphate of soda, to lower the apparent atomic weight of silver. If therefore the prime equivalent of chloride of sodium be called 4.5 chlorine $+3$ sodium $=7.5$, then we must infer from the above experiments that the prime equivalent of silver is 13.875; or, taking the proportional weight of hydrogen in water as the radix of the equivalent scale, chloride of sodium will become 60, and silver 111: or, in volume on Sir H. Davy's plan, 120 and 222.

The same solution of silver was used for determining the quantity of chlorine in liquid muriatic acid, having found by experience that its saturation by carbonate of potash, soda, or lime, and evaporation of the chlorides, did not afford results of the minute and consistent accuracy, required by the theory of equivalents. Liquid acid having the specific gravity of 1.200, was diluted with 9 times its weight of water, when its density at 60° Fahr. became 1.020. On this dilute acid I made many experiments with the nitrate of silver, and satisfied myself at last that 1000 grains of it were equivalent to $122\frac{1}{3}$ grains of silver, which, from the above-recited experiments, represent 39.675 of chlorine; the quantity, therefore, present in 100 grs. of acid, at the density of 1.200. If to 39.675 we add one prime equivalent of hydrogen $= \frac{39.675}{36} = 1.1021$, the sum 40.7771 will represent the corresponding quantity of muriatic acid gas. Hence, for ordinary practice, if we estimate in round numbers, the value of liquid acid, specific gravity 1.20, at 40 *per cent.* of chlorine and 41 of muriatic gas, we shall commit no error of consequence.

The correspondence between the above experimental results, and those given by Sir H. Davy in his valuable *Elements of Chemistry*, is almost exact, though the two methods are sufficiently distinct. Sir H. and Mr. E. Davy found that 47.25 grains of water at 43° Fahr. barom., 30.2 absorbed 34.8 grains of gas, and formed a solution of specific gravity 1.21; and the whole precipitated by nitrate of silver, afforded about 132 grs. of dry horn silver. Again 57.5 grains of water, at 44°, barom. being 30.1, gained nearly 38 grains by absorbing acid gas, and formed a solution of specific gravity 1.2*. The only other point which they determined by experiment was for the specific gravity 1.114. Let us compare a little their direct experiment of condensing the gas in water (whose result agrees very nearly with mine,) with their result from chloride of silver. If 82.05 grains of acid of 1.21 resulted from the combination of 47.25 water +34.8 acid gas, then 100 of such acid, at 43°, should contain just 42.41 parts, as given in their table. But if we calculate the acid gas present in the same 82.05 grains of liquid acid, from the weight of horn silver which they obtained, we shall have a considerably different result. Naming the equivalent of chloride of silver, 18.375 (4.5+13.875) we shall have this proportion; 18.375 : 4.5 : : 132 : 32.7 of chlorine, corresponding to 33.225 of acid gas, instead of 34.8, as by their direct experiment. If, with Dr. Thomson, we call chloride of silver 18.25 (13.75+4.5), then we have the proportion 18.25 : 4.5 : : 132 : 32.548 of chlorine, or 33.452 of muriatic gas. Finally, by Dr. Wollaston's scale, 132 grains of muriate

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* *Elements*, p. 252, Note.

of silver are equivalent to 33.5 of the acid gas. The first number makes the quantity of muriatic gas, in 100 parts of acid of 1.21, to be only 40.5; the second, 40.77; the third, 40.83, while the number in their table is 42.43. In like manner, when I attempted to get very nice results by weighing the chloride of silver, I found a perceptible and fluctuating deficiency.

From the excellent accordance which exists between Sir H. and Mr. E. Davy's experiments on the direct combination of the acid gas and water, and my result drawn from the silver equivalent, I shall consider that 100 grains of acid, specific gravity 1.20, contain 39.675 of chlorine. *That* acid was mixed with distilled water in successive proportions, and the mixtures being placed in well-stopped phials, and agitated, were left to cool to 60° Fahr. Their specific gravities were then taken with every requisite care.

The following table is the result of these experiments. The densities marked with an asterisk are experimental, the rest are interpolated; but the latter are, I believe, as well as the former, entitled to the entire confidence of practical chemists.

Table of Muriatic Acid.

Acid of 1.20 in 100.	Specific Gravity.	Chlorine.	Muriatic Gas.	Acid of 1.20 in 100.	Specific Gravity.	Chlorine.	Muriatic Gas.
100*	1.2000	39.675	40.777	81	1.1641	32.136	33.029
99	1.1982	39.278	40.369	80*	1.1620	31.746	32.621
98	1.1964	38.882	39.961	79	1.1599	31.343	32.213
97	1.1946	38.485	39.554	78	1.1578	30.946	31.805
96	1.1928	38.089	39.146	77	1.1557	30.550	31.398
95*	1.1910	37.692	38.738	76	1.1536	30.153	30.990
94	1.1893	37.296	38.330	75	1.1515	29.757	30.582
93	1.1875	36.900	37.923	74	1.1494	29.361	30.174
92	1.1857	36.503	37.516	73	1.1473	28.964	29.767
91	1.1846	36.107	37.108	72	1.1452	28.567	29.359
90*	1.1822	35.707	36.700	71	1.1431	28.171	28.951
89	1.1802	35.310	36.292	70*	1.1410	27.772	28.544
88	1.1782	34.913	35.884	69	1.1389	27.376	28.136
87	1.1762	34.517	35.476	68	1.1369	26.979	27.728
86	1.1741	34.121	35.068	67	1.1349	26.583	27.321
85	1.1721	33.724	34.660	66	1.1328	26.186	26.913
84	1.1701	33.328	34.252	65	1.1308	25.789	26.505
83	1.1681	32.931	33.845	64	1.1287	25.392	26.098
82	1.1661	32.535	33.437	63	1.1267	24.996	25.690

Acid of 1.20 in 100.	Specific Gravity.	Chlorine.	Muriatic Gas.	Acid of 1.20 in 100.	Specific Gravity.	Chlorine.	Muriatic Gas.
62	1.1247	24.599	25.282	31	1.0617	12.300	12.641
61	1.1226	24.202	24.874	30*	1.0597	11.903	12.233
60*	1.1206	23.805	24.466	29	1.0577	11.506	11.825
59	1.1185	23.408	24.058	28	1.0557	11.109	11.418
58	1.1164	23.012	23.650	27	1.0537	10.712	11.010
57	1.1143	22.615	23.242	26	1.0517	10.316	10.602
56	1.1123	22.218	22.834	25	1.0497	9.919	10.194
55	1.1102	21.822	22.426	24	1.0477	9.522	9.786
54	1.1082	21.425	22.019	23	1.0457	9.126	9.379
53	1.1061	21.028	21.611	22	1.0437	8.729	8.971
52	1.1041	20.632	21.203	21	1.0417	8.332	8.563
51	1.1020	20.235	20.796	20*	1.0397	7.935	8.155
50*	1.1000	19.837	20.388	19	1.0377	7.538	7.747
49	1.0980	19.440	19.980	18	1.0357	7.141	7.340
48	1.0960	19.044	19.572	17	1.0337	6.745	6.932
47	1.0939	18.647	19.165	16	1.0318	6.348	6.524
46	1.0919	18.250	18.757	15	1.0298	5.951	6.116
45	1.0899	17.854	18.349	14	1.0279	5.554	5.709
44	1.0879	17.457	17.941	13	1.0259	5.158	5.301
43	1.0859	17.060	17.534	12	1.0239	4.762	4.893
42	1.0838	16.664	17.126	11	1.0220	4.365	4.486
41	1.0818	16.267	16.718	10*	1.0200	3.968	4.078
40*	1.0798	15.870	16.310	9	1.0180	3.571	3.670
39	1.0778	15.474	15.902	8	1.0160	3.174	3.262
38	1.0758	15.077	15.494	7	1.0140	2.778	2.854
37	1.0738	14.680	15.087	6	1.0120	2.381	2.447
36	1.0718	14.284	14.679	5	1.0100	1.984	2.039
35	1.0697	13.887	14.271	4	1.0080	1.588	1.631
34	1.0677	13.490	13.863	3	1.0060	1.191	1.224
33	1.0657	13.094	13.456	2	1.0040	0.795	0.816
32	1.0637	12.697	13.049	1	1.0020	0.397	0.408

At the density 1.199, Mr. Dalton's table* has 25.6 *per cent.* of real muriatic acid by weight, equivalent to only 32.9 chlorine, instead of 39.47, which I believe to be the exact value. If we term the correct quantity 100, then Mr. Dalton's number would be only 83; which is no less than 17 *per cent.* of defect from the truth. I have purposely omitted in this new table the column of dry, or, as it was also called, real muriatic acid; first, because there is no evidence at present of the existence of any such body;

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* *New System of Chemical Philosophy*, ii. p. 295.

and secondly, because, though it was a convenient column for finding by inspection the increase of weight which any salifiable base would acquire by saturation with the liquid acid, yet that convenience may be obtained by the following simple calculation. Since the prime equivalent of chlorine is to that of the supposed dry muriatic acid, in the ratio of 45 to 35, or 9 to 7; if we multiply the number opposite to the given specific gravity, in the chlorine column, by 9, and divide by 7, we shall have the relative quantity of the *fixable* muriatic acid.

Heretofore, several chemists have, without due consideration, assumed the half sum, or arithmetical mean of the specific gravities of two substances (water and acid, for example,) to be truly the *calculated* mean; and on comparing the number thus obtained with the *experimental* specific gravity of the compound, they have inferred the change of volume due to chemical combination. It is a singular fact, that the above arithmetical mean, which is always greater than the rightly computed mean, (see my paper on Mean Specific Gravity, in this *Journal*, vol. iv. p. 151,) happens to give in the case of liquid muriatic acid, an error in excess, very nearly equal to the actual increase of density. From this curious coincidence we derive a very simple rule for finding the approximate value of chlorine in the liquid acid at any density. Multiply the decimal part of the number representing the specific gravity by 200, the product will be the chlorine present in 100 parts. Thus, the specific gravity is 1.0437, what is the quantity *per cent.* of chlorine? $0.0437 \times 200 = 8.74$. Now the tabular number is 8.729. The sp. gravity being 1.059, what is the value of the chlorine in 100 parts? $0.059 \times 200 = 11.8$. The table has 11.9. Towards the head of the table this rule gives a slight error in excess; and towards the foot an equally slight error in defect, but the approximation is always good enough for ordinary practice, seldom amounting to one-half *per cent.* If to the number thus found for chorine we add $\frac{1}{36}$ part, the sum is the corresponding weight of muriatic acid gas.

ART. X. *On an easy and secure Method of Secret Correspondence.*

To the Editor of the QUARTERLY JOURNAL.

London, Dec. 11*th,* 1821.

SIR,

THE ingenious paper on secret writing, in your last Number, by Mr. Hincks, shews how little reliance can be placed on the methods suggested by Mr. Chenevix; and the dot-writing, as described in the elaborate article on Cipher, in Rees' *Cyclopædia,* is not only equally insecure, but would be liable to great perplexity if separate keys were furnished to several correspondents. Ministers and Generals, however, are often obliged to communicate in cipher with many agents at the same time, and perhaps with more than one person even in the same mission.

The mode to which I am about to advert was given to me many years ago by my father; but whether it has ever been published I do not know. Its pretensions are simplicity, impossibility of detection, and the capability of multiplying its keys to infinity.

Let the key for each of the correspondents be a line of poetry, or the name of some memorable person or place, that cannot be forgotten; and let them all be provided with a copy of the following table; which may be printed, as it matters not into whose hands it falls.

When the despatch is composed, write the key-sentence underneath the text, letter for letter, repeating it as often as it may be necessary. For example, suppose the key-sentence to be " Sir Humphry Davy," and the secret clause of the despatch to be " Protract the negociation as much as possible." These, when written as above directed, will stand thus:

Protract the negociation as much as possible
Sirhumph ryd avysirhumph ry davy sir humphry

a	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*	*q*	*r*	*s*	*t*	*u*	*v*	*w*	*x*	*y*	*z*
b	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*	*q*	*r*	*s*	*t*	*u*	*v*	*w*	*x*	*y*	*z*	*a*
c	*d*	*e*	*f*	*g*	*h*	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*	*q*	*r*	*s*	*t*	*u*	*v*	*w*	*x*	*y*	*z*	*a*	*b*
d	*e*	*f*	*g*	*h*	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*	*q*	*r*	*s*	*t*	*u*	*v*	*w*	*x*	*y*	*z*	*a*	*b*	*c*
e	*f*	*g*	*h*	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*	*q*	*r*	*s*	*t*	*u*	*v*	*w*	*x*	*y*	*z*	*a*	*b*	*c*	*d*
f	*g*	*h*	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*	*q*	*r*	*s*	*t*	*u*	*v*	*w*	*x*	*y*	*z*	*a*	*b*	*c*	*d*	*e*
g	*h*	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*	*q*	*r*	*s*	*t*	*u*	*v*	*w*	*x*	*y*	*z*	*a*	*b*	*c*	*d*	*e*	*f*
h	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*	*q*	*r*	*s*	*t*	*u*	*v*	*w*	*x*	*y*	*z*	*a*	*b*	*c*	*d*	*e*	*f*	*g*
i	*j*	*k*	*l*	*m*	*n*	*o*	*p*	*q*	*r*	*s*	*t*	*u*	*v*	*w*	*x*	*y*	*z*	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*
j	*k*	*l*	*m*	*n*	*o*	*p*	*q*	*r*	*s*	*t*	*u*	*v*	*w*	*x*	*y*	*z*	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*
k	*l*	*m*	*n*	*o*	*p*	*q*	*r*	*s*	*t*	*u*	*v*	*w*	*x*	*y*	*z*	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*
l	*m*	*n*	*o*	*p*	*q*	*r*	*s*	*t*	*u*	*v*	*w*	*x*	*y*	*z*	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	*k*
m	*n*	*o*	*p*	*q*	*r*	*s*	*t*	*u*	*v*	*w*	*x*	*y*	*z*	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	*k*	*l*
n	*o*	*p*	*q*	*r*	*s*	*t*	*u*	*v*	*w*	*x*	*y*	*z*	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	*k*	*l*	*m*
o	*p*	*q*	*r*	*s*	*t*	*u*	*v*	*w*	*x*	*y*	*z*	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	*k*	*l*	*m*	*n*
p	*q*	*r*	*s*	*t*	*u*	*v*	*w*	*x*	*y*	*z*	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	*k*	*l*	*m*	*n*	*o*
q	*r*	*s*	*t*	*u*	*v*	*w*	*x*	*y*	*z*	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*
r	*s*	*t*	*u*	*v*	*w*	*x*	*y*	*z*	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*	*q*
s	*t*	*u*	*v*	*w*	*x*	*y*	*z*	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*	*q*	*r*
t	*u*	*v*	*w*	*x*	*y*	*z*	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*	*q*	*r*	*s*
u	*v*	*w*	*x*	*y*	*z*	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*	*q*	*r*	*s*	*t*
v	*w*	*x*	*y*	*z*	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*	*q*	*r*	*s*	*t*	*u*
w	*x*	*y*	*z*	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*	*q*	*r*	*s*	*t*	*u*	*v*
x	*y*	*z*	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*	*q*	*r*	*s*	*t*	*u*	*v*	*w*
y	*z*	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*	*q*	*r*	*s*	*t*	*u*	*v*	*w*	*x*
z	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*	*q*	*r*	*s*	*t*	*u*	*v*	*w*	*x*	*y*

Now look in the left-hand vertical column for the first letter of the text *P*, and in the upper horizontal column for the corresponding letter of the key *S;* where these columns meet *h* will be found, which write down. Proceed in the same manner for the representation of the next letter *r*, (that is, at the junction of the columns *r* and *i*) which will be *z*; and so on through the whole of the paragraph. It will then appear as follows:

Hzfalmra kfh nzegkzhnudu rq puxf sa gvmcxicc

The inverted process for deciphering needs no explanation.

I am, Sir, *&c. &c.*

B.

ART. XI. *A Translation of* REY'S *Essays on the Calcination of Metals,* &c.

[Communicated by JOHN GEORGE CHILDREN, Esq., F. R. S., *&c.*]

Continued from page 64.

ESSAY XV.

Air diminishes in weight in three ways. The balance is deceitful, the means of remedying that.

I RESUME the thread of my discourse, which I had somewhat interrupted to solve the objection advanced against me, and by so much the better clear up this matter; and I say, that in three preceding essays; *viz.*, the 10th, 11th, and 12th, I have shewn the three different ways by which air increasing in weight, may manifest that increase when balanced in a free and pure air. Now the law of contraries requires, that, by three ways, the reverse of the former, it may diminish in weight. These ways are the separation of any heavier foreign matter—its extension to more ample bounds, and the abstraction of its heavier parts. But since the knowledge of the former throws sufficient light upon the latter, I omit any fuller explanation; only begging the reader to observe, that this increase or diminution of weight, of which I have spoken in the said essays, always refers to a portion of air compared with another of equal bulk. For when we do not take the volume of a substance into account, if we examine its weight by reason, I say that nothing gains weight but by the addition of matter, nor loses it but by its subtraction—so inseparably are matter and weight united, as has been shewn above in the sixth essay. But if we investigate the subject by the balance, a case occurs in which, without any addition or subtraction of matter, a substance will appear more or less heavy; namely, by its contraction or expansion. Now this was the only mode of investigation known to the ancients when they contended that the elements, in their mutual conversion into one another, increase or diminish in weight in proportion as they increase or diminish in dimension, by the sole aid of nature, very expert in this operation—not that art is unable to increase or diminish the weight of things, dilating or contracting them. Hammer a piece of cold iron for a considerable time; you will unite its parts and diminish its bulk, and then it will appear heavier when put into the balance. Likewise, if you put a ball of feathers, closely tied up, into the scale, it will weigh more than when the feathers are left at their full size. From this I infer, what has been already cursorily touched on, that the balance is so fallacious, that it never indicates the true weight of substances, except when two portions of the same substance and figure, as two leaden bullets, are counterpoised against each other. But two ingots, one for instance of gold, and the other of iron, which appear by the balance to be equal, are nevertheless not so—for the iron is as much heavier than the gold, according to reason, as the air which it displaces is heavier than that displaced by the gold, which difference I could shew exactly in every thing we weigh, and could reduce the whole to its just weight, if I had made the trial I have suggested above in the 7th essay.

ESSAY XVI.

Formal answer to the question, Why Tin and Lead increase in weight when they are calcined?*

I have now made the preparation; laid as it were the foundations of my answer to the Sieur Burn's demand; namely, that

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* *Note by M. Gobet.*

John Baptist Porta, a Neapolitan gentleman, author of several works on Natural History and Agriculture, has recorded the same fact.

"Plumbum ponderosius reddere, docet Galenus. Nam comprobans plumbum particeps esse aëreæ substantiæ, hoc affert experimentum. Omnium quæ novimus, unicum plumbum, tum mole ipsâ, tum pondere augetur, si condatur in ædibus subterraneis, aërem habentibus turbidum, ita ut quæcumque illic ponantur, celeriter situm colligant. Tum etiam plumbea statuarum vincula, quibus earum pedes annectuntur, sæpe numero crevisse visum est, et quædam adeo intumuisse, ut ex lapidibus dependerent crystalli, modo verrucæ." (Lib. v. cap. 11.)—*Magiæ Naturalis, libri viginti.*—Hanoviæ, 1619.

Bacon says the same,—*(Sylva Sylvarum.)*

having put *two pounds six ounces* of fine English tin into an iron vessel, and heated it strongly on an open fire for six hours, stirring it continually, without having added any thing, he obtained *two pounds thirteen ounces* of a white calx; which at first occasioned him great surprise, and the desire to ascertain whence these seven ounces of increase were derived.—Now, to augment the difficulty, I say, that we must not only inquire whence these seven ounces are derived, but, moreover, whence that which has replaced the loss of weight, necessarily arising from the enlargement of volume of the tin, by its conversion into calx, and from the vapours and exhalations that have escaped. To this question, then, resting on the foundations I have laid, I answer, and proudly maintain, "That this increase of weight comes from the air, thickened and made heavy, and in some measure rendered adhesive in the vessel by the violent and long-continued heat of the furnace—which air mixes with the calx (its union being assisted by the continual stirring), and attaches itself to its smallest particles—no otherwise than as water, when sand is thrown into it, makes it heavier by moistening it, and adhering to its smallest grains." I imagine there are many persons who would have been *startled (effarouchées)* at the mere statement of this answer, had I given it in the outset, that will now receive it willingly, being, as I may say, *tamed (apprivoisées)* and rendered tractable, by the evident truth of the preceding essays. For doubtless they whose minds were preoccupied with the opinion that air is absolutely light, would have rushed to the encounter, exclaiming, Why do we not extract heat from cold, white from black, light from darkness, if from air, a thing absolutely light, we can extract so much weight? And they, who might have given credit to the weight of air, would have been unable to persuade themselves, that it could ever increase the weight of a substance balanced in itself.

On this account I have been obliged to shew, that air is possessed of weight; that it is proved by other investigation than that of the balance; and that even by that instrument, a portion previously altered and thickened, may make its weight manifest. This I have done as briefly as possible, without having advanced any thing not bearing fully on the subject; to clear up which, in every point, it only remains to give a succinct account and refutation of the opinions which others have followed, or follow; and to solve the objections that might be made to my answer.

ESSAY XVII.

It is not the disappearance of the celestial heat which animates the Lead, or the death of the latter that increases its weight in calcination.

Of all, as far as I know, that have written on this question, Cardan is the first, who, in his fifth book, *De Subtilitate*[*], says, that lead, by conversion into ceruse, or by calcination, gains one-thirteenth part in weight, and gives this reason for it—The lead dies, for the celestial heart, which was its soul, vanishes; whose presence gives it life, and renders it light; as its absence occasions its death, and makes it heavy. This he confirms by the example of animals, which become heavier after death, from the extinction of the celestial heat, the soul, (as he thinks), both of animals and all other mixed and compound bodies. This opinion is defective, to say no worse of it, in many respects. First, in attributing life to lead. Secondly, in supposing that the presence of the celestial heat makes it light, and its absence heavy. Thirdly, because it assigns the same reason for the increased weight of lead by calcination, and of animals by death. There is nothing of the kind. For as to life, how can lead possess it, since it is a homogeneous body,

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[*] *Note by M. Gobet.*

"Nam plumbum cùm in cerussam vertitur, ac uritur, tertiâ decimâ parte sui ponderis augetur. Hoc fit, quia calor ille cœlestis evanescit: nam certum est, adjici nihil, et tamen crescit: cùm igitur par ratio etiam in animalibus videatur, quæ graviora morte fiunt, quoniam exhalante animá, secum calor etiam, ac quicquid ab illo est elaboratum evanescit: manifestum est corpora metallica, et lapides ipsos etiam vivere." (Lib. v. *de mistione et mistis.*)—Cardani, *de Subtilitate*, lib. xxi. Parisiis, 1551.

This author also observed, that a tile gains weight in burning.

without difference of parts, without organs, and without any vital effect or action? If it move downwards, so does ceruse, which is only its corpse; if it *be cooling* (*rafraischit*), so is ceruse. Then how could it preserve this life, under a million of forms, that it may be made to assume and to cast off, yet always continuing to be lead? How, in the furnace (which would be a much greater wonder), where it may be kept in fusion a day, a month, or a whole year? It must have a very tenacious soul to undergo so much without being dislodged! Moreover, all the world is agreed, that from death to life there is no return. Yet the chemists assure us, if we moisten the calx of lead, and mix it with water in which samphire (salicot) has been dissolved, then, having dried it, put it in a crucible with a small vent, and heat strongly and quickly, that we shall reduce it to its original state. With regard to the celestial heat making bodies light, Scaliger* very properly objects that the

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* "In lapide, inquis, vita.—Quia ubi calor, ibi anima. Ubi anima, ibi vita. Igitur in lapide vita. Ostende mihi tu in lapide calorem, et ego ostendam tibi calorem sine animâ. Fortasse negabis in igne calorem; animâ carere non negabis. Nam quod ais, omnem actionem ab animâ provenire, si verum est, dabis animam etiam cerusæ. Descendit enim. Ergo per terræ formam, quæ est ibi terræ anima, quâ movetur. Sic enim et sapis, et scribis. At ais, mortuam cerusam, quia facta est è plumbo, à quo calor ille cœlestis exhalavit. Calorem hunc, inquam, ostende nobis. Nam calor, non nisi tactu cognosci potest. Equidem lapidis contactu nunquam pilo factus sum calidior. Effectionem, deces, nempe descensionem. Nego tibi fieri à calore. Si enim à calore fieret descensio, à calore non fieret, ascensio. Exemplum verò de cerusa ridiculum. Cerusà, inquis, plumbo gravior: quia calor abiit cœlestis ille. Ergo cœlum habet levitatem, et erit quintum corpus cum aliis univocum. Namque viventia, O! subtilissime Philosophorum, non propter animam levia sunt, sed propter calorem elementarem. Alioqui omnia tua entia essent levia. Quippe vivunt omnia. Vivunt autem propter formam. Forma illa anima, anima illa calor. Calor ille autor levitatis. Levia igitur omnia, quia materiæ forma dominatur: et cœlestia potiora, atque efficaciora. Tactu autem percipimus calorem elementarem. Quem in plumbo qui sentit tactu, sentiat etiam in igne frigus. At plumbum absumptis partibus aëreis gravius fit. Quâ de causà later quoque coctus crudo gravior. At contra, arundo combusta levior fit. Verè tamen illa, atque sine controversià viveb At ejus abiit anima; ita de aliis arboribus dicendum. Igitur anima neque gravis, ne-

heavens, which abound in this heat, as being the source of it, *must be light (feut leger)*, and consequently *univocal* (*univoque*) with the other bodies, which is absurd. Neither can the loss of this heat render them heavy, for I have already proved that nothing increases in weight but by the addition of matter, or by diminution of volume; but here there is nothing of the kind; so that the disappearance of the heat cannot add any thing, and as to its bulk, it is visibly enlarged; the compact and solid substance of the lead being reduced (*amenuisée*) to so many small parcels, that their number is almost infinite. Plants too ought to become heavy by death, the celestial heat being expelled: but the contrary is evident to all. As to the increased weight of animals by death, the true cause, far remote from that which increases the weight of lead when calcined, is this: in the living animal its natural heat subtilizes, dilates, and augments the dimensions of the humours, the flesh, and every thing in it capable of dilatation—but losing this heat by death, the whole on this becoming cold, contracts and diminishes, whence the increase of weight, as I have often said already. What is there like this in lead? Thus the opinion of Cardan appears so frivolous, that I am grieved that a great man, and one who is justly esteemed by all the world, should have lately declared to me that he inclines towards it.

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que levis; neque cœlum ipsum. Id quod è secundo de animâ, primo et secundo de cœlo colligi potest. Ideo cerusa friabilis, amisso pingui aëreo. Tum quæro de te plumbum quomodo vivum sit. Namque est excoctum igni, sicuti cerusa aceto excocta est. Ignis autem destruit: ut passim, quiritaris, apud te vixerit in fodinâ, in apothecis, in fornacibus interemptum nequit vivere. Alioquin ab igne nostro cœlestis ille tuus esset generatus." (Exercit. ci. cap. 18.) And further on, he says, "Plumbum quoque aiunt augescere. Calcem in fornace tectâ vidimus adeò turgescere, ut tigna atque tegulas sustolleret."

Jul. Cæs. Scaligeri, *de Subtilitate*, &c. Francofurti, 1592.

[To be continued.]

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

The meetings of the Royal Society were resumed on Thursday the 8th of November, after the long vacation. At this and the succeeding meeting the *Croonian Lecture* was read by Sir Everard Home: it contained an account of the means by which the eye adjusts itself to the distinct vision of near and remote objects.

Thursday, Nov. 22. The *Bakerian Lecture* by Captain Edward Sabine, was read; it contained an account of experiments to determine the dip of the magnetic needle in London in August 1821, with remarks on the instruments usually employed in such determinations.

Friday, Nov. 30, being St. Andrew's day, the Society held their anniversary meeting. At this meeting the President announced the allotment of two of Sir Godfrey Copley's prize medals to J. F. W. Herschel, Esq. and Captain Edward Sabine, in a very appropriate and impressive discourse, of which the following is an outline.

The progress of discovery, said Sir Humphry, even when belonging to past times or distant countries, is always an agreeable subject of contemplation to philosophical men, but the pleasure derived from it is much higher when it arises from the exertion of the talents of our own countrymen, when it has originated in our own body, and when there is the power not only of acknowledging and rejoicing at it, but likewise of distinguishing the persons to whom it is owing, by a permanent mark of respect; he, therefore, had much satisfaction in announcing the decision of the Council of the Society, upon the present occasion. The President then took a review of the labours of Mr. Herschel, and more particularly of those which were considered as entitling him to the present mark of distinction. He said, there was no branch of science more calculated to awaken our admiration than that which Mr. Herschel had so successfully cultivated: the sublime or transcendential geometry not only demonstrated the powers and resources of human intellect, but also the wisdom and beauty of the laws which govern the universe. It is, perhaps, said the President, the highest triumph of human intelligence, that proceeding from the consideration of mere unities, or points, lines, and surfaces, it should by gradual generalizations, substitutions, and abstractions, be able to arrive not only at the knowledge of all possible conditions of number and quantity, but likewise of time and motion; and by employing its own pure intellectual creations, in many cases anticipate the results of observation and experiment, and determine the movements not only of the bodies which form permanent parts of our system, but likewise of those which seem only occasionally to visit it, and which belong as it were to the immensity of space.

Sir Humphry then paid an eloquent tribute of applause to the zeal and success with which Mr. Herschel had pursued these inquiries, and proceeded to enumerate his mathematical communications to the Royal Society, printed in their Transactions. He should not, he said, attempt an analysis of these papers, for they required profound study; they were especially distinguished by the simplicity of the processes, by perspicuity of arrangement, and by the absence of all metaphysical abstractions, and they proved in the author an intimate acquaintance with the works of the great masters of analysis; he had not, however, limited himself to formulæ, but had a higher claim upon the approbation of the Society in their application; for, though as a mere exercise, the higher mathematics strengthen the reasoning faculties and afford intellectual pleasure, yet it is in enabling us to solve the physical phænomena of the universe that they have their grandest end and use; in these respects, said Sir Humphry, they are really power, and they may be compared to that power which we witness in the vapour of water, which passing into the free atmosphere, exhibits only a striking spectacle, but which applied in the steam-engine, becomes the moving principle of the most useful and extensive machinery, and the source of the most important arts of life.

Sir Humphry then adverted more particularly to Mr. Herschel's investigations connected with the polarization of light, and to the importance in practical optics of his paper on the aberration of compound lenses and object-glasses; and having stated to the Society the leading features of these communications, said that he felt convinced they could not but approve of the decision of their Council upon the grounds he had already mentioned, and should therefore not attempt further to expatiate upon the diligence and merits of their author.

In addressing Mr. Herschel personally, Sir Humphry alluded to several subjects of inquiry which he recommended to Mr. Herschel's attention; he trusted that he would continue to devote himself to philosophical pursuits, and that he would receive the medal not merely as a mark of respect for acquirements already made, but as a pledge of future exertions in the cause of science, and of the Royal Society. "Believe me," he added, "you can communicate your labours to no public body by whom they will be better received, or through whose records they will be more honourably transmitted to the scientific world. And these pursuits, you will find not only glorious, but dignified, useful, and gratifying in every period of life; this, indeed, you must know best in the example of your illustrious father, who, full of years and of honours, must view your exertions with infinite pleasure, and who, in the hopes that his own unperishable name will be permanently connected in the annals of science with yours, must look forward to a double immortality."

The President then spoke of the researches of Captain Edward Sabine; he eulogized his industry and perseverance in conducting his inquiries in the Arctic Expedition, and his fortitude and patience in enduring the hardships and privations to which he was then exposed: his experiments he said, were principally conducted on the ice of the polar sea, where the vessel was for several months frozen up; during a considerable portion of the time, he was in darkness, or only guided by a very doubtful twilight, and the temperature of this inclement spot, probably as cold as any belonging to the northern hemisphere, was such, that the artificial horizon of mercury became frozen during an observation; yet Captain Sabine's inquiries seem to have been conducted with as much care and precision as if he had been possessed of the conveniencies and luxuries of a Royal Observatory, and the advantages and repose of the happiest climate and situation.

Sir Humphry then entered into some historical details respecting the researches connected with the length of the seconds' pendulum in different latitudes, and its application to determine the figure of the earth. The true investigation of the properties of the pendulum as an universal standard of measure was owing he said to the zeal and enlightened views of Mr. Davies Gilbert, who had directed all the scientific talents and resources of the country to the object, by making it a question of national importance in parliament; the fortunate result had been the invariable pendulum contrived with such a happy spirit of invention, and examined with such unceasing activity and minute accuracy, by Captain Kater.

" Captain Sabine did not accompany the Arctic expedition now absent, because," said the President, " he conceived that he had effected all that he was capable of performing with the pendulum in northern latitudes, which was the great object of his researches in the two former voyages; but his scientific ardour had induced him to endeavour to complete his investigations, even to the Line; and having braved the long night and perpetual winter of the Polar Regions, he was now gone with the same laudable object to expose himself to the burning sun and constant summer of the Equator."

In Captain Sabine's absence, the President delivered the medal to his brother. " In informing him," said Sir Humphry, " of what has taken place this day, you will, I trust, state to him our deep sense of his merits; his knowledge of this expression of our opinion may, perhaps, animate him during the difficult enterprise he has undertaken, for he has already shown how highly he values the praise of the Royal Society, which, with the good opinion of his countrymen, has been hitherto the only reward of his labours. Assure him how strongly we feel his disinterestedness and genuine love of science, and that our ardent wishes are expressed for his safe return, and for the successful accomplishment of the objects of his voyage, which will ensure to him additional claims upon the gratitude of all lovers of science."

The Society then proceeded to the election of a Council and Officers for the year ensuing. On examining the lists, the following was found to be the state of the elections:

1. *Members of the Old Council to continue for the ensuing Year.*

Sir H. Davy, Bart.
William Thomas Brande, Esq.
Samuel Goodenough, Lord Bishop of Carlisle.
Taylor Combe, Esq.
Davies Gilbert, Esq.
Charles Hatchett, Esq.
J. F. W. Herschel, Esq.
Sir Everard Home, Bart.
John Pond, Esq.
William Hyde Wollaston, M.D.
Thomas Young, M.D.

2. *Members of the Society chosen into the Council.*

George, Earl of Aberdeen.
Matthew Baillie, M.D.
John Barrow, Esq.
B. C. Brodie, Esq.
William Hamilton, Esq.
James Ivory, Esq.
Henry Marquis of Lansdowne.
Alexander Marcet, M.D.
Thomas Murdoch, Esq.
Sir Robert Seppings, Knt.

3. *Officers for the Year ensuing.*

President, Sir Humphry Davy, Bart.
Treasurer, Davies Gilbert, Esq.
Secretaries, { William Thomas Brande, Esq.
Taylor Combe, Esq.

Thursday, Dec. 6.—A paper was communicated by the Society for the Improvement of Animal Chemistry, entitled, "On some Alvine Concretions found in the Colon of a young Man, in Lancashire, after death." By John George Children, Esq. F.R.S.

Thursday, Dec. 13.—A paper was read, " On the Concentric adjustment of a triple Object Glass." By W. H. Wollaston, M.D. and V.P.R.S.

On the same evening a communication was also read, entitled, " On a new species of Rhinoceros, found in the interior of Africa; the skull of which bears a close resemblance to that found in a fossil state in Siberia, and other Countries: by Sir Everard Home, Bt., V.P.R.S."

Thursday, Dec. 20.—A paper was read on the Electrical Phenomena exhibited in vacuo, by Sir H. Davy, Bart., P.R.S.

The Society then adjourned over the Christmas Vacation, to meet again on Thursday, the 10th of January, 1822.

ART. XIII. *Account of the Progress of Foreign Science.*

THE daily increasing intercourse between this country and foreign nations, has enabled us to extend and systematize this valuable department of a scientific journal. In future, the account of foreign science will be presented in the following order:

I. CHEMICAL SCIENCE. 1. Principles of combination. 2. Repulsive power or caloric. 3. Simple bodies. 4. Inorganic compounds; acids, bases, salts, *&c.* 5. Organic compounds, vegetable and animal. 6. Analysis and chemical apparatus.

II. APPLICATIONS of CHEMISTRY, to Medicine and the Arts; Agriculture, Bleaching, Dyeing, *&c.*

III. PHYSICS; Optics, Electricity, Magnetism, Acoustics, *&c.*

IV. MECHANICS. 1. Statics, Dynamics. 2. Hydrostatics and Hydrodynamics. 3. Ærostatics and Pneumatics.

V. NATURAL HISTORY; Zoology, Botany, Mineralogy, Geology, Meteorology.

VI. ECONOMICS; or miscellaneons applications of Science to the ordinary uses of life.

The deficiencies in any of the above departments which may exist in one Number, we shall endeavour to supply in another.

I. *Chemical Science.*

1. PRINCIPLES OF COMBINATION. It gives us pleasure to observe, that the accumulation of new facts in favour of the chloridic theory first promulgated by Sir Humphry Davy, has at length forced something like an assent to its truth, from the ablest and most obstinate partisan of the French oxymuriatic hypothesis. Professor Berzelius, after waging a war of obscure and ineffectual words, like Priestley with Phlogiston, against the English system, says, in his late paper on the Sulpho-cyanides: "What I have just stated, both on the cyanides in my preceding memoir, and on the sulpho-cyanides in the present, proves that the phenomena relative to these substances, *can admit of explanation only*, on a theory quite analogous to that which in these later times has been adopted for the muriates, in considering oxymuriatic gas, as a simple body-chlorine. This theory becomes more general and at the same time more interesting, by extending it to all the acids combined with water (hydrates), as well as to all the salts, in the manner proposed by M. Dulong, by considering the acids and the salts, as compounds of hydrogen and of metals combined with the radical of the acid, and the oxygen both of the acid and of the base, together forming one body." "The decompositions and the recompositions of water, which I have conjointly with other chemists, regarded as a great objection against the new theory of the nature of muriatic acid, take place with the sulpho-cyanides in a manner which leaves no doubt of their reality; and, we may add that the cyanides, and especially the sulpho-cyanides, have a perfect analogy with the salts formed by oxidized bases, and oxygenated acids; that is to say, formed by a combustible radical and oxygen*." This *Palinodia* would have been better without the errors tacked to it. The above view of the constitution of acids and salts was given long ago by Sir H. Davy, as may be seen in his paper, "On the analogies between the undecompounded substances, and on the constitution of acids," published in the first volume of this Journal. Thus; "it is a simple statement of facts to say, that liquid nitric acid is a compound of two proportions of hydrogen, one of azote, and six of oxygen; and, as I shewed long ago the only difference between nitre and hyperoxymuriate of potash is, that one contains a proportion of azote, and the other a proportion of chlorine," p. 287. And, with regard to these decompositions and recompositions, they are mere fictions conjured up by himself and the late Dr. Murray. Chlorine dislodges oxygen from potash, soda, lime, *&c.*,

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* Annales de Chim. et de Physique, Tome XVI, pp. 35, 36, et 37.

as we know by indubitable experiments; and, what reason is there to fancy or to assert, that on putting any of these chlorides into water, the oxygen of the latter displaces the chlorine, which goes to the liberated hydrogen to form muriatic acid? In fact, the water quietly combines with a chloride as it does with an oxide; so that the ghost is entirely of their own raising, and they may lay it at their leisure. If heat be applied, indeed, then a decomposition may be induced under new circumstances, as happens with some peculiar chlorides.

The jealousy of English predominance in European affairs, which was somewhat natural, and perhaps excusable in a military and political point of view during the late war, is quite absurd and ludicrous, as now displayed by many eminent continental chemists with regard to the theory of chemical equivalents, or the English atomic theory. Berzelius and many other philosophers, whom we could name, still continue to refer all their analyses of oxides and salts to a supposed principle, that the oxygen in the oxides is a sub-multiple, by a whole number, of the oxygen in the acids. Now, this shews a very limited and imperfect comprehension of the general laws of equivalent combination, as taught by Mr. Dalton, and as represented on Dr. Wollaston's scale. In fact, that supposed *principle* of Berzelius, is merely a partial and accidental corollary of the great system of chemical proportions. Thus, as carbonic acid contains two atoms or proportions of oxygen, its combinations with protoxides will give the ratio of oxygen in it, to that in them, of two to one; but with deutoxides, the ratio of equal quantities. Again, as sulphuric acid contains 3 atoms or proportions of oxygen; its combinations with protoxides will furnish the ratio of oxygen in it, to that in these bases, of three to one; but with deutoxides, the ratio of 3 to 2, or 1½ to 1. And, as nitric acid contains 5 atoms or proportions of oxygen, its combinations with protoxides will give the ratio of oxygen in it, to that in these bases, of 5 to 1; but with deutoxides, the ratio of 5 to 2; or 2½ to 1; or that of 1 to 0.4. Hence this partial conception and imperfect representation of the theory of equivalents, leads to very confused and perplexing results, which we altogether avoid, by tracing up the ratio for every simple or compound body, directly to its place in the general scale of proportions.

We shall give an example or two from a very recent and otherwise very valuable paper of Berzelius, "on the composition of several inorganic combinations, which serve as the base of calculations, relative to the theory of chemical proportions *." "Bucholz found by a series of very exact experiments, that the sulphuret of molybdenum contains 60 parts of metal to 40 of sulphur; and, that these 60 parts of metal absorb 30 parts

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* Annales de Chim. et de Physique, Tome XVII, p. 6, et seq.

of oxygen to become molybdic acid. It follows that for molybdenum, there is between the sulphur and the oxygen of the acid a ratio analogous to that which we have found, for the sulphuret and the acid of arsenic; that is to say, the sulphuret of molybdenum is proportional to a degree of oxidation inferior to that of the acid." Now, this proposition is true no doubt, but very much mystified in the enunciation. The sulphuret of molybdenum is evidently a compound of one atom metal + 2 atoms sulphur; or is a bisulphuret: and the acid is like all the well-marked metal acids*, a compound of one atom metal + 3 atoms oxygen. Hence the ratio of 4, (=2 × 2) to 3 as found by Bucholz.

Ten grammes of nitrate of lead, equivalent to 6.731 oxide of lead, yielded of molybdate 11.068 grammes, according to Berzelius. He then proceeds: " By a very simple calculation we find that the molybdate of lead is composed of—

Molybdic acid .	39.185 .	100.0
Oxide of lead .	60.815 .	155.2

Now, 155.2 parts of oxide contain 11.129 parts of oxygen, which are a submultiple of 50 (the quantity of oxygen absorbed by 100 of molybdenum, in the experiments of Bucholz,) by 3; for 11.129×3=33.387. It follows thence that, in the neutral molybdates, the acid contains three times as much oxygen as the base. It is then composed of—

Molybdenum .	66.613 .	100.00
Oxygen .	33.387 .	50.12

Now, this marvellous prolixity and obscurity of induction, might have been all saved, by referring molybdenum directly to its place in that general scale of equivalents of which oxygen is the *radix*, or *modulus*. He would also have avoided the unwarranted position with which the quotation concludes, of the acid containing three times as much oxygen as the base; for molybdic acid forms an insoluble precipitate with some of the deutoxide salts of copper, which compound consists, in all probability, of an atom of acid to an atom of base; and therefore the former will contain a quantity of oxygen, to the latter in the ratio of 3 to 2, and not, as he says, of 3 to 1. Again, when treating in the same paper of the composition and the capacity of saturation of chromic acid, Berzelius says, " It thence follows that the chromate of lead ought to be composed of—

Chromic acid .	31.853 .	100.000
Oxide of lead .	68.147 .	213.924

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* Thus, as to the acids of Chromium, Manganese, Arsenic, Tungsten, consist each of one atom metal + 3 atoms oxygen. Selenic acid has two atoms, but its base can hardly be called a metal. Antimonic acid has also probably two; but it resembles an oxide more than an acid.

Now, the 213.924 parts of oxide of lead contain 15.34 parts of oxygen. This number, then, expresses the capacity of saturation of the chromic acid." That is to say, that weight of every base which 100 parts of chromic acid neutralize, must contain 15.34 parts of oxygen. Again, from the decomposition of Siberian chromate of lead, he thus argues: "The loss in this experiment can be nothing but oxygen; so that 24.25 parts of green oxide of chromium will afford, with 7.37 parts of oxygen, 31.62 parts of chromic acid. Now, the oxygen contained in the 68.38 parts of oxide of lead is 4.9, which, multiplied by $1\frac{1}{2}$, gives a product of 7.35; that is to say, chromic acid, when it is reduced to the state of green oxide, loses a quantity of oxygen, equivalent to $1\frac{1}{2}$ times its capacity of saturation." "The analysis" (of chromate of barytes) "has then given—

Barytes .	59.68
Oxide of chromium .	30.43
Loss .	9.69

These 59.88 parts of barytes contain 6.26 parts of oxygen, and $6.26 \times 1\frac{1}{2} = 9.39$. The result of this analysis accords, therefore, with that of the preceding experiment (on the Siberian chromate.) The small difference (=0.3,) can be ascribed only to an error of observation, inseparable from this mode of operating."

Now, we must confess that, though the experiments were judicious and accurate, and though the reasoning be ultimately not untrue, yet it is presented in so involved a manner as to create very superfluous confusion to the reader. Instead of hanging his results upon a little peg of a corollary to the system of equivalents, why does he not assign them their due position in the general fabric, so that we could at once see their relation to every other chemical body, on the comprehensive plan of Richter? Direct experiments shew that chromic acid has the equivalent weight of 6.5 on the oxygen scale (or 6.54 by the above synthesis of the chromate of lead;) and that it contains 3 atoms of oxygen to one of metal, while its green oxide seems to consist, by the preceding experiments, of 3 atoms of oxygen to 2 of metal. On this point we shall say a word or two in its place; our sole object here being the general principles of combination.

M. A. le Roger et J. A. Dumas, Pharmaciens, have subjected a number of bodies to a nice hydrostatic process, in order to determine their specific gravity very exactly, and thence deduce a general atomic law. They employed both alcohol and oil of turpentine for the liquids of immersion. They justly observe that M. Hassenfratz, by employing mercury as the hydrostatic liquid in the examination of salts, fell into gross errors, such as that caustic lime and calcined alum were lighter than water, errors which are ingrafted, without comment, into the largest system of English chemistry. The above experimentalists imagine that, on considering the quotients obtained by dividing the weights of the atoms by the respective specific gravities of the bodies, they find the volumes of the atoms to form a series, which pervades the greater number of simple substances. Although we conceive this series to be a matter of illusion, yet as the hydrostatic experiments seem to have been executed with care, we shall insert their first table of results.

Name of the Substance.	Weight of the Atom.	Density reduced to the Vacuum and at 39.2° Fah.	Volume of the Atom.	Ratio.	Volume of the Atom by Calculation.
Ice *	112.4354	0.950	117	1	116
Silica	596.42	2.650	225	2	232
Boracic acid	269.65	1.830	220	2	232
Arsenious acid	1240.77	3.698	335	3	348
Protoxide of copper	891.39	5.749	155	$1\frac{1}{3}$	155
Oxide of bismuth	1973.8	8.449	233	2	232
Oxide of lead	2789.0	8.010	347	3	348
Peroxide of mercury . . .	2731.6	11.29	240	2	232
Caustic lime	712.06	3.08	231	2	232
Carbonate of lime	1262.72	2.717	464	4	464
Solid carbonic acid in the carbonate of lime	. . . . .	. . .	116	1	116
Anhydrous sulph. lime . . .	1714.38	2.960	579	5	580
Solid sulph. acid in sulphate of lime	. . . . .	. . .	174	$1\frac{1}{2}$	174
Crystallized sulph. of lime .	2164.12	2.322	932	8	928
Alumina	642.32	4.200	152	$1\frac{1}{3}$	154
Nepheline, silicate of alumine	1238.75	3.270	378	$3\frac{1}{3}$	386
Water in cryst. sulph. lime .	. . . . .	. . .	88	$\frac{3}{4}$	88
Sulphur	. . . . .	2.086	. .	.	. .

In the preceding table, the common divisor employed by these gentlemen is 116= their volume of the atom of ice. Their atomic weights are mostly taken from Berzelius. They seem perfectly unacquainted with Dr. Prout's happy generalization; which renders it more than probable that the weight of all the atoms are multiples of the atomic weight of hydrogen, by a whole number. Indeed, we have little reason to be surprised at this ignorance in these gentlemen, when M. Berzelius

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* Our experiments on the sp. gr. of ice give the number 0.9133. It was taken in oil of turpentine, at 32° Fahr.—*Trans.*

and M. Dulong, two of the most distinguished chemists on the Continent, in the sequel of their late elaborate memoir on the specific gravity of some elastic fluids *, speak of Dr. Prout's speculations in the following words:—"Before concluding we shall observe, that the new determinations, which we now present, differ little from those which are to be found in an anonymous memoir, printed in the *Annals of Philosophy*, for November 1815, and February 1816. But the English author has made no experiments, and the hypotheses, which he employed to correct his adopted numbers, being absolutely gratuitous or false, his results could not inspire any confidence." This we consider as a harsh and unjustifiable criticism. Can any thing be a better proof, that the hypotheses were *not* gratuitous and *not* false, than that *after the* progressive investigations of five years, two French chemists, by applying the most refined and rigid methods of experiment, should arrive at precisely the same results, which the English author had theoretically deduced?

II. CALORIC.—The principal paper on this subject, which has recently appeared on the Continent, is that of M. Navier, Professor of Practical Mechanics in the *Ecole des Ponts et Chaussées* of Paris; and it is entitled, "Note on the Mechanical Action of Combustibles †." Several writers have endeavoured to establish a comparison between the mechanical actions, capable of being produced by the same weight of a combustible, employed to vaporize water, and to heat atmospherical air. It has been advanced, that the latter process, making allowance for the variable causes of loss which occur in machines, might be more advantageous than the former. Several artists have endeavoured to construct machines on this principle ‡.

In the calculations presented on this question, there exists a cause of error, which has been remarked by M. M. Clement and Desormes, in a memoir presented to the Academy of Sciences, in 1820, but not yet printed. The authors of those calculations had not taken into account, the quantity of heat which the air absorbs, when it dilates, without changing its temperature. Mr. Navier takes for his unity of heat the quantity necessary to raise by 1° C., the temperature of a kilogramme (about 2¼ pounds avoirdupois) of water in the liquid state; this unity he styles a *degree of heat*. The number of *degrees of heat* necessary to raise by 1°, the temperature of 1

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* *Annales de Chimie et de Physique.* tom. xv. p. 386.

† *Ibid*, tom. xvii. p. 357.

‡ By far the most ingenious of these is the heated air-engine, for which a patent was, a few years back, granted to the Rev. Mr. Sterling, of Kilmarnock. Of this we shall give a short account in a future Number.

kilogramme of air, of which the volume, which was w at the temperature 0°C., under the atmospheric pressure $0^{m}76$,= (29.92 English inches), has become O, he represents by the following formula :—

$$0.26\sqrt{0.76\frac{o}{w}+0.24}$$

By means of these results, he seeks to establish a relation between the quantity of action (power) which it is possible to obtain, in heating and dilating the air, and the quantity of heat which is consumed. When the volume of the atmospheric air is thus made to vary, the quantities of heat, which it absorbs or disengages, are proportional to the variation of the specific heat. So that, calling C the primitive specific heat, c the specific heat after the change of volume, we have for the quantity absorbed ; a, (c ——— C), a being a constant co-efficient, whose value in round numbers may be stated at 1200°. After a train of algebraic formulæ which we cannot here insert, he concludes, that in expending a degree of heat to warm the air, we cannot obtain a quantity of action which surpasses the elevation of a weight of 33 kilogrammes (72.8 libs.) to the height of one metre ; considering always 500° C. (= 932° F.) as the highest temperature to which it can be carried. In treating of the comparative powers of heat, when employed in the vapour of water, he endeavours to shew, that there is an advantage in producing the vapour at the highest possible temperature. Supposing the vapour produced under the pressure of 5 atmospheres; that is, let H (the elastic force of the vapour) = $3^{m}.8$ (149.6 E inches), and V (corresponding temperature = 165° C. or 329 F *. The value in this case most suitable to V′ (temperature of the condensed water), as may be found by calculation, would be less than 10° C. (50° F.) which has been adopted for the exterior temperature v. Suppose, however, that V′ = 10° C. and of consequence H′ (the elastic force of vapour corresponding to V′ or 10° C.) = $0^{m}.0095$. our formula will then give for the *maximum* of the quantity of action which it is possible to obtain,

123300 Π kilogrammes × metres; and for the *minimum* of the corresponding expenditure of heat; 685 *degrees.*

The ratio of these two numbers being 180, we see that by expending a degree of heat to produce aqueous vapour, the limit of the quantity of action which it is possible to obtain, is the elevation to one metre in height, of a weight of 180 kilogrammes

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* This temperature seems to be calculated from some erroneous formula of the force of steam. The temperature corresponding to five atmospheres, by Dr. Ure's experiments, is only 305 F., or 151.6C.

(368.56 lbs. avoirdupois), supposing the steam generated under the pressure of five atmospheres *.

From these results the theoretic limits of the quantities of action which it is possible to obtain in heating air, or water, are nearly to each other in the proportion of 33 to 180; or of 1 to 5½. The difference is so considerable, that, notwithstanding the uncertainty which may exist in some of the preceding numerical values, the preference due to steam appears to be, in no respect, doubtful.

But steam engines present always a very considerable abatement under the theoretic *maximum* of effect, now computed. It appears that the best steam engines, possessing the average power of ten horses, consume 2½ kilogrammes (5½ lbs. avoirdupois) of charcoal (coal?) to obtain a quantity of action, equal to 28800 k. × m. Admitting that a kilogramme of charcoal consumed in the calorimeter gives 7000° of heat, we see that we obtain, for one degree of heat, only a quantity of action $= \frac{28800}{2.5 \times 7000} = 16.5$ kilog. × met.; which is very far from the limit 180 k × m. found above. It would be important to investigate and to appreciate exactly the causes of this considerable loss. We hope to see many of these important problems, referred to by M. Navier, treated in a clear and conclusive manner, in M. Clement's new Treatise, Theoretical and Practical, on Heat, and its applications to the Arts.

We are indebted to M. Navier, also, for an ingenious memoir, in the same number of the *Annalés,* on the variation of temperatures, which accompanies the changes in the volume of gases. His reasoning is founded on the experiments of M. M. Clement and Desormes, Delaroche, and Berard. From his formulæ it appears, that on reducing the volume of air to $\frac{1}{5}$, so that the pressure becomes 149.6 E inches, we can obtain an elevation of temperature of about 237° C. (426.6 F.)

It further appears, from his mode of research, that the rise of temperature obtained by the condensation of atmospheric air, is susceptible of a limit, somewhat confined. The formula gives 360° C. as the *maximum* effect. As to the cold producible by the dilatation of air, the formula does not assign it any limit.

M. Flaugergues, the astronomer, has made, at the Observatory of Viviers, a series of nice observations on the heat produced by the sun's rays, during the eclipse of this luminary,

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* The formulæ for the *maximum* is,

$\pi .\ 0.76 .\ \frac{\Pi}{0.59} \cdot \frac{1 + 0.00375\ V}{1.375}\ \log. \frac{H}{H'} \cdot$ where $\pi = 13568$ k = the weight of the metre cube of mercury. The other formulary is

$$\Pi\,(530^\circ + V - V').$$

the 7th of September, 1820 [*]. He employed for this purpose the therméliometre, described in the *Journal de Physique*, vol. lxxxvii, p. 256. He concludes from his observations, that the experimental heats are equal to those calculated on the hypothesis, that the effect of the sun-beams is proportional to the extent of surface of the portion of the disc of the sun uncovered; and of consequence that this hypothesis is the true one. Whence we may infer, that the disc of the sun is equally luminous over its whole extent, and that one part taken near the centre, emanates no more rays than an equal portion taken near the border of this disc. The opposite opinion was maintained by Bouguer; who said, that if we compare by the heliometer, (an instrument of his own invention), the sun's centre to a spot distant from it, by $\frac{3}{4}$ of the diameter, the quantities of the rays, which we receive, are in the ratio of 48 to 35. But this distinguished philosopher ingenuously admits, that his experiment contained difficulties which he had not surmounted, and that it, therefore, required verification. The equality of lustre now found by M. Flaugerghes, over all the parts of the disc of the sun, shews, moreover, he thinks the incorrectness of the geometrical hypothesis admitted by several philosophers, that the portions of the surface of a body, project in all directions an equal quantity of light; for on this supposition, the lustre of a spherical luminous body, ought to increase from the centre to the circumference, and become even infinite at the limb; which does not take place, as we may satisfy ourselves, in considering an iron bullet heated to whiteness; or a white paper globe exposed to the light of day. These spherical bodies will appear equally luminous over their whole surface. He also infers from his observations, that the opinion of M. Laplace, who pretends that the sun is sorrounded with an atmosphere such, that were it stripped of it, " this luminary would appear *twelve* times more brilliant," is void of foundation.

Some remarks on the temperature of mines will be found under GEOLOGY.

We shall conclude this subject for the present, by expressing our surprise at the ignorance of the state of English science, displayed in the following introduction to a Memoir of M. Despretz, read at the Institute, 29th November, 1819, and inserted in the 16th volume of the *Annales de Chimie et de Physique*, p. 105:—" It is admitted with Mr. Dalton, in all the works on physics, that on departing from the point of ebullition under the same pressure, or for greater generality, on departing from the point where the elastic forces are equal, the variation in the elastic force of the vapour for a

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[†] *Journal de Physique*, tom. 92, p. 435.

like number of degrees of the thermometer, is the same for all liquids." If M. Despretz will consult Mr. Dalton's *New System of Chemical Philosophy*, vol. i. p. 20, published upwards of thirteen years ago, he will find that the English philosopher there abandons that supposed relation, which otherwise is shewn in a Paper on Heat, published in the *Philosophical Transactions* for 1818, not to accord with experiment. We are thus saved the trouble of analyzing M. Despretz's Memoir.

III. SIMPLE BODIES.—In the number of *Gilbert's Annalen* for last May, Counsellor Giesse, Professor of Chemistry at Dorpat, communicates the account of a new metal, which, he thinks, he has extracted from the residuum left, on distilling to dryness different varieties of English sulphuric acid. One variety left out of 16 ounces, 9½ grains of a white residuum, which differed from the greater part of these residuums by the total absence of sulphate of lead. " The tints of colour," he says, " were surprising, which the residuum exhibited, when it was heated slowly and repeatedly in a platinum crucible over a lamp. From citron-yellow, it passed on cooling back again to white. At the second gradual heating, the mass appeared greenish yellow, then of a fine reddish yellow; and, on cooling, it passed once more through lemon-yellow to white. Strongly heated, a third time, it became sulphur-yellow, lemon-yellow, and reddish-yellow; it then fused with intumescence, and diffused a vapour, smelling of sulphuric acid. On becoming cold, the middle part was whitish; but the borders of a brownish hue. Though the complete series of experiments, which he meditated, were interrupted by a fit of sickness, yet he thinks sufficient have been made to satisfy him, that the above residuum contains a new metal, which must have come from the sulphur employed in manufacturing the acid.

The appearances characteristic of the new metallic substance, are produced by pouring on the residuum, after it has been thoroughly edulcorated with water, a little caustic alkaline ley. Immediately after the addition of the first drops, the white colour becomes yellow, which with heat assumes a golden tinge. He poured muriatic acid on the inspissated heated mass, so as speedily to dissolve it; he then diluted and filtered the solution, and poured in sulphuretted hydrogen water, which occasioned a precipitate of yellow-brown flocks, of the hydro-sulphuret of the metal. After these were separated, the liquid yielded, with ammonia, a hydro-sulphuret of iron in flocks, which speedily became dark green.

The metallic substance is susceptible, he says, of different degrees of oxidation; and becomes first yellow, then passes into a lively brown, bluish or dark grey; and it finally changes to white. At a certain stage of oxidizement it exhibits more the properties of an acid, than of a base; as it unites in this case easily with caustic alcali; but not with acids, such as the sulphuric and muriatic. The alcaline combinations of this new *metallic oxide* readily form double salts with acids; those with the above acids, are all crystallizable, and when dissolved by the aid of heat, yield, with ammonia, a precipitate of the metallic oxide.

These saline triple or quadruple combinations, do not always afford with sulphuretted hydrogen, a brownish-yellow precipitate; but occasionally give one of a bluish tinge, which communicates its colour to the whole liquid. The hydro-sulphurets of this metal, readily take fire with heat, and become white. A simple voltaic pair, consisting of zinc and platinum, throws down this new body from its solution, bluish-grey and with a perfect metallic lustre. The metal deposited at the platinum wire, dissolves in dilute nitric acid, with disengagement of gas. We must confess that the above account, containing the substance of the Professor's details, seems to us very inconclusive; and we should not be surprised to find that persulphate of iron is concerned in these phenomena.

The notice which was inserted in the *Journal of Science and the Arts*, vol. ii. p. 385, relative to the transudation of melted tin through cast iron, was slightly incorrect. M. Clement, the author of the experiment, had formed the cylinder of a pump, out of a copper tube, which being rather feeble, he wished to strengthen, by an outer case of cast iron. The interval between the two cylinders being filled with melted tin, this metal transuded on the *outer* surface of the cast iron, in the cotton-like efflorescence, which burned at the flame of a candle. This experiment shews the porosity of the cast iron employed, which we suspect must have been of inferior quality, or ill-founded.

IV. INORGANIC CHEMICAL COMPOUNDS.—The French chemists and artists have been much occupied of late in repeating and varying the experiments on the alloys of steel, which were made in our laboratory, and which are recorded in the ninth volume of this *Journal*. M. J. B. Boussingault, has made several, in the laboratory of the school of miners at Sainte-Etienne, particularly on the combination of silicium with platinum, and on the presence of silicium in steel. When platinum was placed in a crucible, lined with a paste composed of a mixture of powdered charcoal, and a little clay, (the *creuset brasqué* of the French chemists) it always fused, in a powerful air furnace, into a button; and the fusion was more easy, when the metal was covered with charcoal. It was observed that the platinum had increased a little in weight. The properties of platinum thus fused, are the following: it has a greyish-white appearance; it is scarcely affected by a knife, and with difficulty by a file; and has a specific gravity of 20.5. In the cold it yields a little under the hammer; but it presently cracks, and presents a granular fracture. Forged at a cherry-red, it crumbles in pieces; at a very dull red it flattens at first slightly, and then breaks. It is not softened in its temper at the blast of a forge; cemented with oxide of manganese for an hour, to remove its supposed carbon, it remained intractable. During the solution of the above platinum, no trace of charcoal was perceived; but when the process was somewhat advanced, a transparent jelly was observed to cover the fragments of metal, and to render its solution very difficult. After long action of the acid, with frequent agitation, the platinum was at last dissolved, and a white powder remained, which turned out to be silica, which M. Boussingault believes to have existed in a state of silicium, in the metal. He thinks it more than probable that this silicium comes from the wood-charcoal employed; for on burning it, the ashes yielded a notable quantity of silica, and from another experiment he satisfied himself that the silica was not derived from the crucible. Perhaps the silica, he says, may be in the deoxidated state in the charcoal. 5 grammes of pure platinum afforded 5.025 of the siliceous compound, 1 gramme of which yielded 0.010 of silica, being double the increase of weight on 1 gramme of platinum; thus indicating the conversion of 0.005 of silicium into 0.01 of silica, agreeably to Berzelius's proportions.

M. Boussingault next examined different varieties of French steel, as also a specimen from Monkland near Glasgow, made from Dannemora Swedish iron. His process consisted in dissolving the steel in sulphuric acid, diluted with six times its weight of water. The residuum, being well dried, and weighed, is then burned, and he infers the carbon from the loss. He concurs in opinion with Mr. Daniell, that these residuums, as well as those of cast iron, contain silicium; possibly in the state of a persilicate of iron, as the carbon is in the state of plumbago. It is to be remarked, that the residuums of the carbonaceous steels take fire in the platinum crucible before it is red hot; sometimes indeed the hand can endure the heat at which inflammation happens. What remained after the combustion, was digested with dilute muriatic acid, which dissolved the metallic oxides, and left the silica sensibly pure: it was calcined, and weighed while hot. In this process the manner of estimating the carbon was far from rigorous; but the chief object was the silica. The following is a table of the results;

	Iron.	Carbon.	Silicium.	Manganese & Copper.
Iron of Rive	99.825	traces	0.175	traces
Steel of cementation	99.325	0.450	0.225	traces
Cast steel	99.442	0.333	0.225	traces
Monkland Steel *Acier poule*	99.375	0.500	0.125	traces

It would appear from these results, that in the cementation, at the same time that the iron combines with the carbon, it absorbs also a small quantity of silicium. If this fact, which at present is somewhat doubtful, were established by a great number of analyses of iron, before and after cementation, we could conceive the utility of certain substances which are added to the charcoal in the chests of cementation*. By exposing a pure iron, which contained no silica, to a melting heat, in contact with lime and clay, he obtained a portion of the steel of Clouet, which consisted, by analysis, of iron 99.2 silicium 0.8. From 100 parts, 1.6 parts of silica were obtained. There was no carbon in it. This steel has the property of hardening with water in the usual way, but it is not trusted to by workmen. Ten grammes of slender iron nails (which yielded no residuum after solution in sulphuric) were fused in a Hessian crucible. The brilliant melted button was more difficult to hammer and to file than the iron employed. It consisted, in 100 parts of iron 99.46, silicium 0.54; which last yielded 1.08 of silica.

We cannot therefore judge of the degree of fusion of iron when this operation is conducted in a Hessian crucible, since it seems to be sufficiently demonstrated, that at a high temperature, the iron reduces the silica, combines with the silicium, and forms a compound more fusible than the metal. The same thing happens to platinum, when it finds silicium ready reduced; and if it does not fuse in a Hessian crucible like iron, it is because from its slight affinity for oxygen. it cannot, like iron, decompose silica. In a *creuset brasqué,* steel melts first, then silicated platinum, and lastly manganese, in the same state of combination with the other metals. M. Boussingault infers that pure manganese is more difficult of fusion, than pure platinum.

On some Combinations of Gold, by M. J. Javal†. The object of this paper is to prove by experiment, that M. Pelletier's late notions concerning the equivalent number and combinations of gold are erroneous. M. Javal shews by varied researches on the oxide of gold, obtained by adding barytes to the chloride of that metal, that it consists of 11.909 oxygen united to 100 of metallic gold; a result which approaches nearly to 12.077, that obtained by Berzelius, by decomposing the neutral muriate of gold with mercury, and estimating from the quantity of gold obtained, of mercury which supplanted it, and rate of oxidation of mercury, the rate of oxidation of gold. M. Oberkampf had shewn that the sulphuret of gold obtained by precipitating this metal from the muriate, by sulphuretted hydrogen, consisted of 24.39 sulphur + 100 gold. Now $\frac{24.39}{2} = 12.195$, is very near the preceding number.

The yellow crystals which are obtained by evaporating the

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* There is nothing added at Monkland to the charcoal; it may however contain a little silica.

† *Ann. de Chim. et de Phys.* Vol. xvii. p. 337.

acid solution of gold treated with potash, was considered by M. Pelletier as a mere mixture of the two chlorides. By evaporating a mixture of the muriate of gold and muriate of potash, M. Javal obtained crystals of a fine golden yellow, having the form of elongated quadrangular prisms. These crystals did not lose their colour by washing; on the contrary, they continue to possess the colouring power in a very high degree. M. Javal afterwards analyzed these crystals, and found them composed as follows:

Chloride of potassium	24.26	1 atom by Berzelius	25.21
——— of gold . .	68.64	2 atoms	68.71
Water	7.10	2 atoms	6.08
	100.00		100.00

Mr. Grouvelle, (not Granville, as misprinted in the last Number of our Journal, p. 174), has published in the *Annales de Chimie et de Physique* for last August, a paper on some compounds of chromium, and on several combinations, in which one of the elements enters only in a very small proportion. He says that bichromate of potash, when calcined strongly, melts, and passes to the state of neutral chromate, giving up the half of its acid which is decomposed, and leaves a crystallized oxide of chromium in spangles of a magnificent green colour. The neutral chromate obtained was analyzed by a solution of sulphurous acid, which converts it instantly into sulphate of potash, sulphate and sulphite of chronium. The metallic oxide was precipitated by ammonia, and the sulphate of potash evaporated.

M. Grouvelle says, that Vauquelin, by pouring sulphuric acid and potash into chromic acid, obtained a brown precipitate, which he regarded as an oxide of chromium, with more oxygen than the green oxide. It is not, says M. Grouvelle, an oxide, but a carbonate of chromium. Surely then it must have been a carbonate of potash which M. Vauquelin employed. It dissolves, he adds, with effervescence in dilute acids. When boiled in distilled water, it is decomposed, and we obtain green oxide, and carbonic acid. Consequently, we must avoid washing it with hot water.

We can also obtain this carbonate of chromium, by evaporating to dryness, a mixture of nitrate of ammonia, chromate, and carbonate of potash, or of muriate of ammonia, and an alkaline nitrate, carbonate, and chromate. The matter dried at a gentle heat blackens. It is to be re-dissolved in water, and a drop of water of ammonia is to be poured in, to separate a small quantity of carbonate of chromium, which, he believes, the nitrate of ammonia had rendered soluble. If it be too strongly heated, the excess of nitrate would re-produce some chromate. Here it is the protoxide of azote in the nascent state which decomposes the chromic acid; once gaseous, it has no longer this power. The mixture of nitre and sal-ammoniac acts as nitrate of ammonia, because a double decomposition is effected, in virtue of the facility with which the nitrate of ammonia is converted into gas. This double decomposition always happens when we heat, along with sal-ammoniac, the nitrate of a metal susceptible of forming a fixed chloride. In order to obtain the protoxide of azote, we may therefore use, instead of nitrate of ammonia, a mixture of nitre and sal-ammoniac, in the proportion of 3 of the former to 1 of the latter, leaving an excess of nitre, to prevent sublimation of the sal-ammoniac. By Dr. Wollaston's scale, 3 of nitre are equivalent to 1.58 of sal-ammoniac; so that M. Grouvelle's excess seems superfluously great; 2 of nitre to 1 of sal-ammoniac would be good proportions, and, to secure intimate mixture, they should be dissolved together in hot-water.

M. Grouvelle, in a short section on the chromites, offers nothing very definite or satisfactory. In treating of chromate of lead, he says, we may obtain a reddish chromate, by employing an alcaline chromate of potash; or, if we make use of a subacetate of lead, and a neutral chromate, boiling both together, a yellow precipitate is formed, which passes in a few seconds into a very brilliant orange-red. We may procure a still deeper tint, by boiling a little alcali with the red, or even with the yellow chromate of lead. He has analyzed, comparatively, the yellow chromate, the red, and the red ore of Siberia. All give exactly the same ratio between the acid and the oxide. They are neutral chromates, only the red chromate contains a small quantity of alcali, which appears to him about 1 or 1½ per cent. A red chromate may be obtained, by boiling together chromate of potash and litharge. From some experiments he infers, that the alcali is combined with the oxide of lead, and that this combination, united to the chromate of lead, gives rise to the red chromate, which thus contains a little more oxide than the neutral chromate. A few drops of dilute nitric acid deprive it immediately of its red colour, by dissolving the alcali and a little oxide of lead. He has found in the Siberian ore a little lime, but he is not sure whether it may not have been accidental. The above circumstances explain the fact known to painters, that the yellow chromate of lead mixed up with whiting, for painting walls, produces as great a body of colour as the same weight of the deep orange chromate.

M. Grouvelle precipitated magnesia, by caustic soda purified with alcohol, washed the precipitate till the water no longer reddened turmeric paper, treated it with nitric acid, calcined strongly and repeatedly the compound, then washed the magnesia, and obtained, finally, a nitrite which, with sulphuric acid, gave well-pronounced crystals of sulphate of soda. With potash the same result was obtained. We must take care to treat the nitrite by nitric acid, since being very alcaline it would not fuse on burning coals.

If we evaporate the washings of magnesia when the water ceases to be alcaline, reduced to about one-quarter it deposits a very notable quantity of magnesia, and then reddens turmeric paper. It is probable that the water decomposes the neutral combination formed, at the moment of precipitation, into a soluble compound, with excess of alcali, and an insoluble compound, with excess of base. The only means, therefore, of estimating exactly the quantity of magnesia which a solution contains, is to precipitate by phosphate of ammonia.

Magnesia, treated with barytes, presents the same phenomena, but they are easier to ascertain. M. G. acted on the compound of magnesia and barytes with a quantity of nitric acid, insufficient to dissolve the whole. The re-agents indicated both magnesia and barytes in the part dissolved, as well as in the part undissolved, which proves that there was really a combination, for otherwise the barytes would have been the first united with the acid. About 4 per cent. of barytes is thus associated with the magnesia. In like manner, oxide of copper retains a small portion of the potash, or barytes, employed to throw it down from its acid solutions.

In treating of the principles of combination at the commencement of this article, we promised to return to one of Berzelius's speculations. We shall now bestow a word or two on it. He is investigating the combinations of chromium. "Not having at my disposal," says he, "any metallic chromium, I have endeavoured to deduce the composition of its green oxide from its capacity for the acids. I dissolved the hydrate of chromium in muriatic acid; I evaporated this solution to dryness, and heated the muriate to a temperature sufficiently high to expel every trace of muriate of ammonia which might be present. The muriate thus heated appeared under the form of a red, pulverulent, and bulky mass; it dissolved very slowly, but without residuum in water. The solution was precipitated by ammonia, added in so small an excess that the liquid only re-acted feebly; it was thereafter digested with this excess to remove the muriatic acid precipitated with the oxide, in the form of a submuriate. The oxide thus obtained weighed 3.05 grains. The liquid, neutralized with nitric acid, and precipitated by nitrate of silver, produced 15.61 grains of muriate of silver. The experiment has then given, for 100 parts of muriatic acid 102.3 parts of oxide of chromium, which makes 28.5 parts of oxygen in 100 parts of green oxide. If the acid contains the metal combined with twice as much oxygen as in the green oxide, this last must contain 29.89 hundredths of its weight of oxygen; if, on the other hand, the ratio of the oxygen of the oxide were to that of the acid as 2 to 5, the oxide ought to contain only 20 parts of oxygen. If, finally, the oxygen of the oxide were to that of the acid in the ratio of 3 to 5, its composition would recede equally from the result obtained by experiment*."

As Dr. Wollaston's scale coincides for muriate of silver with Berzelius's atomic numbers, we shall employ it in the examination of the above passage; 15.61 muriate of silver contain 2.98 of what Berzelius still reckons muriatic acid, which were previously combined with the 3.05 of oxide of chromium. And 2.98 : 34.12: :3.05: : 34.92, a number representing, by Berzelius's experiments, the equivalent of oxide of chromium, on Dr. Wollaston's scale. And 2.98 : 3.05: : 100 : 102.3, precisely as M. Berzelius has stated. He adds, "which makes 28.5 parts of oxygen in 100 parts of green oxide;" hence, by proportion, in 3.05 parts 0.869, and in 3.49, or (to bring the atom to round numbers), in 3.5 parts, almost exactly 1 of oxygen. Thus the atom of chromium would appear to be 2.5, that of the green oxide 3.5, and that of the acid 5.5, the latter number of which is quite discordant with that deduced from his experiment on chromate of lead†, which gave 213.924 oxide of lead to 100 of acid; whence the latter becomes on the equivalent scale 6.54. One of the canons of combination laid down by Berzelius is, that 100 parts of muriatic acid neutralize a quantity of base, which contains 29.4 of oxygen, and from this canon his above calculation seems to be made. Now, this proportion is precisely what we find by sliding 100 on Dr. Wollaston's scale opposite to dry muriatic acid, for 29.4 is then opposite to oxygen. In like manner, by sliding 100 opposite to each other body on that instrument we may make as many canons as we please, but they all merge in the general system of equivalents. Dr. Thomson, in his account of the atomic theory‡, says, "According to Berzelius in order to saturate 100 parts of muriatic acid, a metal must be combined with 42 parts of oxygen." We shall not attempt to unravel this atomic knot. While we entertain the highest respect towards the Swedish chemist for his unwearied and valuable researches, we cannot help thinking his manner of viewing chemical proportions to be unaccountably perplexed and defective. We can more easily understand why his partial mode of notation should be preferred by many writers in France to the universal system, as taught by Mr. Dalton, and so clearly exhibited on Dr. Wollaston's scale, for the same reason that vaccination was so ill received in that country,—because it was discovered by an Englishman.

We are lately indebted to Professor Berzelius also, for a

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* *Annales de Chimie et de Physique.* Tom. xvii. p. 10.

† *Ibid.* Tom. xvii. p. 7. ‡ *Annals of Phil.* ii. 39.

pretty long memoir, on the mode of analyzing the ores of nickel, and on a new combination of nickel with arsenic and sulphur. We shall reserve an account of this for our next Number, in order to connect it with some more extended observations on analysis, than we have room for in the present. Mr. Robiquet's observations on the memoir of M. Berzelius, relative to the composition of the triple prussiates or hydrocyanates, concerns a subject too intricate and important to be lightly passed over. We shall, therefore, bring it under review in the next Number.

V. ORGANIC COMPOUNDS.—Under this head we shall consider all chemical combinations, which directly or indirectly result from vegetable and animal organization.

M. Dive, apothecary, of Mont-de-Marsan, has formed prussiate of potash by calcining in a covered crucible a mixture of 64 grammes of the dry powder of crude tartar, and 8 grammes of pulverized sal ammoniac. He considers that the nascent carbon of the tartar, being presented to the nascent ammonia, disengaged from the muriate by the potash, acts so as to form cyanogen; and that this action is favoured by the temperature, which weakens the combination of the hydrogen and azote in the ammonia. The latter element being, in every point of the mass, in immediate contact with particles of carbon, easily unites to it, in the requisite proportions for forming cyanogen, which is immediately fixed by the potash. The same gentleman finds that a current of carbonic acid partially decomposes the neutral tartrate of potash; and he ascribes to this cause the formation of the bitartrate in the juice of the grape during its fermentation. Accordingly, on mixing neutral tartrate with fermentable materials, he found cream of tartar in the fermented liquor*.

In a report made to the Institute, 15th January last, by M. M. Thenard and Berthollet, on M. Chevreul's 8th memoir on Fat Bodies, we find the following results of their decomposition by ignited oxide of copper.

1. That the fat of man and the sow contain nearly the same proportions of elements; that mutton suet contains more carbon and hydrogen; and that in the three fats the carbon is to the hydrogen, by volume, nearly as 10 to 18; which approaches to the constitution of percarburetted hydrogen.

2. That the stearines contain less oxygen, and more carbon and hydrogen than the claïnes; and that the ratio of the carbon to the hydrogen in the stearines is 10 to 18, whilst it is a little less in the claïnes.

3. That the sum of the weight of the saponified fat, and the

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* *Journ. de Pharm.* Oct. 1821. p. 487.

sweet principle, which are the result of saponification of different species of fats, is greater than the weight of the fat employed. If we recollect that the saponification takes place *in vacuo*, without any other substance than the fat, the potash, and the water, and that there is no disengagement of hydrogen and oxygen, we must necessarily admit the fixation of water in one of the products of the saponification. Comparative tables of all the products and of their elements, whether in weight or in volume, establish these consequences; and the author observes, that, in the acidified fats, the hydrogen is to the carbon in the ratio of the elements of olefiant gas. When we heat gently with massicot (yellow oxide of lead) the margaric and oleic acids, and consequently the acidified fats which are formed from these acids, there is disengaged a quantity of water, which, as the author shews, is formed of the combination of the hydrogen of these acids with the oxygen of the massicot, or which is disengaged simply by the combination of the massicot with the dry acids. The author adopts the latter opinion. He exhibits, in comparative tables, the elements of natural fats, and the products of their saponification, both of the margaric and oleic acid. There results from their comparison:

1. That in the three margaric acids which he has examined, the carbon is to the hydrogen perceptibly in the proportion of the elements of olefiant gas.

2. That the oxygen of the margaric acid of the sheep, is to the oxygen of the margaric acids of man and the hog, nearly as 1 to $1\frac{1}{2}$. From this observation the author proposes to name the margaric acid of mutton suet, *margarous* acid.

3. That in the oleic acids of man and the hog, there is more oxygen than in the margaric acid; and that in that of sheep there is more than in *margarous* acid.

It is remarkable that the composition of oleic acid is represented by olefiant gas + oxide of carbon.

When potash or any other alcaline base acts on the fats which have been examined, the greater part of their carbon and hydrogen, in a ratio very near to that of olefiant gas, retains a portion of oxygen to constitute the margaric and oleic acids, whilst the rest of the elements of the fat, that is to say, of the carbon, hydrogen, and a quantity of oxygen, which seems less by one-half than what would be necessary to burn the hydrogen, form the sweet principle, by fixing probably a portion of water.

The elementary analysis of cholesterine shews, by its excess of carbon, the reason why this compound produces no margaric acid, when it is exposed to the action of alcalies.

At page 389, vol. x. of this *Journal*, we have given some account of the chemical researches of M. M. Pelletier and Caventou, on cinchonine and quinine. But the mode of obtaining these new vegetable bases was omitted; we shall now lay it before our readers.

Two kilogrammes (about 4½ pounds avoird.) of pale cinchona, bruised, were acted on by 6 kilogrammes (about 13½ libs.) of strong alcohol. This operation was repeated four times. The alcoholic tinctures were united, and distilled, to withdraw the alcohol. Care was taken to add 2 kilogrammes of distilled water, in order that the matter dissolved in the alcohol should be protected from the immediate action of the heat, after the separation of the alcohol. This substance, received on a filter, which allowed the aqueous liquid to pass through, was of a reddish colour, and a resinous aspect. In this state it was washed on the filter with water rendered slightly alcaline by potash. The liquor which had passed through the filter served for the first washing, after having been previously alcalized. After several days of edulcoration, the alcaline liquids passing limpid and colourless, the matter left on the filter was washed with a pretty considerable body of distilled water. The substance was then of a greenish-white, very fusible, soluble in alcohol, and crystallizable. It was the cinchonin of Dr. Gomez. It possessed in this state some of the characters of resinous substances; but, on dissolving it in an acid very much diluted with water, it deposited a considerable quantity of a fat matter of a green colour, which had all the characters of the green fat matter obtained for the first time by M. Lauber, by the direct action of ether on cinchona. We may remark, that if too strong an acid be employed, a great quantity of the fat matter would remain in solution in the liquor, and the cinchonine, which we should subsequently obtain, would be polluted with it.

The acid liquor (dilute muriatic acid was employed) was of a golden-yellow. Evaporated, it yielded crystals soluble in alcohol, and in water. Its taste was very bitter. It fell down readily with alcaline solutions; the gallates and alcaline oxalates occasioned precipitates soluble in alcohol. The above solution was treated with pure magnesia at a moderate heat. The mixture, after thorough cooling, was thrown upon a filter, and the magnesian precipitate was washed with water. The first washings were yellow; but they soon became colourless. The magnesian precipitate, sufficiently washed and dried at the *balneum mariæ,* was treated three several times with alcohol of 40° (0.817). The alcoholic liquors, very bitter and slightly yellowish, gave by evaporation crystals in needles, of a dirty white. These crystals, re-dissolved in alcohol, and made again to crystallize, gave a very white and brilliant crystalline matter. We may also obtain very white crystals by washing them in the cold with a little sulphuric ether. These crystals are pure cinchonine.

Quinine is obtained from yellow Peruvian bark, by employing the different processes indicated for the extraction of cinchonine. In case of a natural or artificial mixture of cinchonine and quinine, crystallization and ether may serve to separate these two bodies. Quinine is very soluble in ether, and does not crystallize *.

M. J. Voreton, of Grenoble, has given the following as an improved process †. He takes 5 kilogrammes of yellow bark of good quality, reduced to coarse powder. These are infused in water acidulated with about one hundredth of its weight of muriatic acid. After macerating 24 hours, he expresses strongly, and treats the matter with new acidulated water till the bark gives out no more bitterness. He then filters, and treats the united infusions with an excess of pure magnesia. This mixture is boiled an instant, and then allowed to cool. The remainder of the process is exactly conformable to that already known. Thus we filter, wash the magnesian precipitate with cold water, dry it, and treat it with alcohol. We obtain the quinine by distilling off the first portions of the alcohol, and evaporating the rest at a very low temperature.

By this process, more simple than that formerly published, since we avoid the preparation of the alcoholic extract, he has obtained from 5 kilogrammes of bark about 70 grammes of quinine, instead of 45 or 50, which was all he could obtain by the former process. This arises, he believes, from the muriatic acid dissolving more easily all the quinine, when this is not enveloped by the fat or resinous matter of the alcoholic extract.

But the most economical and productive process is that of M. Henry, fils ‡. A kilogramme of yellow bark, (cinchona cordifolia) reduced into a pretty fine powder, was acted on twice with heat, by a dilute sulphuric acid, consisting of 50 or 60 grammes diluted with 8 kilogrammes of water for each time. The filtered decoctions were very bitter, and had a reddish colour, which assumed on cooling a yellowish tint. To discolour (blanch) these liquors, and saturate the acid, he employed pulverized quick-lime instead of the more expensive magnesia. The liquors, entirely deprived of colour, were passed through a cloth, and the precipitate which formed, was washed with a small quantity of water, to separate the excess of lime. The deposit (on the cloth,) well drained and almost completely deprived of moisture for twelve hours, after having been put three successive times to digest in alcohol of 36° (0.837), furnished by distilling off the alcoholic liquid, a brown viscid matter, becoming brittle on cooling. It was acted on with water sharpened with sulphuric acid, and the refrigerated liquor afforded nearly 32 grammes of white

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* *Annales de Chimie et de Phys.* xv. p. 289.

† *Ibid.* xvii. p. 439.

‡ *Journ. de Pharm.* July 1821. p. 296.

silky crystals, entirely soluble in alcohol, scarcely soluble in cold water, but more in boiling water, particularly if this was slightly acidulated. They consist of pure sulphate of quinina. Messrs. Pelletier and Caventou, while they approve of the above process, consider the product as overrated, probably from M. Henry not having sufficiently dried the sulphate obtained. The pure sulphate of cinchonina ought to be brilliant, crystallized in parallelipeds, very hard, and of a glassy white; the sulphate of quinina is on the contrary of a dull white, silky and flexible; both should be soluble in alcohol and should burn without leaving any residuum*.

In the *Journal de Pharmacie* for last September, M. Baup, of Vevai, describes the crystallized sulphate and supersulphate of quinina, as consisting, the first of—

Quinina	1 atom	45
Sulphuric acid	1 —	5
Water	4 —	4.5
		54.5

The supersulphate consists of—

Quinina	1 atom	45
Acid	2 —	10
Water	16 —	18
		73

The crystalline form of cinchonina is a rhomboidal prism, of 108° and 72°, terminated by a bevelment.

M.M. Pinel, Thenard, and Hallé, in a report recently made to the Academy of Sciences, on a Memoir of M. Choulant, entitled, " Observations on the employment of the Sulphates of Quinina and Cinchonia in Intermittent Fevers," give the following conclusions: The number of cases reported by M. Choulant is 14; in 10 of them the cessation of the fever was due to the sulphate of quinina, which occurred immediately after either the first or the second dose; and, in the latter case, the paroxysm that followed the first was considerably milder; the effectual doses varied from 6 to 12 grains. The reporters also state, that M. Double has administered the sulphate of quinina in tertian and quartan agues, with immediate and complete success, and almost always after the first doses. The small bulk of the medicine renders its administration easy in many cases in which the bark in substance would be inadmissible. The red or yellow bark seems to yield the most febrifuge quinina.

VI. ANALYSIS AND APPARATUS.—In the last Number of

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* *Journ de Pharm.* July, 1821. p. 305.

our Journal, p. 169, we have described M. Berthier's mode of analyzing minerals which contain alcaline matter. The following Table of results seems important:

Constituents.	Adularia.	Petrosilex.	Pumice.	Domite.	Domite.	Vitreous Lava.
Silica . . .	0.6420	0.7520	0.7000	0.6550	0.6100	0.6440
Alumina . .	0.1840	0.1500	0.1600	0.2000	0.1920	0.1564
Potash . . .	0.1695	0.0340	0.0650	0.0910	0.1150	0.0540
Lime . . .	trace	0.0120	0.0250	0.0220	. . .	0.0120
Magnesia . .	. . .	0.0240	. . .	. . .	0.0160	0.0120
Oxide of iron .	. . .	. . .	0.0050	0.0300	0.0420	0.0430
Water . . .	. . .	0.0150	0.0300	. . .	0.0200	0.0710
	0.9955	0.9870	0.9850	0.9980	0.9950	0.9924

The domites from Puy de Dome, and the vitreous lava, have been called feld-spar lavas.

1. *Analysis of the crystallized Calamine of Limbourgh* (Electric oxide of Zinc, Haüy,) by Professor Berzelius. (From Schweigger's Journal for last December.)

Mr. Smithson first distinguished several varieties of calamine which have since formed a single species. That now under consideration was by him found to consist of, silica, 25; oxide of zinc, 68.3; and water, 4.2=97.5.

The silica and oxide are in the equivalent proportions, but the water is in this respect anomalous. With a view of settling this point, M. Berzelius undertook a careful analysis of the minerals. On redissolving in ammonia, the precipitate by subcarbonate of soda, from the sulphuric acid solution, he separated completely the oxide of zinc from the lead, and found of the latter 0.003 parts. The following is his result:

Silica	24.9		26.23
Oxide of zinc . .	66.84	or	66.37
Water	7.46		7.40
Carbonic acid . .	0.45		100.00
Oxide of lead . .	0.28		
	99.93		

Representing this compound by the formula $2\ zn.S+aq.$, and subtracting the carbonate of zinc.

2. *Analysis of an arsenical pyrites, from Zinnwald*, by Dr. du Menil; specific gravity, 6.064; constituents, iron 42.44, arsenic 52.12, sulphur 2.4.

3. A variety of *malacolite* from Norway (sahlite,) sp. gr. 3.1, analyzed by Count Trolle, gave silica 57.4, lime 23.1, magnesia 16.74, loss 2.76, in 100 parts. He considers it as represented by the formula $CS^2 + MS^2$; that is, a bi-silicate of lime associated with a bi-silicate of magnesia.

4. *Analyses of some northern minerals*, by C. d'Ohsson, Swedish ambassador at the Hague. They were undertaken at the invitation of Berzelius, and verified by him.

Garnet of Broddbo, sp. gr. 4.25. It fuses into a black globule before the blow-pipe. It consists of silica 39, alumina 14.3, protoxide of iron 15.44, protoxide of manganese 27.9, tungstate of zinc 0.5, silicate of zinc 0.5, loss 2.36 in 100. He represents its atomic constitution, on Berzelius's plan, by $FS^2 + 2MgS + 2AS$.; that is, 1 atom of bi-silicate of iron, +2 atoms silicate of manganese, +2 atoms silicate of alumina.

Malacolite of Bjornmyresveden, sp. gr. 3.331. It consists of silica 57.28, lime 24.88, magnesia 9.12, protoxide of iron 6.04, protoxide of manganese 0.72, loss 1.96; or, $MS^2 + 2CS^3$, viewing the iron and manganese as accidental associates.

A new mineral from Pargas, called *Chondrodite* (granular,) colour wine-yellow, occurs in grains about the size of a pin-head, fracture small conchoidal, lustre between waxy and vitreous, scratches glass, sp. gr. 3.18. Dissolves in neither nitric nor muriatic acid; infusible before the blow-pipe, but becomes darker coloured; with soda it fuses into a bright green slag. With borax it readily melts, with intumescence, into a bright greenish-yellow glass. Its constituents are, silica 38.0, magnesia 54, oxide of iron 5.1, alumina 1.5, potash 0.86, manganese a trace, loss 0.54. Viewing the iron and the latter articles as unessential, M. d'Ohsson thinks its constitution may be represented by the simple formula MS, a silicate of magnesia. It occurs near Pargas, in Finland, interspersed in granular limestone.—Schweigger's *Journal*, Dec. 1820.

Examination of a meteoric stone which fell in Courland, in the circle of Dünaberg, on the 12th July, 1820, by Theodore Von Grotthus. Between five and six o'clock in the evening, a fire-ball apparently as large as the full moon, was seen moving slowly from south to north. It seemed to burn with a reddish flame, which also followed it, like the train of a comet, leaving in the distance spiral clouds, which moved very slowly, and then melted in air. After this meteor had described in the heavens an arch of nearly 100°, it became extinguished. In less than a minute afterwards there resounded from the region where the fire-ball had vanished, a noise resembling, first, three rapid discharges of great guns; then a fire of musketry; and finally, a continuous rolling like thunder peals. At the same time, a stone fell from the air, 3½ German miles from the country palace of

Lixna, about fifty paces from two husbandmen, who were working in the field, and who were greatly astonished. About four wersts distant, in the presence of six labourers who were mowing hay on the banks of the Kolupschen lake, there fell at the same time, with a frightful hissing sound, a large body into the water dashing its spray several fathoms into the air. Also at a third place, three wersts in the opposite direction, there was observed to fall from the air into the river Dubna, something which rendered its waters muddy for nearly an hour. The stone which fell in presence of the two husbandmen, penetrated a foot and a half into a dense, dry, clayey loam. It was so hot that the men when they tried to touch it, after recovering from their surprise, burned their hands. There was also a smell of gunpowder diffused round it. Its shape, when entire, resembled a rounded anvil, of which the narrow end was undermost. It weighed altogether about forty pounds. Its external characters are described by M. Grotthus, and resemble those of other meteoric stones. Its composition, according to his elaborate analysis, is as follows : Iron 26, nickel 2, sulphur 3.5, (which bodies he regards as forming 31.5 of a sulphuret of iron and nickel), silica 33.2, protoxide of iron 22, magnesia 10.8, alumina 1.3, metallic chromium 6.7, lime 0.5, manganese a trace. His mode of analysis is somewhat peculiar, and we shall perhaps advert to it in our next Number. He thinks the proportions of chromium and manganese are very difficult to determine, when present in small quantity, unless we remove the greater part of the iron by muriatic acid, before fusing the powder with caustic potash. In this way also the mass yields more readily to the alcali. He believes the chromium to have existed in this meteorolite in the metallic state, because had it been oxidized, it would have dissolved in the muriatic acid.

The following mode of analyzing copper pyrites by Professor Dobereiner seems ingenious. He transmits over it a stream of chlorine, dried by passing previously over muriate of lime. The pyrites is entirely decomposed, and chlorides of sulphur, iron, and copper result. On heating this mixture with a spirit lamp, the first chloride distils over into a separate vessel, the second sublimes in brilliant flakes into the upper part of the retort, or matrass, and the cupreous proto-chloride remains at the bottom in the form of a cinnamon-brown semi-fused mass*. There is much resemblance between this method and Berzelius's for analyzing the ores of nickel, of which an account will be given in our next Number. We were surprised however to perceive M. Dobereiner describing, as a new contrivance of his own, the pneumato-mercurial apparatus, so long ago invented by Mr. Pepys ; with which he and Mr. Allen performed their celebrated experiments on carbonic acid and respiration.

Meteorolite in France.—On the 15th of June last, about three

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* Gilbert's *Annalen* for 1821, Part 4.

o'clock P.M., while the sky was serene, a very considerable globe of fire was seen in the air, which seemed to descend rapidly in a straight line. Immediately thereafter a loud rattling noise and a detonation were heard, and a stone weighing 92 kilogrammes (203 libs. avoird.) fell in the Commune of Juvenas, department of Ardèche. According to the report of a villager its appearance was announced by two strong explosions, like two discharges of a cannon, and followed by a rumbling noise, which spread consternation among the inhabitants. The stone had sunk 18 decimetres into the ground, that is, nearly 6 feet. It is also said, that some smaller stones fell on the occasion. The fragment of the great one, which was sent to the Academy of Sciences, has the usual external characters of meteorolites.

II. Applications of Chemistry.—Under quinina we have already pointed out the medical applications of this substance. M. Şexullas in a memoir, of which there is a copious abstract in the *Journal de Pharmacie* for September, shews, that all the antimonial preparations used in medicine, except carefully crystallized tartar emetic, contain more or less arsenic, which originally combined with the antimony in the ore, continues pertinaciously associated with it through all its modifications. He also proves in the same paper, that a very powerful pyrophorus is obtained, by treating tartar emetic in the same way as the mixture of alum and flour is treated for making the pyrophorus of Homberg. The curious details on this subject are reserved for our next *Journal*.

Of the severity of the French *police Pharmaco-legale,* we may judge, when M. L—, a respectable apothecary of Verdun has been recently fined 3,000 francs, for selling sulphuric acid to a woman, who poisoned herself with it.

The experiments of M. Magendie having shewn that the salt, extracted long ago from opium by Derosnes, and which is improperly called narcotine, produces a stupor differing from real sleep, and acts on dogs as a poison in small doses, M. Robiquet conceived that he might render opium a more soothing medicine by depriving it of this irritating and pernicious principle. Accordingly M. Robiquet has prepared an extract of opium on good chemical principles, which has produced happy effects. He macerates the opium, cut into small pieces, in water, as if to obtain the aqueous extract; he filters and evaporates to the consistence of a thick syrup, and treats this extract with ether, agitating very frequently in a convenient vessel. He decants the ethereous tincture. This, when once separated, is submitted to distillation to recover the ether. He repeats the operation as long as he obtains crystals of *narcotine*. When the ether has no longer any action on the extract, he evaporates the solution, and the opium is prepared. As the same ether may be employed to prepare a new extract of opium, the operation is by no means so costly as would at first sight appear.

Dyeing. "Simple and easy method of clearing from their tawny pigment, dyeing infusions made with Brazil woods of inferior quality, and of substituting them with success, for true Fernambuco, by Dr. Dingler, manufacturing chemist at Augsbourg." The watery infusions of these poorer dye woods are to be evaporated, till from 4 kilogrammes of wood there remain only 12 or 15 of liquid. When this liquid is cooled, we must pour into it, after 12 or 18 hours, 2 kilogrammes of skimmed milk. After stirring this mixture well, we boil it for a few minutes, then pass it through a piece of thick flannel. The tawny colour will be then seen to attach itself to the caseous part of the milk, which spontaneously precipitates from this decoction, without occasioning the least loss in the quantity of red colour. The remaining red is of a pure tint. Perhaps this process might be useful with madders, which consist of the same two colours. Two kilogrammes of milk are sufficient for 6 or 8 kilogrammes of young woods *.

Paste for dressing Webs.—It is well known that weavers are obliged to work in damp shops, to prevent the dressing of the web from drying and hardening. M. Dubue has read lately a Memoir before the Academy of Sciences of Rouen, on the subject of pastes, &c., in which he shews that a very minute addition of muriate of lime to them, renders them so retentive or absorbent of moisture, that webs dressed with such pastes, may be wove in the upper and drier chambers of a house, as well as in the lower and ill-aired with the usual dressing. The plan is undoubtedly judicious. Muriate of lime may be had at a very trifling expense from those apothecaries who prepare water of ammonia. The waste whitening steep of the bleacher is merely a solution of muriate of lime.

Agriculture.—Professor Gazzeri, of Florence, has lately published an Essay on Manures, and their most useful mode of employment in Agriculture. To the English farmer, acquainted with the agricultural practice of Mr. Coke, and the Treatise of Sir H. Davy, the above work offers little novelty; but its precepts must be very useful in Italy; and, indeed, his experiments seem so judicious, as to deserve a brief notice in this place. "I shall demonstrate," says he, "both by reasoning and facts, that the previous fermentation of manures is not well fitted to make their substances enter into plants during vegetation, if that fermentation takes place at a distance from the soil which the manures are meant to fertilize; but that, on the contrary, the application of manures in their entire chemical state is advantageous, especially if they have been subjected

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* *Journal Polytechnique d'Augsbourg.*

to mechanical division, (such as chopping of the straw, &c.) To my great surprise, experiments have convinced me of two things; first, that the loss of useful matter suffered during fermentation is much greater than I could have conceived; and, secondly, that if the solubility of the residuum be ever so little increased, the quality of that residuum is still more deteriorated." He placed in a copper pot a mass of horse-dung, weighing 40½ libs, (of 12 ounces each), which he surrounded with coarse cloth, and a large body of straw, to screen it from the action of air and light; and it was placed in a close and covered situation, circumstances all unfavourable to fermentation. It consisted then of

Water	70.37
Fibrous matter	15.17
Attenuated stercoraceous matter	11.12
Soluble parts	2.34
	100.00

At the end of two months it had lost 9½ pounds of its weight. Its proportional composition was now, water 68, fibres 16, attenuated organic matter 11, soluble matter 4. At the end of another month it had lost 3 pounds more. Some time after this, at the beginning of July, he opened the pot, to give the manure free access to the air, and on the 18th of that month, being 4 from the commencement, its weight had diminished to 22 libs. 3 ounces. The proportion of its constituents, was, however, much the same. Thus, though placed in circumstances the most favourable to its preservation, it had lost in four months more than one half of its substance. He found that the richer animalized matter was that soonest dissipated. Thus the manures lose their azote, an element essential to the growth of wheat, as it forms a constituent of its gluten, without which panary fermentation does not take place. M. de Saussure's experiments prove that these grains do not derive their azote from the atmosphere. His experiments on the dung of fowls, cows, *&c.* offer similar results. The decomposition of cow dung was found to be much slower than that of horses. He was surprised to find its decomposition accelerated by being mixed with straw. But the freer access of air in this case will account for the difference. In great dung hills, the author says, that decomposition goes on more rapidly, and is more wasteful. The attenuation of the fibrous portion is thus very dearly bought.

Another result of the author's experiments which we think interesting is, that fresh vegetable matter, such as chopped straw, decomposes more rapidly into soluble manure, when mixed with soil in which plants are growing, than when left in soil in which no vegetation is going on. It also disappears sooner, being dissolved by the living powers of the vegetables, and carried into their circulation.

We were not a little amused with the contrast in sense and science, between the work of the Italian Professor, and an Official Report, signed by Count Dubois, Yvart, and Hericart de Thury, Members of the Commission of Manures of Paris, made to the Royal and Central Society of Agriculture, on a new Manure proposed under the name of the Alkalino-Vegetable *Poudrettes.* These gallant gentlemen extol one of their countrywomen, Madame Vibert Dubout, patentee of the Poudrettes, for her discovery and indefatigable practice of the following operations :—" After allowing the consistent ordure to subside, she draws off the urinous liquid into a basin, and leaves it in repose for fifteen or twenty days, to allow time for the alkaline and saline elaborations to be naturally formed. She then slakes or diffuses quicklime in the urines, in the proportion of a fifth part of the whole. At this moment the lady feels herself exposed to an infectious and insupportable odour, from the escape of various gases. At the end of eight days the slaked lime forms a fair or yellowish paste, soft and soapy to the touch, which diffuses the odour of violets. This paste is removed, spread on a spacious and well-aired area, covered with a layer thrice its thickness of the consistent ordure, which had been meanwhile dried and drained in the upper basins. On these two primary beds others are formed in alternate succession, taking care to leave from space to space layers of the thick matter in mutual contact, in order to facilitate their fermentation. In a shorter or longer time, according to the state of the weather, there arises in the mass a fermentation, more or less active, which rises, becomes puffy, and speedily confounds the different strata of the alternate matters. When the fermentation has ceased the great masses must be cut down, intimately mixed together, piled up anew, and left to become thoroughly dry. Finally, at the end of a certain time, a mixture absolutely inodorous is obtained, which is to be reduced into powder, by the ordinary processes, and which forms the new patent *Poudrette* of *Madame Vibert Dubout.*" We shall not stop to enumerate the marvellous virtues ascribed to this spoiled manure, by the learned commissioners. That a lady should contrive, execute, and describe so abominable a process, is to us in England not a little surprising; but that an operation so unscientific, destructive, and absurd, should be recently held forth to admiration, in the *Annales de l'Agriculture Française*, is curious in the extreme, and must give the world an odd opinion of French husbandry.

III. Physics. In our next Number we shall endeavour to present our readers with a view of the discussions on Light, which now occupy so keenly the attention of some leading members of the Academy of Sciences.

Magnetism. Coulomb determined the law of magnetic attractions to be inversely as the square of the distance, by making a magnetic needle vibrate, at different distances from the magnetic pole of a bar, and counting at each station, the number of vibrations. Professor Hansteen of Christiana* has lately applied the same method, to discover if the absolute magnetic force of the earth be an uniform or variable quantity, for any particular place on its surface. He suspends a magnetized cylindric rod, 2 inches and $\frac{3}{4}$ long, and $\frac{3}{40}$ of an inch diameter, by a single thread of a silk cocoon, depending from the top of a brass tube, fixed in the centre of the lid of a brass box. On either side of this vertical tube, the lid of the box is formed of glass, to permit the graduated arcs at the bottom to be seen. This mode of suspension has also, we observe, been adopted by M. Arago, for his very delicate needle mounted on the Royal Observatory of Paris; and, it is considered by him as infinitely superior to the suspension on a point, whose form is liable continually to change by the effect of friction. Professor Hansteen sets his needle in movement by presenting to one of its poles a piece of iron, which is immediately withdrawn. He then counts, by a chronometer, the period of every ten vibrations up to 360; and he compares each equal portion of the whole to find the mean period. Thus he takes the difference between the end of the first, and the end of the 300th; the end of the second, and that of the 310th; and so on, till that of the 60th and the 360th. Did not the resistance of the air gradually diminish the arc of the vibrations (till towards the close, it becomes only two degrees), the above measures would be equal; but in consequence of this resistance, the last measure of the time is found to be about $\frac{8}{10}$ of a second less than the first. The longest period of 300 oscillations observed by M. Hansteen was 813.6″; this time he considers as corresponding to his minimum or zero of magnetic force. In fact, this observation was made during an aurora borealis. But the force of the magnetic power is inversely proportional to the squares of the times of similar vibrations of the same magnetic needle; and, if T T' be in two experiments, the observed times of equal vibrations of the same needle, and if I I' denote the corresponding forces of the magnetic power; then $I : I' :: T'^2 ; T^2$; and thus $I : 1 :: 813.6'' ; T^2$, hence in this case

$$I = \left(\frac{813.6}{T}\right)^2$$

From numerous observations of the above kind, which the

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* In a letter to Mr. Rumker lately of Hamburg, of which an abstract is given by Professor Gilbert, in his Annals for last July.

professor made during the course of the year, he has calculated the mean monthly intensity of the magnetic force. The following is a table of the results:

Hour of Observation.	December 1819.	March 1820.	Hour of Observation.	April 1820.	May 1820.
8 M.	1.01931	1.01095	8 M.	1.00717	1.00582
10	.01902	.01010	10½	.00625	.00548
12	.01915	.01023	4 A.	.00879	.00849
2		.01136	7	.00966	.00844
4	0.01916	.01147	10½	1.00903	1.00740
6	0.01929	.01163	Mean	1.00818	1.00713
9		.01142			
10	0.01732	.01063			
Mean	0.01912	1.01081			

Hour of Observation.	June 1820.	July 1820.	August 1820.	Sept. 1820.	October 1820.
8 M.	1.00407	1.00277	1.03309	1.00560	1.00886
10½	397	235	335	508	800
4 A.	647	461	543	708	909
7	700	500	570	711	953
10½	1.00665	1.00548	1.00555	1.00715	1.00953
Mean	1.00563	1.00404	1.00468	1.00640	1.00900

From these observations it follows; first, that these is a daily variation in the strength of the magnetic power; the *minimum* occurring between 10 to 11 A.M, the *maximum* between 4 and 5 P.M. Second, that the magnetic power is subject also to a yearly variation in its magnitude, as appears from the mean of each month; its amount is manifestly greater in winter, while the sun is in perigee, than in the opposite season; in December and June the greatest monthly variations also take place; and the *minimum* variations occur in the two months of spring and autumn, when the sun is at his mean distance from the earth. The greatest daily variation of the magnetic power is in summer; the least in winter. The maximum difference of the annual variation is 0.0359.

If we denote by F the total magnetic power, by H that portion of this power which acts on a horizontal needle, and by n the magnetic inclination, then $\mathrm{H} = \mathrm{F} +^{2} \cos.\ n$. Hence H may be variable, when the whole magnetic force F is constant, provided the magnetic inclination be variable. But M. Hansteen imagines, from researches which he made in 1820, with a dipping needle by Dollond, that he found the inclination to be in summer about 15′ greater than in winter; and from 4′ to 5′ greater in the forenoon than in the afternoon; which would accord with the foregoing observations *.

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* On this subject M. Arago judiciously observes, that from Mr. Gilpin's observations, whose accuracy is acknowledged by philosophers, there appears to be no appreciable diurnal variation in the dip, and its monthly

During an aurora borealis, he made a series of observations from noon to noon without intermission, which evidently shewed that this meteor displays an enfeebling operation on the magnet; proving in this respect, the connexion between electricity and magnetism *. From analogous researches he has been led to infer, that the magnetic intensity is always impaired when the moon passes through the equator.

During a residence of a few days last year in Copenhagen, M. Hansteen lodged in the round tower, which serves as an observatory. As he was repeating here the above-described observations on the vibrations of his horizontal needle, he found, to his great astonishment, that for 300 vibrations not less than 836.57″ were required: and in a garden contiguous to the tower, only 779″ for the same number of oscillations. This tower is 126 feet high, with very thick walls, and has, built in its centre or axis, a hollow cylinder, round which the stair winds in 7 convolutions. After he had made several observations below, which shewed that the mean period of 360 vibrations was 787″, he returned to the tower, and found the following duration of 300 oscillations; first, aloft on the tower; then at regular stages of descent to the bottom:

Top.	1st platform	3.	4½.	6½.	bottom
842.37	836.57	837.3	834.43	804.07	813.0

Not a little astonished at these results, he continued his observations, and deduced at last the general result, that at the under end of every perpendicular body the magnetic needle oscillates quicker when it is placed northward of the body, or when it presents its south pole to it; but that it oscillates more slowly when placed southward of the body, so as to present its north pole to it. And inversely, it was found, that, at the upper extremity of the vertical body, the needle vibrated always slower on the north side, and quicker on the south side. He thence concludes, that every perpendicular object, of whatsoever matter composed, possesses at its upper end a magnetic south pole, and at its under end a magnetic north pole. This result is of such importance, that we hope some of our philosophers and astronomical observers, accustomed to minute precision of research, will verify these experiments of the learned Dane.

From a comparison of Col. Beaufoy's observations on the magnetic declination needle, M. Arago has shewn, that, since the month of April, 1819, the direction of the movements of the needle

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variations are unfavourable to such an annual change as the Norwegian Professor indicates.

* Baron Humboldt made a similar remark some years ago at Berlin; but unless the observations of the horizontal needle be combined with simultaneous observations of the dipping-needle, the variation of intensity cannot be held to be demonstrated.

has become retrograde; the mean annual amount of which is $= -1' 57''$. The total retrocession between 1818 and 1820, by a comparison of the morning observations, is 3′ 22″
noon .... 4′ 22″
evening.............. 4′ 0″

The Board of Longitude of Paris has established lately, at the Observatory, a magnetic needle, exclusively consecrated to indicate the diurnal variations of declinations. Some derangement, from an unknown cause, having happened to it in 1819, it was re-mounted last February, since which time its march has been regular. The observations made with it indicate already a retrograde movement of the needle towards the east. The *mean declination* of the month of February, 1821, is smaller by 2′ 15″ than that of the month of February, 1820.—*Ann. de Chim. et de Phys.* xvi. p. 54.

We reserve several electro-magnetic notices for next quarter.—We intended to have given M. Moll's account of Colonel Offerhaus's apparatus, published in the *Journal de Physique* for April last, in the form of a letter to the editor; but, as we find that the same letter is re-printed, with the date of June 22d, in the *Edin. Phil. Journal* of October last, we are saved the trouble of transcription.

IV. Under the science of MECHANICS, the most prominent paper is that of M. P. S. Girard, on the uniform discharge of atmospherical air and carburetted hydrogen gas, through conducting pipes. We have no room for an analysis of it at present.

V. NATURAL HISTORY.—Baron Humboldt, in a memoir read to the Institute 19th February last, 1821, entitled, "New Observations on the Laws which we observe in the Distribution of Vegetable Forms," states, that we already know nearly 56,000 species of cryptogamous and phanerogamous *plants*, 44,000 insects, 2,500 fishes, 700 reptiles, 4,000 birds, and 500 species of mammiferæ. In Europe alone, according to the researches of M. Humboldt and M. Valenciennes, there exist nearly 80 mammiferæ, 400 birds, and 30 reptiles. There are, of consequence, under this temperate boreal zone, 5 times as many species of birds as of mammiferæ; as, in like manner, there are in Europe 5 times as many compositæ as amentaceous and coniferous plants; 5 times as many leguminous as there are of orchideous and euphorbiaceous. The fine collections recently brought home from the Cape of Good Hope by M. Delalande prove, (if we compare them with the works of M. M. Temmink and Levaillant,) that in that part of the temperate austral zone, the mammiferæ are also to the birds in the proportion of 1 to 4.3. Such an accordance between two opposite zones is very striking. The birds, and especially the reptiles, increase much more towards the equatorial zone than the mammiferæ. According to the discoveries of M. Cuvier on fossil bones, we might believe, that these proportions have not been the same at all times; and that there have disappeared, in the ancient catastrophes of our planet, many more mammiferæ than birds. We can conceive how, on a given space of territory, the individuals belonging to different tribes of plants and animals may be *numerically limited;* how, after an obstinate struggle and long oscillations, a state of equilibrium comes to be established, resulting from the necessities of nourishment and the habits of life: but the causes which have *limited the forms* are hid under an impenetrable veil, which withdraws from our view whatever relates to the origin of things, or to the first developement of organic life.

On the preponderance of certain families of plants depends the character of the landscape; the aspect of a smiling or majestic nature. The abundance of gramineæ which form vast savannahs, that of palms and coniferæ, have had a powerful influence on the social conditon of nations, on their manners, and the more or less rapid developement of the useful arts. Sometimes a single species of plants, especially among those styled, by M. Humboldt, *social,* covers a vast extent of country. Such are, in the north, the heaths, and forests of pines; in equinoctial America, the union of cactus, croton, bambusa, and brathys of the same species.—*The sequel of this will be given in our next Number.*

GEOLOGY.—Mr. Fox having communicated to the editors of the *Annales de Chimie et de Physique,* in a manuscript letter, new determinations on the temperature of the earth at various depths, these gentlemen have published them, along with an extract made by M. Fourier, from his profound geometrical researches on heat.

The observations were made in ten different mines; *viz.,*—Dolcoath, United Mines, Treskerby, Whealsquire, Ting-Tang, Wheal-Gorland, and Wheal-Damsel (copper-mines); Chase-Water (mine of tin and copper); Wheal-Unity (in the tin part of the mine), and Wheal-Vor (tin-mine). The following is a list of the temperatures observed. The numbers inscribed on the table are the means of particular results obtained for each depth in the *ten* mines above-named.

At the depth of 10 fathoms......	Temp. 50.18° F.—	10.1 C.
20 to 30..............	60.98	16.1
30 to 40..............	59.54	15.3
50 to 60..............	62.06	16.7
60 to 70..............	63.86	17.7
70 to 80..............	65.84	18.8

At the depth of 80 to 90	68.36° F.—	20.2 C.
90 to 100	69.80	21.0
100 to 110	68.54	20.3
110 to 120	69.98	21.1
120 to 130	69.62	20.9
130 to 140	72.14	22.3
150 to 160	75.02	23.9
190 to 200	75.92	24.4
at 230	78.44	25.8
at 240	82.04	27.8

In a great number of cases, the thermometer was sunk in the rock to the depth of 6 or 8 inches; sometimes the temperature of the air or water was merely taken; generally both methods afforded results slightly differing from each other. The thermometer, plunged in the metallic veins, usually indicated a temperature of 1° to 2.8° centigrades (1.8° to 5° F.) higher than that obtained when the thermometer was plunged into the hole of a rock, and particularly in granite. The veins of tin are commonly a little colder than the veins of copper. In the bottom of the mine of Dolcoath, at 240 fathoms of depth, there issues from the vein a very abundant jet of water, whose *constant* temperature is 27.8° C. (82.04° F.) What more evident proof can be given, says Mr. Fox, of the great heat of the interior strata of the globe? The draining-pumps of the *United Mines* were not long ago totally deranged by an accident; two galleries, one at 200, and the other at 190 fathoms in depth, were found eventually filled with water. This state of things lasted two entire days. As soon as the water was pumped out, and before the workmen descended to resume their labours, Mr. Fox ascertained the temperature of the two galleries; that of the first was 31.1° C. (87.98° F.) The thermometer in the second, at a depth of 190 fathoms, continued stationary at 30.8° C. (87.44° F.) It ought to be remarked that, in order to avoid all error in the observation of these temperatures, the thermometer had its bulb plunged several inches under the floor of the galleries. Mr. Fox analyzed the waters of *Dolcoath*, and those of the *United Mines*, whose temperatures are so high, and he found in the first only a small quantity of muriate of lime, and in the others a proportion equally small of the sulphates of iron and of lime. Mr. Fox adds, that new experiments made in coal-mines confirm perfectly the results obtained in the galleries of the mines of copper and tin. Thus in a colliery,

At 10 fathoms depth, the temperature was	50° F.	10° C.
36 . . .	57.92 —	14.4 —
90 . . .	62.06 —	16.7 —

Of M. Fourier's speculations on the above curious facts, we shall give some account in the next Number.

The following extract from a letter of Count Mercate, describing the earthquake in the Isle of Zante, 29 Dec. 1820, may perhaps be found interesting among geological phenomena: "Towards midnight," says he, "I heard a hollow and interrupted noise, which appeared to issue from the bosom of the earth. This noise resembled the sound of a drum, beat from time to time in a subterraneous vault. It was heard by the greater number of persons who were awake at the time. We passed the night in a state of horror, and at ten minutes before four o'clock in the morning a sudden blast of wind, of an extraordinary violence, made us imagine the end of the world had arrived. This wind instantly subsided into a dead calm. Presaging the impending misfortune, I experienced an inward indescribable horror. In this melancholy mood I had thrown myself into bed, when I felt struck all at once by a horrible subterranean bellowing, announcing the commotion of the earth, which immediately ensued. I instantly rose up, but the violence of the shocks made me fall back on the bed. These concussions were threefold; the first, of great violence, was vertical; the second produced an undulatory movement; and the third, which was the most powerful, gave a rotatory motion. The most solidly-built houses could not resist the violence of these earthquakes. Eighty were entirely overturned, nearly eight hundred were horribly shattered, and the others so damaged as to be uninhabitable without being repaired. A confused and sudden noise of shouts and crying announced the universal alarm of the population, who thought their last hour was come. But in the midst of so many disasters, it is remarkable that only four persons were killed, and a few others wounded by the ruins." He speaks in very warm terms of the liberal charity of Lord Strangford, ambassador to the Porte, who was then in the harbour, as well as of the Lord High Commissioner, Sir P. Ross.

Three or four minutes before the first shock there was seen, at the distance of two leagues from the promontory of Geraca, to the south-east of the island, a fiery meteor, almost swimming on the sea, which remained lighted for five or six minutes. The following day there was a meteor, which blazed forth at four o'clock in the evening, and describing in the air a vast parabola from east to west, fell into the sea.

METEOROLOGY. See *Meteorolites*, under ANALYSIS.—M. Vogel of Munich, while spending some days on the banks of the Baltic, was told that different invalids, labouring under asthmas, &c., found themselves much better on sea than on shore. He then examined the air in different situations, and found that the sea air a league from shore, when admitted into an exhausted globe containing barytes water, did not trouble it, whereas the air, admitted into a similar globe on the shore, produced immediate turbidity. Nitrate of silver in solution, exposed to sea air, had some chloride formed in it. Hence he infers that the atmosphere of the Baltic, taken at a league from the shore, contains less carbonic acid than the ordinary atmosphere, and that it is probable the quantity of carbonic acid diminishes as we recede from the land. 2d, That the atmosphere of the Baltic contains muriates in greater or less quantity *.

VI. ECONOMICS.—In pulling down lately the vestry wall of a chapel, near the Lago Maggiore, which had been built more than 300 years ago, as appears by good documents, there was discovered, imbedded in the mortar of the wall, three eggs, which were found to be fresh. M. Cadet, one of the editors of the *Journal de Pharmacie,* after relating this fact, states that naturalists bring from America and India birds'-eggs, covered with a film of wax, which, after removing the wax with alcohol, may be hatched. He then talks of a man who sold eggs at the public market in Paris, which had been preserved upwards of a year in a peculiar composition. A slight layer of carbonate of lime observed on these eggs induced M. Cadet to suspect that lime-water was the preservative composition. He afterwards made experiments on this point, under direction of the Council of Salubrity of Paris, and succeeded in keeping eggs perfectly sound during nine months and ten days, the period of the experiments. We believe this means of preserving eggs has been long known to housekeepers in this country, but it is less practised than it deserves to be. If every farmer would cause the eggs of his poultry to be put into a cask of lime-water the moment they were laid, the inhabitants of London might enjoy better breakfasts than they do at present.

Uninflammable Clothes.—M. Gay-Lussac announced in the sitting of the Academy of Sciences, 6th Nov. 1820, that linen dipped in a solution of phosphate of ammonia became incombustible. MM. Merat-Guillot, father and son, apothecaries at Auxerres, have since shewn that the acidulous phosphate of lime possesses the same property. In fact, linen, muslin, wood, paper, straw, impregnated with a solution of this salt at 30° or 35° of concentration (1.26 to 1.30), and dried, became absolutely uninflammable, and consequently unfit to communicate fire. They carbonize, or char, when they are exposed to a very intense flame, but the carbonization does not extend beyond the focus of heat in which they are plunged.

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* *Journ. de Pharm.* Oct. 1821, p. 461.

ART. XIV. ANALYSIS OF SCIENTIFIC BOOKS.

i. *Fumifugium; or, the Inconvenience of the Aer and Smoake of London dissipated. Together with some Remedies, humbly proposed, by* JOHN EVELYN, Esq., *to his sacred Majestie, and to the Parliament now assembled. London,* 1661.

As we are not restricted to the notice of modern scientific books, we have selected the above scarce tract for the purpose of comparing the grievances occasioned by the smoake of London 160 years ago, when the metropolis was not one-sixth its present extent, with those which are now matter of complaint; and of inquiring how far the evil was then, and is now, susceptible of diminution, or removal.

It is curious enough that Mr. Evelyn's attention was called to the subject before us by "a presumptuous smoke issuing from one or two tunnels neer Northumberland-house, and not far from Scotland-yard," the very seat, if we are not misinformed, of the plots of our modern fumifugists; he therefore prepared the "short discourse" before us, for the reformation of this nuisance, and in the hope of rendering "London one of the sweetest and most delicious habitations in the world, and this with little or no expense;" being amazed, he says, "that where there is so great an affluence of all things which may render the people of this vast city the most happy upon earth, the sordid and accursed avarice of some few particular persons should be suffered to prejudice the health and felicity of so many."

It is also not a little remarkable, that although Mr. Evelyn's *Fumifugium* was written before the great fire of London, many of the evils and nuisances, to which he adverts, are still extant, and in full force: a few of them, though perhaps not the most pressing, either have been remedied, or are likely to be amended. This glorious and ancient city still wraps her stately head "in clowds of smoake and sulphur, full of stink and darknesse," most of our streets are still "narrow and incommodious in the very centre, and busiest places of intercourse," and we still have to deplore the "frequent wharfes and magazines of wood, coal, boards, and other coarse materials, most of them imploying the places of the noblest aspect for the situation of palaces towards the goodly river:" at the same time we need now no longer complain, "of our streets being composed of a congestion of misshapen and extravagant houses; of the ill and uneasy form of the paving under foot," nor of "the troublesome and malicious disposure of the spouts and gutters overhead."

After adverting in the first part of this essay to the transcendent situation of London, "built upon a sweet and most agreable eminency of ground at the north side of a goodly and well-conditioned river, towards which it hath an aspect by a gentle and easie declivity;" to the advantage of the gravelly soil, and to the plentiful and rich supply of water, every where good and abundant, our author proceeds to point out the origin and effects of that "hellish and dismall cloud of sea coale, which is not only perpetually imminent over her head *(conditur in tenebris altum caligine cælum)*, but so universally mixed with the otherwise wholesome and excellent air, that her inhabitants breathe nothing but an impure and thick mist, accompanied with a fuliginous and filthy vapour, which renders them obnoxious to a thousand inconveniences, corrupting their lungs, and disordering the entire habit of their bodies." This is a strong but not exaggerated picture of the state of this monstrous grievance in the year 1661; and, when we reflect upon the gradual increase of the evil up to the present period, we cannot but hail the recent enactments of the legislature directed towards the consumption of smoke, wishing that the subject may receive that serious attention which it so urgently requires. We regret, however, that experience justifies a doubt as to the efficacy of mere parliamentary interference in a matter of this kind; whilst novelty recommends it, and the reports of Committees serve as a vehicle for the publication of individual schemes, all goes on with eagerness and energy; but as soon as parliament is prorogued, and the smoke-burners out of town, we relapse into our pristine fuliginosity, and the pretty-behaved chimneys upon the river-side, which awhile seemed to have forgotten their office, again evolve their wonted columns of sable smoke, "belching it forth," as Evelyn says, "from their sooty jaws, and making the city of London more like the suburbs of hell, than an assembly of rational creatures: so that the traveller, at many miles' distance, sooner smells than sees the place to which he repairs." We might quote, as a parallel instance, the Bill for the removal of nuisances from the streets, which for a time was sufficiently effective, and contributed greatly to the comfort of the metropolitan pedestrian; lately, however, the beggars have resumed their ancient stations, the pavement is as heretofore decorated with the praises of our radical patriots, written, as they should be, "all upside down;" barrow-women vociferate the contents of their moveable magazines in the most audacious defiance of street-keepers, constables, and acts of parliament; hackney-coachmen quit their boxes, and carmen mount their drays with unreserved impunity; the White Horse cellar continues, as heretofore, the eternal and undisturbed resort of stage-coachmen, jews, and pickpockets; Carlile's shop remains open for the edification and instruction of the young of both sexes; stalls and other peccant excrescencies are again shooting up at street corners; and, in short, every thing shews, that unless the orders of the legislature are enforced by a respectable and well-directed police, not active by fits and starts, but permanent and regular in its operations, all parliamentary interference in these matters is utterly ineffectual, and even mischievous, whenever laws are infringed with impunity.

It appears, to return to our subject, that in Mr. Evelyn's time, brewers, dyers, lime-burners, and salt and soap-boilers, were the principal nuisances; and since then, says the editor of the new edition of the *Fumifugium*, published in 1772, "we have a great increase of glass-houses, founderies, and sugar-bakers, to add to the black catalogue, at the head of which must be placed the fire-engines of the water-works at London-bridge and York-buildings, which leave the astonished spectator at a loss to determine whether they do not tend to poison and destroy more of the inhabitants by their smoke and stench, than they supply with water." To this sooty list, what astonishing additions have been made within the last thirty years, in and about London—how many new water companies, and smoke-producing manufactories have been added to the catalogue. A newspaper cannot now be printed, nor a pound of meat minced for sausages, without a steam-engine: to the same smoky servant the druggist resorts to grind his rhubarb, and sift magnesia; and upon all possible occasions the service of the other elements is superseded by that of fire. The natural consequence is, that the smoke of London, always grievous, is now scarcely tolerable: to select a few of Mr. Evelyn's *miseries*, "it obscures our churches, makes our palaces look old, fouls our clothes, and corrupts the waters, so that even the rain and dew are contaminated. It is this which scatters and strews about those black and smutty atoms upon all things where it comes, insinuating itself into our most secret cabinets and precious repositories; yea, though a chamber be never so closely locked up, men find, at their return, all things that are in it covered with a black soot, and all the furniture as full of it as if it were in the house of some miller, or a baker's shop, where the flour gets into their cupboards and boxes, though never so close and accurately shut." "Finally, it is this which diffuses and spreads a yellownesse upon our choicest pictures and hangings; which does mischief at home, is *avernus* to fowl, and kills our bees and flowers abroad, suffering nothing in our gardens to bud, display themselves, or ripen." "Not therefore to be forgotten," continues our author, after some further grumblings, "is that which was by many observed; that in the year 1644, when Newcastle was besieged and blocked up in our late wars, so as through the great dearth and scarcity of coals these fumous works were either left off or diminished, divers gardens and orchards, planted even in the very heart of London (as, in particular, my Lord Marquis of Hertford's, in the Strand; my Lord Bridgewater's, and some others about Barbican), were observed to beare such plentiful and infinite quantities of fruits, as they never produced the like either before or since, to their great astonishment; but it was, by the owners, rightly imputed to the penury of coales, and the little smoke which they took notice to infest them that yeare."

It would be to no purpose further to accompany our author through this region of practical grievances; we shall, therefore, pass on to that part of the tract relating more especially to the influence of London smoke upon the health of its inhabitants. It has sometimes been argued that the fuliginous atmosphere of London is rather salutary than otherwise, and that, at all events, it tends to counteract contagion, and disinfect the air; it has also been contended, that the mortality of its inhabitants has not sustained an increase proportionate to their augmented numbers. We believe it must be admitted, that the town has not become less healthy within the last fifty years, but we doubt whether it is more so, which, considering the enormous supplies of water which inundate every district, the widening of several streets, the removal of many of those nests of filth and poverty which disgraced some of the more prominent parts of London, and, above all, the general amelioration of the sewers and drainage, ought indisputably to have been the case. There are so many contingent circumstances that interfere with our estimate of the mortality of the inhabitants of large cities, that it is very difficult to form a just conclusion as to the effects of individual causes; yet, viewing, the subject in this light, we cannot, however willing, ascribe any serious mischief to the mere smoke; let us, for instance, look at the mortality of Paris, which equals, or even exceeds, that of London, yet its inhabitants enjoy a most sweet and delightful air; the sky is clear and serene; the foggy obfuscations and jaundiced composts of soot and vapour which adumbrate London are there unknown, and our vision extends nearly at all times and seasons from one end of that splendid metropolis to the other; whereas here we consider it wondrous clear when we can discern the Monument from Blackfriars-bridge, or see St. Paul's Cross from the bottom of Ludgate-hill. If indeed we adopt the maxim, *non est vivere, sed valere, vita,* we must then concede all to the inhabitants of Paris; for who has not felt the vivifying influence of their undisturbed and transparent atmosphere, in which every object has a sharp and cleanly outline, and figured to himself the magnificence of London, were it blessed with such a climate. To the circumstance of breathing an air thus pure and uncontaminated, some of the peculiarities of the English at Paris may possibly be referred; and the celebrity of the French *cuisine* among our countrymen is not perhaps altogether independent of such a cause. "I report myself to all those, " says Evelyn, " who have been compelled to breathe the air of other countries for some years, if they do not now perceive a manifest alteration in their *appetite* and clearnesse of their spirits, especially such as have lived long in France and the city of Paris." But Evelyn attributes more to the smoke than can well be substantiated, and consequently often ascribes effects to its absence, which are referable to other causes. "Although," says he, "London is tolerably free from the plague, it is never clear of smoke, which is a plague so many other ways, and indeed intolerable, because it kills not at once but always, since still to languish is even worse than death itself. For is there, under heaven, such coughing and snuffling to be heard as in the London churches and assemblies of people, where the barking and spitting is incessant and most importunate. What shall I say? *hinc hominum pecudumque lues.*" He then goes on to inform us, that the cause of these effects is the inhalation of this "infernal vapour," which irritates the windpipe, together with those multiform and curious muscles, the immediate and proper instruments of the voice, which becoming rough and dry, can neither be contracted nor dilated for its due modulation, so as by some of my friends studious in music, it has been constantly observed that, coming out of the country into London, they lost three whole notes in the compass of their voice, which they never again recovered till their retreat."

If therefore we consider all these evils, and "what a fuliginous crust is yearly contracted and adheres to the sides of our ordinary chimnies, and then imagine if there were a canopy over London what a mass of soot would stick to it which now comes down into the streets, houses, and waters, and is taken into our bodies," it is certainly somewhat surprising that the means of prevention have not been more attentively studied; what these are, and to what extent the proposals for the diminution of smoke and of the various nuisances dependant upon it, have been, or are likely to be carried, we may now proceed to examine.

Mr. Evelyn's plan, which once might have been feasible, is at present out of the question. It consisted in the removal of all nuisance-involving trades from London "five or six miles down the river Thames, or at the least so far as to stand behind that promontory, jutting out and securing Greenwich from the pestilent air of Plumstead Marshes." This is all he has to offer; he then proposes gardens and plantations in and about the metropolis, and enumerates a variety of fragrant plants suited to our climate, and calculated to sweeten and improve the air. It appears probable that the lime trees in St. James's park were planted in consequence of this suggestion.

Among contingent metropolitan nuisances noticed by Mr. Evelyn there are two, which to the disgrace of the present age and in spite of our boasted amelioration and refinement, still exist in full force, namely, burying-grounds and charnel-houses around and under churches, and the "horrid stinks" of our markets; the former, as our author justly observes, independent of the disgusting spectacle of skulls and skeletons which they continually exhibit to the passenger's notice, must tend to contaminate the air; and not the air only, but the water also, for there is a strange custom of tacking a pump to our church-yard walls, the well of which is generally surmounted by heaps of corpses in various states of putrefaction and decay, piled up and kept together by a kind of terrace-wall, which the enormous accumulation of human remains renders a necessary appendage to most of these teeming receptacles of mortality. In respect to our markets, Evelyn's complaints also hold good; the filth of our slaughter-houses and the abominations of Covent-garden, our *marché aux fleurs*, are familiar to every one; but these are matters not now immediately before us, however deserving of that aid which the resources of science have elsewhere lent them; the cemeteries and markets of Paris might especially be resorted to as containing the elements of admirable arrangements.

There is another growing nuisance, to which the following passage from the *Fumifugium* is not inappropriately applicable: we mean the *gas-works*, which have already destroyed the smelts and flounders in the immediate vicinity of London, while the Brentford establishment threatens a similar annihilation of the finny tribes of Isleworth and Richmond, and even promises to interfere with the honest angler's sport as high up the river as Ham and Twickenham: "If," says Evelyn, "we may not hope for so absolute a cure of all that is offensive, at least let such whose works are on the margent of the Thames, and which are, indeed, the most intolerable, be banished further off, and not once dare to approach that silver channel, which glides by her stately palaces, and irrigates her welcome banks."

But it is now time to acquaint our readers with the recent plans and proposals for the destruction of smoke, premising, however, that there is nothing new in them, for they have been canvassed and considered with various ability and success, by almost all our first-rate engineers, at the head of whom we place the late celebrated Mr. Watt, who long ago turned his mind to this subject, and in our opinion, achieved much that has erroneously been given to his contemporaries and successors; indeed, the great engines at the Soho manufactory, have all along been worked without smoke, and we are a little surprised that in the Report, now lying before us, from the committee appointed by the House of Commons, "To inquire how far it may be practicable to compel persons using steam-engines and furnaces in their different works, to erect them in a manner less prejudicial to public health and public comfort," and upon which report the bill of last session was founded, that no notice is taken of Mr.

Watt's suggestions and inquiries. To establish his priority of invention, we shall quote a few lines from the specification of his patent as printed in the *Repertory of Arts* for 1796*. "My newly improved methods of constructing furnaces or fire places, consist in causing the smoke or flame of fresh fuel in its way to the flues or chimneys, to pass, *together with a current of fresh air* through, over, or among fuel which has already ceased to smoke, or which is converted into coke, charcoal, or cinders, and which is intensely hot; by which means the smoke and grosser parts of the flame by coming into close contact with, or by being brought near unto the said intensely hot fuel, and by being *mixed with the current of fresh or unburnt air*, are consumed, or converted into heat, or into pure flame, free from smoke." Mr. Watt's specification then goes on to describe the construction of the fire-place and flues, and continues thus: "My said invention consists only in the method of consuming the smoke and increasing the heat, by causing the smoke and flame of the fresh fuel to pass through very hot funnels, or pipes, or among, through, or near fuel which is intensely hot, and which has ceased to smoke, and by *mixing it with fresh air, when in these circumstances.*"

We do not, however, mean to claim for Mr. Watt the original idea of burning smoke, by causing it to pass through hot fuel, but merely to shew his merit in applying a smoke-consuming apparatus to furnaces of engines, and other fire-places producing large quantities of black and inconvenient vapours. Franklin, as our readers probably know, was a great chimney-doctor, and suggested a similar means of getting rid of smoke in 1785†. But, long before Franklin, namely, in 1682, Mr. Justell read to the Royal Society, "An account of an engine that consumes smoke, shewn lately at St. Germains' fair in Paris‡." This "engine," as it is here called, was merely a chaffing-dish with a descending flue, so that the fumes and smoke of substances burned upon it passed downwards through the ignited fuel and were in that way destroyed. In 1723, too, a Dr. Leutmann of Wirtemburgh, described in his *Vulcanus Famulans,* "a stove which draws downwards." We thrown out these notices merely to shew the little originality of the contrivances of the Marquis de Chabannes, and others who have burned their smoke by a downward draught of air.

In the Parliamentary Report above alluded to, there are two inventions for the destruction of smoke, which appear principally to have occupied the attention of the committee, and which also profess to accomplish that desirable object with a very considerable saving of fuel.

Mr. Brunton is the patentee of one of these inventions; he

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* Vol. IV. p. 226.

† *Memoirs of the Life and Writings of Benjami Franklin,* vol. vi. p. 408.

‡ *Phil. Trans.*

applies to the engine-boiler a newly-constructed fire-place, containing a circular grate, which is made slowly to revolve upon its axis; the fire upon this grate is fed in front by a kind of hopper, continually delivering small coal, which, from the rotary motion of the grate itself, becomes equally spread upon its surface, so as to maintain a thin fire and a sharp draught; the coal is thus rapidly and perfectly decomposed and burned, the smoke at first produced having to pass across the grate and over the red-hot and already coaked fuel. The great advantages of this plan consist in the uniformity of heat, and its proportionate production to the work which the engine has to perform, or to the quantity of steam consumed; the fire-bricks are not injured; the clinkers or scoriæ are produced in thin layers; and the bars are so little heated, that while three bushels of coal per hour are consuming, they are seldom hot enough to singe paper; the boilers are less injured than by a common fire; and there is a considerable saving in the consumption of fuel. The disadvantages of Mr. Brunton's plan are, the expense of the apparatus; the requisite alteration of the boiler, or rather the addition of a supplementary boiler; the necessity of a moving power to effect the rotation of the grate; and the labour of breaking the coals into small pieces before they are put into the hopper. Upon the last point Mr. Brunton, when questioned by the Committee, replies as follows: " No coals should be put on a steam-engine fire until they are small enough to pass a three-inch mesh; therefore, the necessity of breaking the coal to that size is advantageous; but we have lately burned a species of small coal in our own furnace, and also in the town of Birmingham, which has till now been regarded as perfectly useless, and as such there are thousands of tons encumbering the ground in the Staffordshire collieries, incapable of being used with effect in any other furnace, and we have produced with this hitherto supposed rubbish, 70 per cent. of the effect of saleable coal *."

How far Mr. Brunton's invention bears upon the main point of our inquiry, namely, the consumption of smoke, will appear from the following evidence: Mr. James Scott Smith, of the Whitechapel Distillery, says, " We can consume the smoke to a very great extent, and although it is not completely invisible, yet it is never offensive; we never have any of those dark volumes of smoke which are the cause of so much complaint." Mr. Brancker, of Liverpool (a sugar-refiner), also gives evidence as to the great diminution of smoke effected by Mr. Brunton's " fire-regulator;" and both these gentlemen speak in terms the most unequivocal, respecting the saving of fuel,

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* *Minutes of Evidence*, p. 12.

amounting, according to their joint evidence, to from 30 to 38 *per cent.* Upon this subject, however, we greatly apprehend that such evidence is open to unavoidable fallacy. When any thing new is going on in a manufactory, the heads of the concern are generally themselves on the alert, and more than ordinary care and attention are bestowed upon all its details; we are pretty well convinced, and it is indeed obvious, that in any great establishment a monstrous saving of all materials, but of coals more especially, would be effected by the personal superintendence of the master, and of scientific persons well versed in the minutiæ of the concern; yet we are willing to allow, that one great merit of Mr. Brunton's contrivance consists in its being almost independent of the stokers and labourers; there is none of that eternal pitching of coals into the furnace which goes on with such lavish waste in all ordinary engine fires; "the grand principle in this machine," says Mr. Smith, "is, that it makes all stokers alike good, and they always use the same quantity of coals when doing the same work." This is, indeed, a great point carried. We think it but justice to Mr. Brunton to add, that private information derived from various quarters, confirms, to a considerable extent, the extracts we have made from the Minutes of Evidence; we are inclined to consider the saving of fuel quite as important as the consumption of smoke, and in that respect his pretensions seem indisputable; there is also much original ingenuity in his contrivance; indeed, we are not aware that a rotatory grate was ever before either devised or constructed.

Another very effectual, and in some respects, preferable method of consuming smoke, is the invention of Messrs. Parkes, of Warwick. These gentlemen, who are the proprietors of an extensive worsted manufactory, were greatly annoyed by the smoke of their engine-boilers, especially in their bleaching and drying ground; they have now so far effected its consumption that, for about twelve hours of the day, the smoke is nearly invisible, and there is no soot; moreover, these desirable objects are accompanied, they say, by a considerable saving in the article of coals.

In the plan adopted by Messrs. Parkes the boilers remain *in statu quo*; the fire-place is somewhat altered in shape and dimensions, but the principal agent, as far as regards the destruction of the smoke, is a current of air which is admitted just beyond the end of the fire-place, by means of an aperture which may be increased or closed at pleasure, and which they call an *air-valve.* A small fire is first made to burn brightly at the back of the grate; coals are then filled in towards the front, in which direction the fire gradually spreads; their smoke necessarily passes over the clear fire, where it becomes sufficiently heated to constitute flame, as soon as it meets with the current of air entering at the valve; and a striking experiment with this apparatus consists in alternately shutting and opening the air-valve, which is accompanied by the alternate appearance and disappearance of the smoke.

Even from this brief and incomplete view of Messrs. Parkes's contrivance, it is obviously preferable, in some respects, to that of Mr. Brunton: it is much less complex and expensive, and, when properly and assiduously attended to, it effects a more complete destruction of smoke; but, on the other hand, its success is infinitely more dependant upon the persons who manage the fire, and whose dispositions are generally a mixture of ignorance and prejudice, duly tempered with the warmth of the element over which they preside; we also doubt whether the same unequivocal testimony can be adduced in respect to the saving of fuel, for this again is more in the stoker's power. In point of originality, too, Mr. Parkes must undoubtedly yield the palm to Mr. Brunton; he is anticipated in every part of his invention by the words of Mr. Watt's patent; and we rather think that Mr. Gregson, of whose method of constructing chimneys an account will be found in a former Number of this Journal*, has also touched upon some of the most important parts of his contrivance. We, however, by no means state this opinion with a view to detract from Mr. Parkes's merit; on the contrary, if he has attained that in which Mr. Watt failed, the nearer his means approach to those unavailingly employed by his eminent predecessor, the more substantial is the service which he has rendered the public, and the more praiseworthy the perseverance by which he has accomplished his object. We believe that Mr. Watt never affected to combine diminution in the consumption of fuel with the destruction of smoke, and that, on the contrary, with additional expense and trouble, there were more coals burned; at least this is the only cause to which we can refer the non-consumption of smoke, and the non-application of his patent, in the greater number, if not in all, the large engines of his erection which we have had occasion to visit.

One subject seems to us to have been overlooked by the majority of smoke-reformers, namely, the construction, and more especially the height, of the furnace chimneys. By conveying black smoke, and other pernicious fumes into a capacious and very lofty chimney, much of the noxious matters that otherwise escape into the atmosphere are decomposed and precipitated, or condensed within; we were much struck some years ago with the effect of a long flue and lofty chimney attached to the steam-engine of the Dartford Powder-mills; scarcely any smoke issued from its funnel, the fuliginous particles being almost

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* Vol. III. p. 348.

entirely deposited in the chambers of the flue. The chimneys of the Grand Junction engine at Paddington, and of the West Middlesex Water-works at Hammersmith, are more illustrative instances; when these machines are at work, the former produces little smoke; the latter inundates the neighbouring gardens with perpetual showers of the solid soot, and is the greatest of all conceivable nuisances; yet the only difference is in the height of the two chimneys, the boilers being in all respects set and constructed alike. Besides, if a high chimney does void smoke, it is generally wafted away and dissipated, except under particular circumstances of wind and weather.

As to the requisite height of a chimney for the diminution of nuisance from smoke, we are not prepared to give an opinion; it will depend very much on the circumstances of the case; from 150 to 200 feet would, we presume, in most instances, prove effectual; the expense of such a structure may certainly be urged against the proposal, but we are to recollect that one shaft might receive all the tributary fumes of many flues, and that a great saving would be effected in reducing the number of chimneys of medium height. The intolerable nuisance of brewers' chimneys, to whose coppers we fear neither Parkes's nor Brunton's inventions are applicable, would in this way be greatly diminished, if not altogether remedied.

Among other probable causes of the diminution of smoke, we look to the employment of steam as a substitute for fires; in Whitbread's brewery high pressure steam is thus very extensively employed, and, although they make quite smoke enough, it certainly has lately sustained a very perceptible diminution.

Lastly, we come to the most absurd portion of the speculations of theoretical smoke-burners, namely, the improvement of the atmosphere of the metropolis. Though some are sceptical upon the subject, we will admit that, if no smoke were made in London for a twelvemonth, or if wood-fuel were substituted for coals, there might be some amelioration of the atmosphere, although our locality and climate always render it turbid and misty, independent of adventitious effluvia; it is, however, folly to ascribe any sensible influence upon the great mass of London smoke to some few steam-engine chimneys, while every house is busy in the work of contamination, and every street yielding a proportion of filth far exceeding that of any single furnace, though less observed, because administered by separate vents, and in divided doses

When the builders of ordinary houses shall find it worth while to turn their attention to the consumption of smoke, and to display a little more common sense, as well as science, in the mode of warming and ventilating our dwellings; when architects, instead of confining their studies to the dimensions of temples and the measurement of pyramids, shall condescend to consult and visit the chemist and the natural philosopher, and to become acquainted with the doctrines of heat and of pneumatics, then, and not till then, may we expect a diminution of the evils which form the subject of this article of our Journal; and although we are not sanguine enough to anticipate the re-appearance of orchards in the Strand, or vineyards in Barbican, we should then be justified in looking for a degree of relief from those nuisances and evils which smoke inflicts upon the inhabitants of London. C.

ii. *One Thousand Experiments in Chemistry, with illustrations of Natural Phenomena, and practical Observations on the Manufacturing and Chemical Processes at present pursued in the successful cultivation of the useful Arts, with numerous Engravings on Wood and Copper.* By COLIN MACKENZIE. London, printed for Sir Richard Phillips & Co., 1821. Price 21*s*. in boards.

WHEN we consider the manifold and marvellous changes which chemistry effects on natural products, converting the dull and brittle stone into a brilliant metal, to whose adamantine temper the forests yield, and even rocks give way; turning the bland inflammable sulphur into a corrosive acid, which extinguishes flame; extracting from calcined animal bones a substance that takes fire with the heat of the hand, and burns with exceeding splendour; eliminating from the ashes of a wood-fire metallic globules, which kindle and consume by the contact of ice; or evolving from culinary salt, an aërial element, in which refractory metals burn, even without the aid of heat; we can readily imagine that, in rude and remote times, the metallurgist Vulcan might be worshipped as a demi-god; that, in a more advanced state of civilization, but amid literary and scientific darkness, the chemical adept might be thought the elect of heaven, as in Egypt; or the copartner of Satan, as in Gothic Europe; and finally, that in the present period of philosophical light, the genuine chemist should rank high in society. The wonders and uses, rewards and honours, which flow from chemistry, have, however, this unpleasant effect; they tempt multitudes of unhallowed and uninitiated pretenders to rush into the sanctuary of science; and, though they are usually soon and unceremoniously expelled, yet they contrive to persuade the ignorant *profane*, that they possess in their own right mighty secrets, which they have in fact stolen from the very altar of Hermes..

As the English are allowed to surpass all other nations in their funds of cash and credulity, and as the chemical wants and wishes of their manufacturers are numerous and important, it has happened that chemical empiricism has of late begun to rival the illustrious career of Brodum, Solomon, and Eady. One-half, at least, of our chemical factories are committed to the hands of quack-chemists, who, by offers of unlimited profits, dazzle the dark minds of their moneyed proprietors, and supplant men of sound views and principles, who disdain such base methods of delusion. What an instructive book might be made by narrating the senseless schemes and expenditure of our national fire and light companies, of various great printfields, dye-works, glass-houses, sugar-houses, vitriol, alum, and soda works, &c.

We believe ourselves warranted, from an extensive range of observation, in asserting, that a sum of money which would make some figure amid the interest of our national debt, is annually wasted by these crafty and improvident projectors who infest the chemical arts of this country. It has been too much the fashion of late to speak contemptuously of the arts and manufactures of France; but if our ingenious and active neighbours possessed our constitution and our coal-mines, their competition would soon leave the British manufacturer little to boast of, and would compel him to have recourse, more frequently than at present, to the lessons of real science. We beg leave to hold out the following facts, as a slight check on these narrow prejudices, which at once degrade and impoverish their possessors, by fostering notions of superiority on points where they are grossly defective. We also trust that this small *exposé* may induce our operative and consulting chemists to study with a little more industry the scientific principles of their art.

To the manufacturer of sulphuric acid the wholesale price of

	In London.	In Paris.
Sulphur is, per ton	£ 7 7 0	£ 11 4 0
Nitre	27 0 0	76 0 0

and the wholesale price of sulphuric acid in London is 16*l.* 0*s.* 0*d.*
and .... in Paris....12*l.* 0*s.* 0*d.*

If we estimate, for the formation of that acid, 1 part of nitre to 9 of sulphur, though we know some French manufacturers employ only 1 to 10, we have the following proportions:

9 × £ 7 7 = £66 3*s.*		9 × £11 4 = £100 16*s.*
1 × 27 0 = 27 0		1 × 76 0 = 76 0
£93 3	Cost of 10 tons of materials in each country.	£176 16

Thus we see that, at the present prices, the materials in France cost about double what they cost in England; indeed fully so, if we consider the superior quality of our nitre compared with that employed in France; yet the product resulting from these materials is sold 25 *per cent.* cheaper in France than in England. Now, put our sulphuric acid manufacturers in the same predicament with the French, that is, suppose them to buy their materials as dear, and to sell their product as cheap, we know that every vitriol work would of necessity be shut. Even under the existing advantages many of them are making scarcely any profit.

The truth is, that the principles of the formation of oil of vitriol depend on the solution of a chemical problem of four conditions, or where four conflicting affinities are concerned, *viz.*, those between sulphurous acid, nitrous gas, oxygen, and water. Now these affinities are modified in a very essential manner by the quantity of nitrogen present in the mixture, and the degree of its temperature. Thus, a problem which requires a very clear chemical head to regulate, is generally left to a very rude operative hand, or intrusted to a speculative impostor, who knows nothing of chemistry but a few technical terms. The consequence is, that a great part of the materials of that manufacture are sent off by such *managers* into the air, in the form of sulphurous acid and nitrous gas, to the great annoyance of the neighbouring animals and vegetables, and the ruin, too often, of the proprietor. "Drink deep, or taste not," is an adage much more applicable to the Hermetic, than the Pierian spring.

The work, the title of which is prefixed to this article, and of which the perusal has suggested these preliminary remarks, is admirably adapted to the meridian of the great tribe of chemical dabblers. It will furnish them, at no expenditure of intellect, either in its author or themselves, with the ready means of juggling the public. Contemplated simply as a literary object, it is perhaps the strangest farrago which ever issued from our press. "There is *no* method in its madness." It is calculated to give the same idea of chemistry as we should have of Homer or Milton, if the whole of the lines of their divine poems were transposed and printed at random. Indeed, such dislocations as these *thousand experiments* exhibit, appear even greater than hazard could have produced. Chance would now and then have tossed together two similar subjects, as in a thousand rattles of the dice-box two similar throws might appear in succession.

The elaborate confusion of its parts might be accounted for, on the hypothesis that the whole affair is a sort of literary larceny; in which case it would become necessary to disfigure and derange the stolen articles, to prevent their being recognised by their respective owners. This would afford the simplest solution of the Chemical *Millenary.*

It should have been named, however, after a far more edifying and instructive compilation from Arabia, the *thousand and one* experiments, for we conceive the experiment of such a book upon the British public much more surprising than any chemical wonder which its pages display.

There is a quarto plate fronting the title-page, which appears to us the best representation of Pandemonium yet contrived. It leaves Fuseli's genius far behind. This picture professes to exhibit the "drawing the retorts at the great gas-light establishment, Brick-lane," but of their construction and arrangement it communicates no definite idea. We see only a parcel of naked red figures, "smirched with fire," plunging huge pitchforks into the flames, and a number of black pot-lids, hanging apparently from a great iron pipe, with the moon peeping through a grated window, shedding disastrous twilight on the infernal scene. The publishers, however, are well aware that a flaming frontispiece often helps off a very indifferent book.

The author, if this monstrous production can be traced to any individual parent, not only disclaims all pretensions to methodical arrangement, but speaks with sovereign contempt of all system. "Regarding the order of the work, he would say, that, notwithstanding the elaborate researches and ingenious speculations of many learned men in the several departments of chemical science, there are very few phenomena so perfectly developed as to admit of a systematic arrangement of the principles deduced from them; consequently, any attempt at forming a system, where the foundation and other parts of the superstructure are incomplete, would prove abortive. It is for this reason that attempts to systematize chemistry have in every instance been productive of failure*."

We congratulate Mr. Colin Mackenzie on his perfect emancipation from the trammels of order, and we trust that the fate of his disorderly experiments will in future prevent raw hands from meddling with such dangerous articles as acids, alkalis, crucibles, and chlorine. But his modesty is as meritorious as his method. "The author, at the commencement, perceived that a strict adherence to the accounts and opinions *even of the most approved chemical writers* would be far from satisfactory to HIMSELF; and would, in many cases, prove delusive to his readers. He likewise foresaw that, although a general reader might, without further inquiry, acknowledge, or allow as true, every process and fact comprehended in the following multitudinous assemblage; some who should honour his labours by a perusal might be more fastidious; and, by calling in question the truth of a few particular facts, processes, or opinions, might be apt to pass sentence of condemnation upon the whole. These considerations, conjoined with an ardent desire, which he himself had, of becoming practically acquainted with several of the chemical arts, induced him, on many occasions, to seek for information at its ultimately genuine source, *viz.*, the workshop of the artisan †." To make this delusive profession still

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* Preface, p. iv.

† Idem.

more catching, the following motto is contrived for the title-page: "Give me the facts," said my Lord Judge, "thy conclusions are but the guess-work of imagination, which puzzle the brain, and tend not to solve this mystery."

We shall now shew, by a few quotations taken at hazard, how little commensurate is the certainty of *his* facts to his assurance, and how completely at variance are *his* details and those of practical chemistry, as they are to be found in the workshop of the intelligent artisan. The volume contains 528 pages, distributed into 20 chapters, and ornamented with 20 plates. The following is the order of his chapters: 1st, Metallic alloys, under which we meet with the sulphurets of iron and nickel, and the phosphurets of nickel and copper. 2d, Art of coating metals, &c., with metals, 3d, Separation of metals, &c., from their combinations. 4th, Expansion of bodies by heat, under which we have ebullition, sublimation, fusion, and finally, the blow-pipe of Dr. Clarke and Mr. Hare, spread over ten pages. 5th, Chemical affinity, of which the first experiment is, "Non-affinity of steel for water;" the second is, "solution unattended by change of bulk;" of this we shall talk anon. 6th, Elective affinity. 7th, Compound, or double, affinity. 8th, Experiments on crystallization. 9th, Experiments on the discovery of certain substances in combination with others, by means of tests. 10th, Preparation of gases. 11th, Experiments on respiration. 12th, Experiments on combustion. 13th, Preparation and uses of fulminating and detonating compounds. 14th, Of the evolution and absorption of heat. 15th, Experiments on the motion of caloric, and on the capacities of bodies for heat. 16th, Phosphorescence of mineral and animal substances. 17th, Specific gravity. 18th. Colouring and Bleaching. 19th, Fermentation and Distillation, 20th, Preservatives against animal and vegetable putrefaction.

The preceding list of the titles of his chapters will shew every person of the least discrimination into what confusion he has plunged the details of his work. But lest we be deemed of the number of "fastidious" persons, let us now see how far "his ardent desire of becoming practically acquainted with the arts" has carried him; and what kind of instruction he has acquired in his pretended visits to "its ultimately genuine source, the workshop of the artisan."

At *Exp.* 22, under *alloy of gold and platinum*, we find the following statement, entitled, "Observations." "It is a curious circumstance, that the alloy of gold and platinum is soluble in nitric acid, which does not act on either of the metals in a separate state." There is only one thing which lessens a little the curiosity of the circumstance, its being *untrue*.

Exp. 44. "*To make cast-steel.* Put 20 parts of pure iron in small pieces into a crucible, with 6 parts of powdered chalk, and 6 parts of powdered Hessian crucible-ware. Dispose the whole so that after fusion the iron may be completely covered, to prevent the least contact with the air; now give the crucible a gradual heat, and then expose it to a white heat. Generally an hour will be sufficient to convert two pounds of iron into exceedingly hard steel, capable of being forged; an advantage not possessed by steel in the usual manner. *Observations.*—Here the iron is formed into a carburet, by combination with the carbon of the chalk and crucible powder." This is the whole article. We should like to be told what English artisan communicated this process, or at what factory steel is thus made. It is Clouet's old scheme, which no Englishman acquainted with steel would practise.

"*Exp.* 114. *To obtain pure platinum.*" Here we have nothing but Ridolfi's preposterous process, which we trust no artisan in this country has ever practised.

"*Exp.* 120. *To prepare pure alumine.* To a solution of sulphate of alumine (alum) in water, add a solution of pure soda, as long as a white precipitate falls down. Here the sulphuric acid combines with the soda, leaving the alumine free. Dry the precipitate quickly, and preserve it in a phial." Now, as pure soda is an excellent solvent of alumine, the earth cannot be precipitated "free" in this way. Ammonia is the proper precipitant, and the precipitate may be dried as slowly as we please, after it has been thoroughly washed. Our author has here contrived, in the fewest possible lines, to display the greatest possible ignorance of *practical* chemistry.

"*Exp.* 171. *Solution not attended with change of bulk.* In the chemical combination of fluids with solids, the compound occupies no more room than the solvent did previous to combination. Put some sugar, muriate of soda (common salt,) or *any other salt!* into an ounce of water, until no more will be dissolved. The solution will measure just an ounce, as the water did before the addition of the salt; but although there is no increase of *bulk*, there is a considerable increase of density *and* specific gravity." One hardly knows how to treat such effrontery of falsehood, for every assertion here is contrary to the best established facts, and the plainest experiments. Water saturated with common salt has a specific gravity of 1.1962, and contains 25.5 per cent. of salt. The 74.5 parts of water, in dissolving these 25.5 of salt, whose specific gravity is 2.0, would, if there were no increase on the bulk of the solvent, acquire a specific gravity of 1.342. But the real increase of bulk on these 74.5 parts of the solvent is to 83.6 exactly. Were there not a condensation attending this exertion of chemical affinity, the bulk of the solution would be 87.212. Or, in round numbers, 30 parts by bulk or weight of water, dissolve

10 by weight of salt, and the solution occupies the bulk of $33\frac{1}{3}$ parts.

"*Exp.* 182. *The solution of bodies in acids intercepted by mechanical pressure.* Put into a Florence flask some powdered carbonate of lime (chalk), and pour over it some diluted sulphuric, muriatic, or nitric acid; immediate effervescence will be the consequence. Now stop the mouth of the flask with a cork; the effervescence will instantly stop, upon the same principle that fluids refuse to boil when the superincumbent pressure is, to a certain extent, greater than usual. If the cork be withdrawn, the effervescence will be resumed. *Observations.*—The agitated motion of bodies undergoing solution does not differ from ebullition; for in both cases the fluids vaporize; and when this pressure is used the ascension of vapour must stop, not having sufficient mechanical force to overcome the power of the body which presses upon it." We shudder at reading this experiment, as the Florence flask will certainly burst, to the imminent hazard of the operator's eyes. Indeed, the adviser of such a project should be made liable at law for the damages. It reminds us of the school-boy's attempt to extinguish a cracker by putting it into his breeches pocket.

"*Exp.* 183. *Solution of tin in nitrous acid.* Pour half an ounce of nitrous acid, over half an ounce of granulated tin (Pulvis Stanni) in a tumbler: very little action will take place, owing to the inability of both substances to present to each other a sufficient surface. But if an ounce of water be added, a very violent commotion will ensue, during which the tin, in an oxidated state, and of a yellow colour, will be seen to run up and down from the bottom to the surface, whilst a great quantity of nitrous gas is disengaged. This solution is the nitrite of tin." "*Observation.* The above solutions of tin, as will be seen hereafter, are much used by dyers." The dyer is to be pitied, who would try to make a solution of tin, in this way. In fact, the action above described ruins the process, and converts the tin into an insoluble peroxide, useless to the artisan. But this is not all: common nitrous acid invariably acts with great violence upon powdered tin, and a tyro might seriously injure himself by following Mr. Mackenzie's recipe.

Under *Exp.* 412, we find an encomium on the virtues of "Strathpeffer water, a very productive well, in Ross-shire, Scotland;" the district of the Clan Mackenzie. This internal evidence has partly satisfied us of the identity of the author whose name is on the title-page; we once thought it merely a *nom de guerre*.

At *Exp.* 469, p. 251, we find the following sapient remarks: "It is necessary to be observed here, that chlorine, and all other gases readily absorbable by water, should be received over *mercury*, in a trough made for the purpose of containing from a dozen to twenty pounds." So *practical a chemist* as Mr. Colin Mackenzie ought to know that mercury is the very substance employed to absorb chlorine in analytical researches on mixed gases; and that no *practical chemist* of common sense, ever tried to receive chlorine over mercury.

"*Exp.* 471. *Iodine vapour.* Put a small quantity of iodine into a retort, and hold it over a lamp; when heated considerably (about 300°) a very beautiful vapour, or gas, will come over, which may be received in jars on the pneumatic shelf, over water." Quere; is Mr. Mackenzie's water also heated to 300°; did he ever see, or hear, of iodine as a beautiful gas standing over water in the pneumatic trough?

"Combustion is the decomposition of a body at an elevated temperature, with the evolution of light and heat. Some suppose combustion to be the effect of a certain degree of motion of the particles of combustible bodies; and that flame is merely a transparency, or luminosity, of these particles when they are thrown to certain distances with considerable velocity*." One can hardly afford to waste criticism on such pompous nonsense. Combustion is known to be most vivid, when simple bodies are engaged; and when there is *no* decomposition, but the reverse, as with phosphorus and oxygen, sulphur and copper, *&c.*

"*Exp.* 529. *Combustibility of hydrozincic gas.*" This fine name merely signifies hydrogen procured by acting on zinc, as usual, with a dilute acid.

"*Exp.* 545. *Tin burns brilliantly in oxygen gas.* Heat some granulated tin considerably, in a platinum spoon, and in this state immerse it in oxygen gas; a very beautiful combustion, attended by a brilliant white light will instantly take place; when oxide of tin will be formed." The possessor of such a spoon had better not try this trick, for if the tin burns, which we have some doubts about, the spoon would melt, thus adding to the *instructiveness* of our author's experiment.

"*Exp.* 571. *A lighted taper burns with much energy in chlorine gas.*" It so happens that it is presently extinguished.

"*Exp.* 572. *Combustion of charcoal powder in chlorine.* Pour some dry charcoal, newly made and finely powdered, into a jar, containing chlorine gas; a very beautiful combustion will take place, displaying a stream of fire." Not true.

"*Exp.* 719. Latent heat is necessary to preserve bodies in the *solid*, liquid, and gaseous states." Dr. Black imagined that bodies would keep very well in the solid state, without the help of latent heat. Mr. Mackenzie has discovered that they will not.

"When bodies are mixed or combined, and the density or bulk becomes less than that of the fluids before mixture, heat

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* Page 281.

will be evolved*" This absurd enunciation is for some unknown reason, printed in italics. He evidently does not know, that when the density of a body is increased by any means, its bulk becomes less, and *vice versâ!*

The chapter on specific gravity, is stuffed out with common-place and ill-arranged details on the art of swimming, diving bells, weather-glasses, balloons, and air-pumps.

Chapter 18, entitled "*Colouring and Bleaching,*" contains an unmerciful and unacknowledged plagiarism from a theoretical article on Dyeing, in the first supplement to the *Encyclopedia Britannica.* It can be of no service to the artisan.

"*Exp.* 867. *Bleaching by means of the Oxymuriatic gas.*" Towards the end of this article we have the following precious recipes. "The proportions observed when cotton is to be bleached, are manganese 30 parts, common salt 80, sulphuric acid 60, water 120. For linen cloth the proportions are as follow: manganese 60 parts, salt 60, sulphuric acid 50, water 50." We should be glad to know why 30 parts of manganese are sufficient for 80 of common salt in the first case, and 60 are required for 60 in the second. Nothing can place in a clearer light than these recipes, the irrational and dangerous empiricism of his pretended prescriptions for manufacturers. Again, "*Exp.* 870. To bleach by means of the sulphuret of lime." This old project of Mr. Higgins, was very unfortunate. If Mr. Mackenzie will take the trouble of inquiring of any intelligent Irish linen bleacher, (and there are many who understand chemistry infinitely better than he does,) he will find that the linen trade received such a shock by the employment of sulphuret of lime, instead of barilla, as it did not recover in reputation for several years. An immense deal of cloth was ruined by this notable recipe. Mr. Mackenzie's directions to bleachers remind us of those of an eminent agriculturist, (not less profoundly versed in *practical chemistry*), to farmers, who having heard of a plan for preventing the ravages of the fly in turnips, part of which consisted in steeping the seeds in a solution of chlorine: and having, moreover, heard that chlorine was produced by a mixture of oil of vitriol, salt, and manganese, committed the trifling chemical error of directing the seeds to be steeped in this mixture; a perfect cure, no doubt, for the fly, or any other evil to which the crop might have been liable.

"*Exp.* 997. *Copal Varnish.* Reduce to powder an ounce of pure carbonate of potash, and then lay it before the fire till it becomes hot and dry. In this state put it into a pint of alcohol, or oil of turpentine. All the watery particles contained in the oil, or spirits, will be absorbed by the alcali; and the alcohol, or turpentine, will thus become pure, or highly rectified. This process is

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* Page 349.

called *alcalizing* these solvents. Now put the turpentine, or spirits, into a vessel with two ounces of clean dry copal, finely pounded and sifted; place the vessel in warm water; the copal will soon be dissolved." "Observations." "If the spirits of turpentine be alcalized when the copal is dissolving, a little spirits of wine should be added; and if the spirits of wine be alcalized when the copal is dissolving, a little spirits of turpentine should be added; the sediment of the varnish will dry on the silk in a few hours; the thicker the varnish, the sooner it dries." On this occasion, as with most other of his *practical* recipes, Mr. Mackenzie has been humbugged by the workmen to whom he has addressed himself. The above process is good for nothing. An ounce of dry carbonate of potash will not dephlegmate a pint of the alcohol of the shops, and the copal instead of being soon dissolved might remain there till dooms-day.

We presume, we have now satisfied every reader of ordinary intelligence, that this costly octavo is so devoid of scientific views, and so replete with practical errors, as to render its perusal useless to the student, illusory to the political economist, and deceitful to the artisan. Here and there, we meet with extracts from respectable authors, inserted often without acknowledgment, and always without direct reference to the source whence they are immediately derived. But, in general, the book is a *cento* of obsolete and exploded operations. We close these remarks, flowing solely and sincerely from a wish to protect our countrymen from deception and loss, with the following sentence, very slightly altered from the author's note to page 274. "It is hoped that none of the experiments will be repeated through wantonness." In this case, we are sure they will not be tried at all; for neither profit nor instruction can result from their repetition, as described by Mr. Colin Mackenzie.

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iii. *Philosophical Transactions of the Royal Society, of London, for the year* 1821. *Part II.*

In resuming our account of the *Philosophical Transactions* for the year 1821, we shall lay before our readers a brief abstract of the principal facts contained in the several papers published by the Royal Society, examining them in the order in which they stand in the volume.

1. *An account of Experiments to determine the acceleration of the Pendulum in different Latitudes. By* Captain Edward Sabine, of the Royal Regiment of Artillery, F.R.S. & F.L.S.

To the merit of Captain Sabine, as an accurate and acute observer, we have more than once had occasion to advert; he is an exemplary instance of a military officer devoting his time and talents to the abstract pursuits of science, and voluntarily submitting to severe deprivations and personal danger, in the disinterested promotion of natural knowledge. We more particularly admire his various observations and inquiries made during the expeditions to the Arctic regions (under Captain Ross in 1818, and especially under Captain Parry in 1819 and 1820), for the skill and perseverance which they display in overcoming the untoward difficulties and unforeseen obstacles, arising out of situation and climate.

The clocks and pendulums, used by Captain Sabine in the researches described in this paper, belong to the Royal Society, and were prepared by their direction, under the superintendence of Captain Kater, whose description of them is quoted by the author at the commencement of his communication. The experiments were made during two voyages of discovery in search of a North-west passage, the first in 1818, and the second in 1819 and 1820; and Captain Sabine details in succession the proceedings at each station where an opportunity was afforded of landing and setting up the clocks, and concludes by recapitulating the number of vibrations made by each pendulum in the different latitudes in which it was tried, and by stating the deductions regarding the figure of the earth, which follow from the accelerations thus determined.

In the first voyage, the number of vibrations was ascertained at two stations only, namely, at Gardie-house, on the island of Brassa, and on Waygat or Hare Island, on the west coast of Greenland, the latitude of the first being 60° 09′ 42″ N., and that of the second 70° 26′ 17″ N. The number of vibrations in a mean solar day at London being 86497.4, at Brassa they were 86530.507, and at Hare Island 86562.6386, giving an acceleration of 33.107 vibrations between London and Brassa, and of 32.1316 between Brassa and Hare island, or 65.2386 between London and Hare Island.

Captain Sabine then proceeds to detail the preliminary experiments relating to the pendulums, and the results of his various observations, made during the second more auspicious voyage under Captain Parry, from which it appears that at Melville Island, in the Polar Sea, in lat. 74° 47′ 12.″4 N., the mean diurnal acceleration amounted to 74.734 vibrations. Of the proceedings at this station he gives a circumstantial account, which we from lay before our readers the following interesting details:

> As soon as the harbour was determined in which it was purposed to secure the ships for the winter, and whilst a canal was cutting to admit them through the ice by which it was already occupied, its shores were carefully examined, with a view to select a suitable spot for an observatory.
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> The land was found of little elevation, and generally level, except where

intersected by ravines, being the courses in which the winter's fall of snow drained on dissolution to the sea. The soil, which appeared by the banks of these channels to be many feet in depth, consisted of sand intermixed with small stones, being the debris of the sandstone rock of which the island is composed; it was at this time consolidated by the frost, and was harder than the original rock, but much the greater part bore evident marks of being swampy at times; and even the more elevated spots afforded little prospect of a solid foundation for the clock-stands on the return of summer.

However, as no preferable situation could be found within such distance from the ships, as it would have been convenient, or indeed prudent, to venture, one of these was fixed on; and it was hoped that by sinking the legs of the stands a few inches into the frozen soil, and by commencing the experiments as early in the ensuing year as the season should admit, they might be completed before the ground should be affected by a thaw.

It was desirable therefore to be thoroughly prepared before the severity of the winter should set in; accordingly when the ships had been secured, and a party of men could be spared for the occasion, an observatory house was commenced. The house was built of the store plank and boards carried by the ships, care being taken to cut or injure them as little as possible: the walls were weather-boarded, lined, and filled in between with moss; the roof was protected by a tarpaulin covering; it was divided into two rooms, whereof the inner, being designed for the reception of the clocks, was warmed by pipes proceeding from a stove placed in the outer room; the floors were boarded, and the walls furnished on the inside with Russia matting. The house was finished and the clocks moved into it before the end of October.

If any hope had been entertained of being able to do more during the winter than merely to prepare for the return of more favourable weather, it was ended by the severity of cold, far exceeding expectation, with which November set in. From this date until the close of March, the highest degree registered by a thermometer, suspended in the air, was +6° Fahrenheit, and in no one of these five months did the mean temperature rise above −18°; under such circumstances, an attempt to raise the temperature of the house, sufficiently to carry on the experiments, and to keep it up during their course, with the requisite steadiness and uniformity, must have altogether failed. It may not be amiss to remark, that notwithstanding the house was as effectual for the purpose as the utmost liberality in the supply of materials, with no labour spared in their application, could produce, a very little wind with so low a temperature abstracted the heat with such rapidity, that the influence of the stove was scarcely felt beyond its immediate vicinity; and a thermometer placed in those parts of the inner room where the clocks would have stood, could not be kept above zero, with such fire in the stove as it would have been prudent to maintain.

The clocks were therefore suffered to remain unpacked during the winter in the inner room, whilst the outer served a variety of useful purposes, which could not have been conveniently effected on board ship.

On the 24th of February, the matting with which the walls of the outer room were covered accidentally caught fire, and notwithstanding the endeavours of the persons who were present, the fire was communicated rapidly to the roof; it was at length fortunately extinguished by the exertions of the officers and men from the ships, before the clocks or any part of their apparatus had received injury; the packing chest alone of one was slightly scorched: the only personal sufferer on the occasion was an artilleryman, who had accompanied me on the voyage, and who, in his anxiety to place the instruments out of danger, exposed his hands incautiously, and was in consequence so severely frost-bitten, as to render necessary the amputation of three fingers of the left hand, and two of the right.

The house was speedily repaired, the outer room being reduced in size to a porch sufficient to contain the stove; and the inner room, which had scarcely been touched by the fire, remaining as before.

Towards the end of April the sun had influence to keep the thermometer a few degrees above zero for some hours of the day. The clocks were now unpacked and set up: the flooring being removed, the legs of the stands were placed on slippers sunk some inches into the frozen ground in grooves which were excavated by crow bars.

It may be worthy of remark, that when the boxes containing the thermometers which accompany the clocks were opened, the mercury was observed to be retired into the bulbs and freezing, although the temperature of the air had not been so low as the frozen point of mercury for several weeks. The thermometer boxes were enclosed each with the pendulum to to which it belonged, in a stout case of oak; and these again were contained in chests holding each one clock with its apparatus complete. The thermometers had been thoroughly cooled in their cases by the long continued severity of the winter; but the warmth had not yet made its way through such a multiplicity of enclosures. It may be also mentioned, in proof of the slowness with which such a mass of solid brass as constituted the bob of the pendulums conforms to the temperature of the surrounding atmosphere, compared with the mercury in the thermometer tubes, that several hours had elapsed, after the pendulums were taken out of their cases, (when it is presumed they also may have been at −40°) before they ceased to cause a deposit of moisture from the air of the room, which was about the same number of degrees above zero: the mercury in the thermometers, on the other hand, took up the temperature of the room within half an hour after their exposure. The clocks were put in motion on the 30th of April, and the account taken up on the 4th of May, the room having been kept at about the temperature of +45° for the preceding three days and nights.

It was not, however, till the third week in May that the weather became warm enough to commence the observations, for during the first fortnight strong westerly winds prevailed, and the observatory was occasionally buried in the drifted snow, when the only access to it was by digging down to the window of the room containing the clocks, and its mean temperature was only 6°, instead of the desirable one of 45°, at which the experiments in London had been conducted. During the second fortnight the mean temperature was 25°; but various difficulties occurred, and at the end of the month the thaw prevailed to such an extent as to oblige the abandonment of the house, before any satisfactory results had been obtained.

Towards the middle of June, therefore, the clocks were set up in a tent, which could be occasionally heated by a stove, and an account of their going is given from the 30th of June to the 14th of July, when it was conceived that a sufficient number of results had been obtained.

From the observations, fully detailed in this paper, respecting the length of the seconds' pendulum at the several places of observations, it appears that its length at London being, as ascertained by Captain Kater, =39.13929 inches, at Brassa it is 39.16929 inches, at Hare Island 39.1984 inches, and at Melville Island 39.207 inches.

Captain Sabine concludes this important communication with the following table, shewing the diminution of gravity from the pole to the equator, and the resulting ellipticity of the earth, deduced from the preceding observations by Captain Kater's method, described in the *Philosophical Transactions* for 1819.

	Dimunition of Gravity.	Ellipticity.
London and Brassa	.0055066	$\frac{1}{314{,}3}$
London and Hare Island .	.0055082	$\frac{1}{314{,}2}$
Brassa and Hare Island . .	.0055139	$\frac{1}{313{,}6}$
London and Melville Island	.0055258	$\frac{1}{312{,}6}$

2. *Some Observations and Experiments on the Papyri, found in the Ruins of Herculaneum. By* Sir Humphry Davy, Bart. P.R.S.

In this paper Sir Humphry Davy gives an account of his first experiments upon fragments of papyri, which induced him to hope that chemistry might afford assistance towards unrolling the MSS. He then describes those in the Museum at Naples, and the processes carried on there, and concludes with some general observations on the manuscripts of the ancients. The following is a brief outline of the interesting contents of this communication:

The papyri in question appear to consist of leaves reduced to the state of cinder, cemented by a matter soluble in certain liquids, and especially in muriatic and nitric ether; now, as chlorine, while it has a strong attraction for hydrogen, exerts no action upon carbonaceous substances, and as charcoal forms the basis of ancient writing-ink, it occurred to Sir Humphry that that gas might be usefully applied to destroy the adhesiveness of the layers; he therefore tried it and some other agents, possessed of analogous properties, and his attempts were to a certain extent successful.

The state of the Neapolitan specimens, and the general existence in them of undecomposed vegetable matter, suggest some curious remarks respecting the origin of the changes which they have suffered, and which have commonly been referred to the action of fire; the part of Herculaneum, however, in which they were found was not inundated by lava, but covered by sand and ashes, cemented together by the operation of water; and again, that fire is not necessary for the carbonization displayed by the manuscripts, is inferred from the state of the wood in the houses at Pompeii, which is always converted into charcoal, though covered by a shower of ashes, which must have been cold, as they fell at a distance of seven or eight miles from the crater of Vesuvius. Among the Neapolitan manuscripts there are some covered with a glossy substance resembling varnish, arising, Sir Humphry suggests, from the decomposition of the skin used to infold them, and now converted into a brilliant animal charcoal, leaving phosphate of lime when burned, but producing at the same time no inconsiderable quantity of ammonia.

One method only has been adopted for unrolling these fragile coils of carbonized papyrus; it consists in applying thin animal membrane (gold-beaters' skin), by a solution of glue to the back of the manuscript, and carefully elevating the layers by a silk thread, when the glue is dry. Alcohol and ether were found useful auxiliaries in this operation, and great advantage was also derived from throwing warm air upon the surface of the leaves, with precautions which are pointed out in the paper before us. The different MSS., however, required very different treatment.

During the two months that Sir H. Davy was employed in these experiments at Naples, he succeeded, with the assistance of the persons attached to the Museum, in partially unrolling twenty-three MSS., from which fragments of writing were obtained, and in examining about 120 others which gave no hopes of success:

> And I should gladly have gone on with the undertaking, from the mere prospect of a possibility of discovering some better results, had not the labour, in itself difficult and unpleasant, been made more so, by the conduct of the persons at the head of this department in the Museum. At first every disposition was shewn to promote my researches; for the papyri remaining unrolled were considered by them as incapable of affording any thing legible by the former methods, or, to use their own word, *disperati*; and the efficacy and use of the new processes were fully allowed by the Svolgatori or unrollers of the Museum: and I was for some time permitted to choose and operate upon the specimens at my own pleasure. When, however, the Reverend PETER ELMSLEY, whose zeal for the promotion of ancient literature brought him to Naples for the purpose of assisting in the undertaking, began to examine the fragments unrolled, a jealousy, with regard to his assistance, was immediately manifested; and obstacles, which the kind interference of Sir WILLIAM A'COURT was not always capable of removing, were soon opposed to the progress of our inquiries; and these obstacles were so multiplied, and made so vexatious towards the end of February, that we conceived it would be both a waste of the public money, and a compromise of our own characters, to proceed.

In respect to the date of these MSS., Sir Humphry remarks that, from the mixture of Greek and Roman characters, it is probable some of them were very ancient when buried. The ink with which they were written was a mixture of charcoal and glue, and from the omission of any mention by Pliny of an ink of galls and iron, it is not probable that it was used up to his period, but that parchment and our present writing ink were adopted together, " for a mixture of charcoal and solution of glue can scarcely be made to adhere to the skin; whereas, the free acid of the chemical ink partly dissolves the gelatine of the MSS., and the whole substance adheres as a mordant."

The earliest parchment manuscripts are probably the *Codices Rescripti*, discovered in the libraries of Milan and of Rome; in these, time has destroyed the vegetable matter of the ink, but solution of galls revives its blackness.

I have tried several substances for restoring colour to the letters in ancient MSS. The triple prussiate of potash, used in the manner recommended by the late Sir CHARLES BLAGDEN, with the alternation of acid, I have found successful, but by making a weak solution of it with a small quantity of muriatic acid, and by applying them to the letters in their state of mixture with a camel's hair pencil, the results are still better.

After all, we have probably sustained no great loss in the destruction of the Herculanean manuscripts; no fragments of Greek, and very few of Latin, poetry have been found in the whole collection, and the sentences which have been made out shew that the works are of the same kind as those before examined, and belong to the schools of the Greek Epicurean philosophers and sophists.

3. *Observations on Naphthaline, a peculiar substance resembling a concrete Essential Oil, which is apparently produced during the decomposition of coal-tar, by exposure to a red heat. By* J. Kidd, M.D., *Professor of Chemistry, Oxford.*

This is the very singular substance of which an account has already been given in this Journal (Vol. VIII. p. 287), but upon which no distinctive name had then been bestowed; the most important deficiency in its chemical history is unfortunately not supplied by Dr. Kidd. " With respect to the elementary constitution of this substance," he says, " I am not enabled to give any satisfactory information."

4. *On the Aberrations of Compound Lenses and Object Glasses.*

By J. F. W. Herschel, Esq. F.R.S., &c.

In this elaborate and truly important paper Mr. Herschel presents, under a general and uniform analysis, the whole theory of the aberrations of spherical surfaces, and furnishes practical results of easy computation to the artist, disentangled from all algebraical complexity, and applicable, by interpolations of the simplest possible kind, to all the ordinary varieties of the materials on which he has to work.

5. *An Account of the Skeletons of the Dugong, two Horned Rhinoceros, and Tapir of Sumatra, sent to England by Sir Thomas Stamford Raffles, Governor of Bencoolen. By* Sir E. Home, Bart. V.P.R.S.

This memoir is illustrated by five plates, from drawings by Mr. Clift, without reference to which the details would scarcely be intelligible to the majority of our readers. Sir Everard justly eulogizes the exertions of Sir Thomas Raffles in promoting the pursuits of natural history and comparative anatomy; to his active interference we are indebted for the account of that very singular animal, the Dugong, published in the preceding volume of the *Philosophical Transactions*; it is the only one yet known that grazes at the bottom of the sea without legs; being of the figure and form of a whale, the position and structure of its mouth enables it to browse upon the fuci and submarine algæ like a cow in a meadow, and the whole structure of the masticating and digestive organs shews it to be truly herbivorous. It never visits land, or fresh water, but lives in shallow inlets, where the water is two or three fathoms deep. Their usual length is eight or nine feet. But a curious and, to some, perhaps the most interesting part of the history of this animal is, that the flesh resembles young beef, being very delicate and juicy; so that, as Sir Stamford, in the description of its dissection, remarks, " it afforded no less interest under the knife, than satisfaction on the table."

6. *On the Mean Density of the Earth. By* Dr. Charles Hutton, F.R.S.

We can never feel satisfied that it is fair to infer the density of the globe from that of any protuberance upon its surface, and are, therefore, not inclined to acquiesce in the importance of Dr. Maskelyne's researches, who aimed at ascertaining the mean density of the whole " terraqueous globe of our planet," by measuring the dimensions of an insignificant mountain in Scotland, and comparing its attraction on a plummet, with that of the whole earth on the same. Mr. Cavendish sought the solution of this problem by another method, consisting in ascertaining the attraction on small pendulous balls, of two inches diameter, by larger ones of ten inches diameter, as compared with the attraction of the earth on the same. The respective merits of these two experiments admit of considerable discussion, and upon these it is not our intention to enter; the learned author of the paper before us is in favour of the former, and he bore an active share in the investigations connected with it; he took all the measurements upon which the calculations respecting the magnitude of the mountain were founded, and, in short, performed all those computations recorded in the *Philosophical Transactions* for 1778, and which employed his daily labours during the greater part of two years. Under these circumstances he justly complains that his name has been withheld, with regard to the great share he took in those inquiries, and that assertions have been attributed to him which are not justified by any thing he has written. From a review of the investigations, he thinks it highly probable that the mean density of the earth is five times that of water, but not higher; Mr. Cavendish has assumed the mean density $=5.48$, a result, upon the accuracy of which Dr. Hutton has thrown much doubt. We do not think that the computations founded upon the mountain experiment, derive that verification from Mr. Playfair's *Lithological Survey* which Dr. Hutton is inclined to insist upon, and we wish that he had deduced the number 5. from more satisfactory data. When our author's computation of the earth's density was first made, the real density of the hill was unknown, "it was only known that it consisted chiefly of very hard and dense rocks," much heavier than common stone, which is allowed to be $2\frac{1}{2}$ times the density of water.

I then, by way of example in applying the density, multiplied $\frac{9}{5}$ by $2\frac{1}{2}$, which produced $\frac{9}{2}$ or $4\frac{1}{2}$ for the density of the earth, on the smallest assumption; till such time as we should come to know more nearly what the real density of those rocks is: and therefore I must feel reason to complain, that this number ($4\frac{1}{2}$) has often been stated, rather unfairly, as my final conclusion for the earth's mean density; instead of being only the very lowest limit that might be used, till we could better learn something on that point with more certainty. But a lithological survey of the mountain being afterwards accurately made, at my earnest request, by that excellent philosopher and geologist, Mr. PLAYFAIR, the result of which was published in the *Philosophical Transactions* for the year 1811; I then applied his mean statement of the rocks to my own calculations, which gave me the number 5 for the density of the earth; as I published in the fourteenth volume of my edition of the *Philosophical Transactions*, and in the second volume of my *Tracts*.

In concluding his paper, Dr. Hutton suggests that one of the pyramids in Egypt might profitably be employed instead of a mountain for this experiment; such a body, he says, offers several advantages; its mass is sufficiently large, standing upon a base of about the size of the whole space of Lincoln's-inn Fields, and of a height almost double of that of St. Paul's Steeple.

Then the station for the plummet, or zenith sector, could be taken much nearer the centre of the mass, than on a mountain, which would give a larger quantity of deviation of the plummet; then the regular figure and the known composition of the mass would yield great facilities in the calculation of its attraction; lastly, the deviation of the plummet might be observed on all the four sides. Should such a project take place, it will be best to take the stations at about one fourth of its altitude above the base, that being the place where the deviation of the plummet would be the greatest. Finally, so favourable for such an experiment do those circumstances appear, and so anxious are my wishes for its completion and success, that, were it not for my great age and little health, I should be glad to make one in any party to undertake such an expedition.

7. *On the Separation of Iron from other Metals. By* J. F. W. Herschel, Esq.

The following is Mr. Herschel's mode of proceeding:

The solution containing iron is to be brought to the maximum of oxidation, which can be communicated to it by boiling with nitric acid. It is then to be just neutralized *while in a state of ebullition*, by carbonate of ammonia. The whole of the iron to the last atom is precipitated, and the whole of the other metals present (which I suppose to be manganese, cerium, nickel, and cobalt), remains in solution.

The precautions necessary to ensure success in this process are few and simple. In the first place, the solution must contain no oxide of manganese or cerium above the first degree of oxidation, otherwise it will be separated with the iron. It is scarcely probable in ordinary cases that any such should be present, the protoxides only of these metels forming salts of any stability; but should they be suspected, a short ebullition with a little sugar will reduce them to the minimum. If nitric acid be now added, the iron alone is per-oxidized, the other oxides remaining at the minimum. Moreover, in performing the precipitation the metallic solution should not be too concentrated, and must be agitated the whole time, especially towards the end of the process; and when the acid re-action is so far diminished that log-wood paper is but feebly affected by it, the alcaline solution must be added cautiously, in small quantities at a time, and in a diluted state. If too much alcali be added, a drop or two of any acid will set all right again; but it should be well observed, as upon this the whole rigour of the process depends, that no inconvenience can arise from slightly surpassing the point of precise neutralization, *as the newly precipitated carbonates of the above enumerated metals are readily soluble, to a certain extent, in the solutions in which they are formed (though perfectly neutral)*. In the cases of cobalt and cerium, this re-dissolution of the recent precipitate formed by carbonate of ammonia is very considerable, and a solution of either of these metals, thus impregnated with the metallic carbonate, becomes a test of the presence of peroxide of iron, of a delicacy surpassing most of the re-agents used in chemistry, the minutest trace of it being instantly thrown down by them from a boiling solution, provided no marked excess of acid be present. To be certain however that we have not gone too far, it is advisable, after separating the ferruginous precipitate, to test the clear liquid, while hot, with a drop of the alcaline carbonate. If the cloud which this produces be clearly re-dissolved on agitation, we may be sure that only iron has been separated. If otherwise, a little acid must be added, the liquor poured again through the filter, so as to wash the precipitate, and the neutralization performed anew.

Such of our chemical readers as may be concerned in the analysis of ferruginous compounds, will find some remarks on the subject in the observations which follow the above extract.

8. *On the Re-establishment of a Canal, in the place of a portion of the Urethra, which had been destroyed. By* Henry Earle, Esq. *Surgeon to the Foundling,* &c.

The details of this paper are too purely surgical to admit of being quoted at length in this Journal, at the same time they lead to the demonstration of a curious and important physiological fact, and upon that ground the communication has probably been admitted into the *Philosophical Transactions.*

9. *Calculations of some observations of the Solar Eclipse of the 7th September,* 1820. *By* Mr. Charles Rumker.

The details of this paper scarcely admit of abridgment.

10. *An Account of the Remeasurement of the Cube Cylinder and Sphere used by the late Sir George Shuckburgh Evelyn, in his Inquiries respecting a Standard of Weights and Measures. By* Captain Henry Kater, F.R.S.

Sir G. S. Evelyn's experiments, adverted to in the title of this paper, are detailed in the *Phil. Trans.* of 1798, and though he bestowed the greatest attention on those parts of the inquiry relating to the *weight* of the solids, their *measurement* was not so accurately performed; Captain Kater therefore was anxious to re-investigate the latter subject, previous to the final report of the Commissioners of Weights and Measures.

In this paper, Captain Kater describes the state of the apparatus, and the means which he adopted in effecting their measurement. The mean result of the measurement of three sides of the cube gives for its contents 124.1969 inches.

The length of the cylinder, deduced from three means, is 5.9960 inches.

In the original measurement of the sphere, a brass square was employed, the side of which was a little larger than the diameter. The sphere being properly placed and supported within the square, a micrometer screw, which passed through one of the sides of the square, was brought in contact with the diameter of the sphere, and the reading of the micrometer head noted; the sphere being then removed, a brass ruler, of known length, was put into its place, and the micrometer screw being brought in contact with the end of the ruler, the difference between its length and the diameter of the sphere was obtained, from which the latter was determined.

Captain Kater details at length the repetition of Sir George's measurements, whence it appears that the excess of the diameter of the sphere above the length of the ruler gives 0.0012281 inches. The author then proceeded to measure the brass ruler, the length of which was found $=6.0063609$ inches, and the diameter of the sphere thence deduced gave 113.5264 inches for its solid contents.

Captain Kater concludes this paper with a table, shewing the data furnished by Sir G. S. Evelyn's experiments, and his own measurements, from which it appears that the weight of a cubic inch of distilled water, in a vacuum, at 62°, is $=252.888$ grs. of Sir George's standard, or 252.722 grains of the parliamentary standard.

11. *An account of Observations made with the Eight-feet Astronomical Circle, at the Observatory of Trinity-College, Dublin, since the beginning of the year* 1818, *for investigating the effects of the Parallax and Aberration on the places of certain Fixed Stars; also the comparison of these with former observations for determining the effects of Lunar Nutation.* *By* the Rev. John Brinkley, D.D., F.R.S. &c.

The observations, of which an account is given in this communication, were instituted with a view to discover the source of the differences that have existed between the observations made at Greenwich and those at Dublin, relative to parallax. After a perspicuous and detailed statement of the method of conducting his observations, Dr. Brinkley avows his inability to rectify the discordancies, observing, at the same time, that, as the whole extent of the difference in question is but one second, it may by some be considered as showing the great precision of modern observations. Yet, independent of the interest of the question of parallax, it is important that the origin of the difference, small as it is, should be ascertained.

12. *On the Effects produced on the Rates of Chronometers by the Proximity of Masses of Iron.* *By* PETER BARLOW, Esq., *of the Royal Military Academy.*

In a paper printed in the *Philosophical Transactions* for 1820, Mr. George Fisher, who accompanied Captain Buchan to the Arctic regions in 1818, has shewn that the rates of chronometers differ on board and on shore, gaining in the former, and losing in the latter situation; this he attributed to the magnetic influence of the ship's iron on the balance, and instituted various experiments, which with their results are detailed in his communication, to shew that the magnetic influence has a tendency to accelerate the motion of the time-keepers by its influence on the steel part of their balances. Mr. Barlow's inquiries contained in the paper before us have led him to different conclusions; he found, as might be expected, that the proximity of masses of iron influences the rates of chronometers, but so far from accelerating them, five out of six upon which he experimented were actually retarded, and the acceleration of the sixth was doubtful: he shews that much depends upon the *position* of the balance in regard to the iron. All these experiments prove that chronometers should be as carefully secluded from the action of any partial mass of iron, as the compass itself; and Mr. Barlow suggests, that as much of the iron of a ship is hidden, the best way of ascertaining a proper place for a chronometer will be to set down a compass in the situation designed for it, and to observe and compare the direction of its needle with that of an azimuth compass on deck, while the vessel is on different tacks, and if the disagreement between the two be considerable, to select another situation.

13. *On the Peculiarities that distinguish the Manatee of the West Indies, from the Dugong of the East-Indian Seas. By* Sir EVERARD HOME, Bart., V. P. R. S.

This is a short comparative description of the anatomy of these animals, illustrated by four plates from the accurate pencil of Mr. Clift. The first of these shews the external form of the Manatee, which has a broader tail and greater lateral extension of the ribs than the dugong; its habits of life place it between that animal and the hippopotamus; it has no tusks, and feeds upon plants growing at the mouths of large rivers. The second plate is a general view of the skeleton; the third, a representation of the stomach; and the fourth exhibits the peculiarities of its cæcum.

14. *On a new Compound of Chlorine and Carbon. By* RICHARD PHILLIPS, F. R. S. E., *&c., and* M. FARADAY, *Chemical Assistant in the Royal Institution.*

This compound was accidentally found by M. Julien, a nitric acid manufacturer of Abo, in Finland, amongst the products of his processes. It is a soft white tasteless solid; insoluble in water; volatile and inflammable; insoluble in acids and alcalis; and when raised in vapour over hot mercury, and detonated with excess of oxygen, chloride of mercury and carbonic acid are the results.

The analytical details of this paper prove the substance described to consist of one portion of chlorine = 33.5 and two of carbon = 11.4, so that it is intermediate between the protochloride and perchloride of carbon, previously described by Mr. Faraday. All endeavours to form this chloride, or to convert it into the other chlorides, have proved ineffectual; yet the smallness of the quantity of this compound in possession of the authors, prevented their pursuing this investigation to the extent they could have desired.

15. *On the Nerves: giving an Account of some Experiments on their Structure and Functions, which lead to a new Arrangement of the System.* By Charles Bell, Esq.

This paper, consisting of an inquiry into the functions of the nerves of the face, is preceded by some general observations on the nervous system.

If the views which Mr. Bell has presented in this communication be found to be correct, a very important addition is undoubtedly made to our knowledge of anatomy; and a part of the system hitherto very imperfectly understood assumes a character of simplicity and order in contrast not only with the doctrines which have been taught in our schools, but also with the opinions of the most celebrated foreign anatomists.

When we speak thus hesitatingly, we allude to the prefatory part of the paper; with regard to the facts and observations in the body of it, there can be but one opinion—they are quite new to us, and lead to the knowledge of phenomena important both in theory and practice.

Mr. Bell's leading principle of arrangement for the nerves of the *frame of the body*, is to divide them into two systems; one for the supply of common sensibility and the power of locomotion, to be found in all animals which have nerves, even in those very low in the scale of existence—while the presence of the other system depends on the complication of the structure of the animal, being complex in proportion to the number of the organs of the animal economy, and to the variety of functions which those organs perform. When an animal has no heart or concentrate organ of respiration, there will be none of those nerves found, which in the higher classes give so much intricacy to the whole system; but when these organs are present, then new nerves are bestowed upon the animal, and these will be simple or complicated, according as the relations of the superadded organs are few or many. For example, all animals that have a *heart*, *lungs*, and *stomach*, have a nerve appropriated to these organs, but this nerve will be found simple or otherwise, according to the functions performed by the viscera. If an animal merely breathes, the nerve will be simple in its distribution and in its connexions; but if the office of the lungs be multiplied, if they be employed in producing the natural cries of animals, or combine with the organs of the voice so as to produce articulate language, or with the organs of the face, so as to express emotions, then the simple respiratory nerve will become complicated, forming an intricate system of nerves for the combination of many remote parts in the acts of breathing, coughing, sneezing, hiccoughing, vomiting, *&c.*

This view, owing to its simplicity, is seducing enough, but its truth must be put to the test. The author brings the question very distinctly before us in explaining the variety of functions performed in the face.

In the head and face of man, and of the higher classes of brutes, there are two distinct sets of nerves which pass to the same parts. Anatomists have hitherto supposed that this two-fold provision was for increasing the nervous power, and for securing its influence by numerous communications in parts liable to accident. But Mr. Bell, in opposition, as far as we know, to all anatomists and physiologists who have preceded him, has proved these two classes of nerves to be entirely different in functions.

By cutting across one of them he deprived the muscles of the face, and particularly of the nostril, of all power of motion in consent with the act of respiration; but this was not the only effect produced, for at the same time all that animal activity in the features which indicates passion, was destroyed. But it is very remarkable, that whilst those particular functions of the muscles of the face were suppressed by cutting one set of nerves, the muscles still retained their full power when employed in other functions, as, for example, in the act of eating. The next set of experiments is equally curious. Upon cutting across the other class of nerves, leaving that which controls the action of respiration entire, the sensibility of the skin and the action of the muscles in mastication were destroyed, whilst all those operations which we might suppose are the most delicate, as belonging to the act of breathing and expression, continued unimpaired.

We need scarcely add that these experiments were made upon brutes, but numerous examples of injury or disease of the nerves of the human face prove, that the same distinction or classification of functions exists in the human countenance.

We look forward with much interest to the promised paper on the Nerves of the Neck and Throat—and shall then endeavour to lay before our readers more detailed observations on the new views which Mr. Bell's investigations suggest to the physiologist, and on their probable importance in the practice of physic and surgery.

XVI. *Farther Researches on the Magnetic Phenomena produced by Electricity, with some new Experiments on the Properties of electrified Bodies in their Relations to conducting Power and Temperature.* *By* Sir H. DAVY, Bart., P. R. S.

This is, in our opinion, by far the most important addition to the discoveries in electro-magnetism which has been made since the announcement of galvo-magnetism by Oersted; it is a clear and perspicuous, though brief narration, of several important researches, of which we need not say more than that they are worthy of the President of the Royal Society. Of these inquiries the following is a condensed abstract.

1. Magnetic phenomena are the same, whether electricity is small in quantity and passing through good conductors of considerable magnitude; or whether the conductors are so imperfect as only to carry a small quantity of electricity; in both cases the magnetism exhibited is extremely feeble.

2. Imperfect conducting fluids do not give polarity to steel when electricity is passed through them; but electricity passed through air produces this effect.

> Reasoning on this phenomenon, and on the extreme mobility of the particles of air, I concluded, as M. ARAGO had likewise done from other considerations, that the voltaic current in air would be affected by the magnet. I failed in my first trial, which I have referred to in a note to my former paper, and in other trials made since by using too weak a magnet; but I have lately had complete success: and the experiment exhibits a very striking phenomenon.

3. Metals are well known to be capable of transmitting large quantities of electricity; and one obvious limit to this quantity is their fusion by the intensity of the heat thus excited, which intensity is, of course, in part dependant upon the medium which surrounds them; thus, a platinum wire becomes much less heated by the transmission of a certain quantity of electricity when suspended in air, than when in the exhausted receiver of an air-pump. Reasoning on this fact, it occurred to Sir Humphry,

> That by placing wires in a medium much denser than air, such as ether, alcohol, oils, or water, I might enable them to transmit a much higher charge of electricity than they could convey without being destroyed in air; and thus not only gain some new results as to the magnetic states of such wires, but likewise, perhaps, determine the actual limits to the powers of different bodies to conduct electricity, and the relations of these powers.
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> A wire of platinum of $\frac{1}{220}$, of three inches in length, was fused in air, by being made to transmit the electricity of two batteries of ten zinc plates of four inches with double copper, strongly charged: a similar wire was placed in sulphuric ether, and the charge transmitted through it. It became surrounded by globules of gas; but no other change took place; and in this situation it bore the discharge from twelve batteries of the same kind, exhibiting the same phenomena. When only about an inch of it was heated by this high power in ether, it made the ether boil, and became white hot under the globules of vapour, and then rapidly decomposed the ether, but it did not fuse. When oil or water was substituted for the ether, the length of the wire remaining the same, it was partially covered with small globules of gas, but did not become red hot.

To ascertain whether short lengths of fine wire, prevented from fusion by immersion in a cooling medium, transmitted the whole electricity of large batteries, a second independent circuit was so made from the ends of the battery by silver wires in water, that the decomposition of the water indicated the residuary electricity. It was thus found that an inch in length of platinum wire of one 220th of an inch diameter, kept cool by water, left a great residual charge of electricity in a combination of twelve batteries similar to those above mentioned; several trials shewed that it barely discharged six batteries.

4. Having therefore determined that there is a *limit* to the quantity of electricity which wires are capable of transmitting, it became easy to institute experiments on the different conducting powers of different metallic substances, and on the relation of this power to the temperature, mass, surface, or length of the conducting body, and to the conditions of electro-magnetic action.

The leading result of these researches was, that "*the conducting power of metallic bodies varied with the temperature, and was lower, in some inverse ratio as the temperature was higher.*" Thus a wire of platinum of one 220th of an inch diameter, and three inches long, discharged the electricity of two batteries when kept cool by oil; but when in air it barely discharged one battery; and some apparently paradoxical results depend upon this circumstance. Thus, let a fine wire of platinum be heated red hot in the galvanic circuit; then apply a spirit lamp to any part of it, so as to heat that part to whiteness; the consequence is, that the rest of the wire ceases to remain red hot, in consequence of the bad conducting power of the white hot portion: again, for the converse illustration, apply a piece of ice to a part of the wire, and the remainder, previously only red hot, will now become heated to intense whiteness, in consequence of the improvement in conducting power occasioned in the cool part, enabling a larger quantity of electricity to traverse the wire.

Another important fact developed in these researches relates to the great difference in the conducting power of different metals: in experiments with 6 inches of wire of $\frac{1}{220}$ inch diameter, silver discharged the electricity of 65 pairs of plates; copper and lead that of 56; tin of 12; platinum of 11; and iron of 9; all the wires being kept cool by immersion in water. These facts lead to others connected with the magnetic energies of the different wires which are not a little curious. If wires of the different metals be placed successively in the voltaic circuit the best conductors become most powerfully magnetic; that is, they take up the largest quantity of iron filings; so that in this way a silver wire becomes an infinitely more powerful magnet than one of iron. In a powerful voltaic circuit, (the wires not being capable of carrying off the whole of its electricity) two inches of silver wire of 1-30th inch diameter took up 32 grains of iron filings; a similar wire of copper only attracted 24 grains; of platinum 11; and of iron only $8\frac{2}{10}$.

There are various other important topics treated of in this paper; more especially the production of heat in various media by the transmission of electricity. That different wires become very differently heated when placed in the voltaic circuit was previously demonstrated by Mr. Children in the magnificent series of experiments which he instituted at Tunbridge with batteries of large plates, and of which an account is given in the *Philosophical Transactions* for the years 1809 and 1815*, and he ingeniously referred this variation of heat to the variable resistance opposed to the passage of electricity, assuming the elevation of temperature to be inversely proportionate to the conducting power. "The greatest heat, however," says Sir H. Davy, "is produced in air, where there is reason to suppose the least resistance, and as the presence of heat renders bodies worse conductors, another view may be taken; namely, that the excitation of heat occasions the imperfection of the conducting power. But till the causes of heat and of electricity are known, and of that peculiar constitution of matter which excites the one, and transmits or propagates the other, our reasoning on this subject must be inconclusive."

This is the concluding paper of the *Philosophical Transactions* for the year 1821. The hasty sketch of its contents in this and in our preceding Number will enable our readers to appreciate the novelty and importance of the papers printed in this volume; and will serve, we hope, as an index of reference to its valuable and multifarious information.

In taking our leave of this publication we seize the opportunity which it suggests of congratulating the members of the Royal Society upon the dignified independence and exalted station to which that learned body has attained, and which it promises to preserve with untarnished and even increasing splendour.

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* The facility with which an important discovery may escape the most vigilant observers is brought to our recollection by the multiplied attempts that were made during these experiments to influence the magnetic needle by the voltaic pile: they failed, because the separate poles only were presented to it; had the needle been at hand when the bar of platinum of $2\frac{1}{2}$ inches length, and $\frac{276}{1000}$ inch diameter, was ignited by this powerful apparatus, the whole of the phenomena might have been developed, and under circumstances most favourable to their investigation.

Art. XV. ASTRONOMICAL AND NAUTICAL COLLECTIONS. No. VIII.

i. *Report on the present State of Ramsden's Dividing Engine. By Dr.* W. H. Wollaston. *Published by Permission of the* Board of Longitude.

Since the Dividing Engine belonging to the Board of Longitude appeared to Captain Kater and myself, from our examination of the divisions made by it, to be more defective than any other engine examined by the same means, we thought it desirable that this engine should be used a second time, with all possible care, to re-divide the double circle constructed for the purpose of this inquiry.

Mr. Cary readily undertook to repeat the operation, with attention to all the precautions that appeared necessary.

In the measurement of this second set of divisions, which I commenced shortly afterwards, I discovered that the divisions upon the outer circle, though made at the same time with those on the inner circle, and by the same motion of the cutting-point, were not coincident with them, as had been presumed to be the case in our former examination; and that it consequently became necessary to alter the mode of examination, which had been pursued under that presumption.

It is in fact the inner circle alone that really needs to be measured. For, since the motion of the cutting-point in marking the divisions is from the centre outwards, the inner circle must be regarded as a more correct copy of the engine than the outer, of which the divisions are liable to be misplaced by any irregularity, given to the motion of the cutter, in its passage over the line of contact between the inner and outer circle. In any examination by aliquot parts, (as by 6ths,) when any one arc on the outer circle has been compared with each arc of the inner in succession, and the differences measured, the sum of these differences divided by their number shows the error of the outer arc so compared, and thence the value of each inner arc, to which it has been compared, is ascertained.

In this method the number of measures, really necessary to be taken, is the same as before, but they occasion considerably more trouble than so many taken in one position of the instrument, from the necessity of placing the circles at a new point of coincidence previously to each measurement.

In my examination, however, I did not limit myself to the mere comparison of a single arc, but in each position I measured the errors of each aliquot part, so that, in fact, the number of measures actually taken, in an examination by 5ths, 6ths, 8ths, and 9ths, was $5^2 + 6^2 + 8^2 + 9^2$, the mean of which ensures greater accuracy in the results. The total amount of the errors on 22 points, obtained by this estimate of the divisions of this circle, after correction for common and central error, was found to be 78 seconds, or about $3\frac{1}{2}$ seconds, on an average, for each division.

By subsequent examination of another set of points, beginning from 12° (instead of 0) in the same manner, I found a total of errors 95″, or $4\frac{1}{3}$ on an average; so that the mean of both examinations shows this engine to be liable to an error of about *four seconds* on each division.

In consequence of the discovery of a difference between the inner and outer circles, it became necessary to re-examine the circle divided by Allan, but the measurements that I have taken do not invalidate our former result as to the comparative merit of his mode of constructing his dividing engine. The total amount of errors, in a set of 22 points, did not exceed 42″, making the average error of his engine less than *two seconds.*

The greatest error discovered on Allan's circle was $6\frac{1}{2}$ seconds, while the greatest by Ramsden's engine is as much as 11″.

Specimen of the Examination of Cary's *Second Division by Ramsden's Engine.*

0.	0	72	144	216	288	Excess of Points on Outer Circle.	(INNER CIRCLE.)	Errors of Arcs.	Errors of Points.
							+27.	+5.4	
72.	4.	1.	5.	10.	7.	27.			+5.4
							+12.	+2.4	
144.	3.	2.5	11.	14.	8.5	39.			+7.8
							+ 1.	+0.2	
216.	3.	8.	12.	13.	4.	40.			+8.0
							--27.5	−5.5	
288.	1.	3.	6.	5.	−2.5	12.5			+2.5
							−12.5	−2.5	
	36.	31.5	13.	8.	30.	36			0.

(Outer Circle.)

	+4.5	+18.5	+5.0	−22.	−6.
Errors of Arcs	+0.9	+ 3.7	+1.0	− 4.4	−1.2

Errors of Points . .	+0.9	+4.6	+5.6	+1.2
Do. Inner Circle . .	5.4	7.8	8.0	2.5
Diff. computed . . .	4.5	3.2	2.4	1.3
Do. observed Col. I.	4.	3.	3.	1.
	0.5	0.2	0.6	0.3

Specimen of the Examination of Cary's *Second Division by Ramsden's Engine.*

0.	0	60	120	180	240	300	Excess of Points on Outer Circle.	(INNER CIRCLE.)	Errors of Arcs.	Errors of Points.
							—	+14.5	+2.4	
60.	3.	−4.	3.	8.	1.	3.5	14.5			+ 2.4
								+47.	+7.8	
120.	4.	4.	15.5	16.	10.	12.	61.5			+10.2
								−24.	−4.0	
180.	1.	5.	10.	13.	6.5	2.	37.5			+ 6.2
								−26.	−4.3	
240.	3.	0.	6.	9.	−3.	−3.5	11.5			+ 1.9
								− 7.	−1.2	
300.	1.	0.	6.	1.	−5.	1.5	4.5			+ 0.7
								− 4.5	−0.7	
	32.5	36.5	0.	−0.5	33.	28.	32.5			0.
(Outer.)	−4.	+36.5	+0.5	−33.5	+5.0	−4.5				
Errors of Arcs	−0.7	+ 6.1	+0.1	− 5.6	+0.8	−0.7				
Errors of Points . . .		−0.7	+ 5.4	+5.5	−0.1	+0.7				
Do. of Inner Circle .		2.4	+10.2	+6.2	+1.9	+0.7				
Computed diff. . . .		3.1	4.8	0.7	2.0	0.				
Observed do. 1st Col.		3.	4.	1.	3.	1.				
		0.1	0.8	0.3	1.0	1.0				

Allan—*Measured 5th February*, 1821.

						Excess of Points on outer Circle.	(INNER CIRCLE.)	Errors of Arcs.	Errors of Points.
0.	0	72	144	216	288				
							−16.5	−3.3	
72.	0.5	7.	4.5	3.5	1.0	16.5			−3.2
							+17.	+3.4	
144.	2.	4.5	1.	−3.	−5.	− 0.5			+0.1
							+ 8.	+1.6	
216.	2.	3.5	−4.5	−7.	−2.5	− 8.5			+1.7
							+ 8.5	+1.7	
288.	0.	− 1.5	−7.	−4.5	−4.	−17.			+3.4
	−8.0	−18.5	+2.	+7.	+8.0	−8.0	−17.	−3.4	

(Outer Circle.)					
	−10.5	+20.5	+5.	+1.	−16.
Errors of Arcs	− 2.1	+ 4.1	+1.0	+0.2	− 3.2
Errors of Points	−2.1	+2.0	+3.0	+3.2	0
Do. Inner Circle	−3.2	+0.1	+1.7	+3.4	
Diff. computed	1.1	1.9	1.3	0.2	
Do. observed Col. I. .	0.5	2.0	2.0	0	
	0.6	0.1	0.7	0.2	

N.B.—Since Allan's divisions are numbered from left to right, the sign + shows that the *outer* circle is in excess, and requires to be changed with reference to the inner circle.

Allan—*Measured 5th February*, 1821.

0.	0	60	120	180	240	300	Excess of Points on Outer Circle.	(INNER CIRCLE.)	Errors of Arcs.	Errors of Points.
								−17.5	−2.9	
60.	+1.	7.	3.	2.5	1.	3.	+17.5			−2.9
								+8.5	+1.4	
120.	2.5	5.5	3.	1.	−1.5	−1.5	+ 9.			−1.5
								+5.3	+0.9	
180.	2.7	4.5	−0.5	−1.	−4.	2.	+ 3.7			−0.6
								+3.4	+0.6	
240.	3.3	2.	−2.0	−3.5	−1.	1.5	+ 0.3			+0.0
								+1.7	+0.3	
300.	0.6	2.	−4.5	+0.5	−1.	1.	− 1.4			+0.3
								−1.4	−0.2	0
	+0.5	−12.5	+11.0	+11.7	+14.3	+4.1	+0.5			
(Outer Circle.)	−13.	+23.5	+0.7	+2.6	−10.2	−3.6				
Errors of Arcs .	− 2.2	+ 3.9	+0.1	+0.4	− 1.7	−0.6				
Errors of Points		−2.2	+1.7	+1.8	+2.2	+0.6	0			
Do. Inner Circle		−2.9	−1.5	−0.6	0	+0.3				
Diff. computed		0.7	3.2	2.4	2.2	0.3				
Do. observed Col. I. . .		1.0	2.5	2.7	3.3	0.6				
		0.3	0.7	0.3	1.1	0.3				

Results of the Measures of Cary's *second and best Division by Ramsden's Engine.*

	INNER CIRCLE.					Corrected by common Error −4.7.	Central Error 6 at 150°	Corrected for central Error.	
	(5)	(6)	(8)	(9)					
0						− 4.7	+ 5.2	+ 0.5	
40				3.6	+ 3.6	− 1.1	+ 2.2	+ 1.1	
45			2.2		+ 2.2	− 2.5	+ 1.5	− 1.0	
60		2.4			+ 2.4	− 2.3	0	− 2.3	
72	5.4				+ 5.4	+ 0.7	− 1.3	− 0.6	
80				7.5	+ 7.5	+ 2.8	− 2.1	+ 0.7	
90			10.4		+10.4	+ 5.7	− 3.0	+ 2.7	
120		10.2		9.6	+ 9.9	+ 5.2	− 5.2	0	
135			11.3		+11.3	+ 6.6	− 5.7	+ 0.9	
144	7.8				+ 7.8	+ 3.1	− 5.9	− 2.8	
160				5.7	+ 5.7	+ 1.0	− 5.9	− 4.9	
180		6.2	7.1		+ 6.6	+ 1.9	− 5.2	− 3.3	
200				8.1	+ 8.1	+ 3.4	− 3.8	− 0.4	
216	8.0				+ 8.0	+ 3.3	− 2.4	+ 0.9	
225			3.9		+ 3.9	− 0.8	− 1.5	− 2.3	
240		1.9		1.4	+ 1.6	− 3.1	− 0	− 3.1	
270			3.3		+ 3.3	− 1.4	+ 3.0	+ 1.6	
280				4.9	+ 4.9	+ 0.2	+ 3.8	+ 4.0	
288	2.5				+ 2.5	− 2.2	+ 4.4	+ 2.2	
300		0.7			+ 0.7	− 4.0	+ 5.2	+ 1.2	
315			−1.4		− 1.4	− 6.1	+ 5.7	− 0.4	
320				−1.4	− 1.4	− 6.1	+ 5.9	− 0.2	
360					108.6	68.2		36.2	= 78″

Average 3½ seconds.

Results of the Measures of Allan's *Circle, February*, 1821.

	INNER CIRCLE.					Corrected by common Error 0.6	Distance from 135°	Central Error 2. at 45°	Corrected for central Error.	
	(5)	(6)	(8)	(9)						
0						+0.6	45	+1.5	+2.1	
40				−3.9	−3.9	−3.3	85	+2.0	−1.3	
45			−3.2		−3.2	−2.6	90	+2.0	− .6	
60		−2.9			−2.9	−2.3	75	+1.9	− .4	
72	−3.2				−3.2	−2.6	63	+1.6	−1.0	
80				−2.1	−2.1	−1.5	55	+1.5	0	
90			−1.2		−1.2	−0.6	45	+1.4	+ .8	
120		−1.5		−1.8	−1.7	−1.1	15	+ .5	−0.6	
135			−1.5		−1.5	−0.9	0	0	− .9	
144	+0.1				+0.1	+0.7	9	− .3	+ .4	
160				−1.0	−1.6	−1.0	25	− .8	−1.8	
180		−0.6	−0.6		−0.6	0	45	−1.5	−1.5	
200				−1.1	+1.1	+1.7	65	−1.8	− .1	
216	+1.7				+1.7	+2.3	81	−1.9	+ .4	
225			+1.0		+1.0	+1.6	90	−2.0	− .4	
240		0		−0.1	−0.1	+ .5	75	−1.9	−1.4	
270			+0.9		+0.9	+1.5	45	−1.5	0	
280				+1.6	+1.6	+2.2	35	−1.1	+1.1	
288	+3.4				+3.4	+4.0	27	− .9	+3.1	
300		+0.3			+0.3	+ .9	15	− .5	+ .4	
315			−0.9		−0.9	− .3	0	0	− .3	
320				−1.2	−1.2	− .6	5	+ .2	− .4	
					34.2				19.	= 40″

Greatest 6½ seconds : average less than 2 seconds.

ii. Mr. Ivory's *Method of computing the Latitude from two Altitudes and the Time intervening.*

Mr. Ivory, after a candid examination of the various approximations that have been proposed for the solution of the problem of two altitudes, is disposed to think them seldom so eligible as the direct computation, which he has somewhat abridged by a method that may be thus enunciated. (Phil. Mag. *Aug.* p. 84.)

Mr. Ivory's Rule.

Putting h the greater altitude.
h' the lesser,
D the mean declination,
t the half time elapsed, reduced to angular space,
(b the half base of the triangle A P B,
p the perpendicular,
y the perpendicular Z D falling from the zenith on p,
x the portion of p between y and A B,)
λ the latitude,
s the horary angle of the middle time,
$A = \frac{1}{2}(\sin h - \sin h')$
$B = \frac{1}{2}(\sin h - \sin h')$

$$1.\ \text{Sin}\, b = \cos D \sin t$$

$$2.\ \text{Cos}\, p = \frac{\sin D}{\cos b}$$

$$3.\ \text{Sin}\, y = \frac{B}{\sin b}$$

$$4.\ \text{Cos}\, x = \frac{A}{\cos y \ \cos b}$$

$$5.\ \text{Sin}\, \lambda = \cos y \ \cos(p \mp x)$$

$$6.\ \text{Sin}\, s = \frac{\sin y}{\cos \lambda}$$

Correction for the change of declination.

d the declination at B the greater altitude,

$\lambda - (D - d)\,\dfrac{\sin s}{\sin t}$ will be the true latitude,

$s + (D - d)\cos s\,\dfrac{\tan \lambda}{\sin t}$ the corrected horary angle.

DELAMBRE'S EXAMPLE. (*Astr. Coll.* No. 5.)

$h = 42° 14' 9''$, $h' = 16° 5' 47''$
$D = 8° 16' 30''$, $d = 8° 15'$
$t = 22° 30'$

Sin h	672184	(1)
sin h'	277244	(2)
2 A	949428	
2 B	394940	
A	474714	
B	197470	

L. cos D		9.995455	(3)
sin t		9.582840	(4)
sin $b = 22°15'11''$		9.578295	(5)
L. sin b	A. C.	0.421705	
B		9.295501	(9)
sin $y = 31°25'44''$		9.717206	(10)
L. cos y		9.931095	
cos $(p - x)$		9.945923	(14)
sin $\lambda = 48° 53' 5''$		9.877018	

L. cos b		9.966386	(6)
	A. C.	0.033614	
sin D		9.158134	(7)
cos p 81° 3′ 14″		9.191748	(8)
L. cos b		9.966386	
cos y		9.931095	(11)
		9.897481	
	A. C.	0.102519	
A		9.676432	(12)
cos $x = 53° 3' 3''$		9.778951	(13)
$p - x = 28$ 0 11			

CORRECTION.

L. sin y		9.7172
cos. λ	A. C.	0.1821
sin s		9.8993
sin t	A. C.	0.4172
2.073		0.3165

$D - d = 90''$
2.073
$186.57 = 2' 47''$

48	53	5
48	50	18

Exceeding the truth by 18″ only.

iii. *Apology for the* POSTSCRIPT *on* REFRACTION, *in answer to Mr.* IVORY'S *Remarks.*

A mathematician of Mr. Ivory's acknowledged celebrity and transcendent attainments might naturally be expected, like the Midas of antiquity, to convert whatever he touches into gold:

but if he should employ his alchemical powers in converting the gold of others into brass, it would not be surprising if it should again be reported, as it was of old of the same Midas, that "*Auriculas asini Mida rex habet;*" and that the punishment should be remembered even longer than the alchemy.

Supposing for a moment that Mr. Ivory is right in condemning the series which has been employed for the computation of refraction, and supposing that his observations on the series "do not bear at all upon the table of refractions published in the Nautical Almanac for 1822; it is still scarcely credible that any mathematician, besides Mr. Ivory, will be hardy enough to assert that "the formula and the table must both be considered as *entirely empirical.*" *Phil. Mag.* Sept. p. 167.

Can it possibly be believed that the most skilful empiric, even with the advantage of all the refined methods of empiricism that were cultivated by Lagrange, could ever have deduced, from a table of refractions only, a formula so complicated as this;

$$A = v\frac{r}{s} + \left(B + \tfrac{1}{2}v^2\right)\frac{r^2}{s^2} + Cv\frac{r^3}{s^3} + \tfrac{1}{2}C\left(B + \tfrac{1}{2}v^2\right)\frac{r^4}{s^4}\ ?$$

The values of B and C only are determined empirically; but was Euler's theory of the moon entirely empirical, because some of the coefficients were determined from observation only?

Again, the only improvement, which the author of the table considers as of practical importance, is the introduction of an accurate and convenient mode of correcting for the heights of the barometer and thermometer. Will Mr. Ivory call this approximate correction *in any degree* empirical? He may assert that this computation is independent of the former: but it is, at least, an essential part of the table: a table on which even Mr. Ivory has condescended to bestow the conditional commendation, that "if it has a real and solid foundation, it must be allowed that no greater or more honourable testimony can be given in favour of—*the French Astronomers!*"

The French Astronomers really stand so little in need of this honourable testimony, that they will probably forgive the author of the table if he persists in withholding it; and its claim to the confidence of the public may safely be allowed to rest upon the acknowledged sufficiency of the French table, with which it agrees, in all ordinary cases, and upon its coincidence with the mean of 156 observations of Mr. Pond in low altitudes, as well as with Bradley's empirical correction for temperature near the horizon.

Nor would it be necessary to inquire whether or no the series is capable of being employed with convenience for an atmosphere of uniform temperature, in order that it may be "whispered into a pit" that an unjust judge has been *metamorphosed.* But the thing is really so easy, in comparison with other modes of computation, that it is not fair to the question to omit a further exemplification of the method proposed, with the assistance of the subdivision of the operation into two parts, which has been already suggested: and the whole may be resumed with little difficulty, from the first elements of the problem. We shall have, therefore,

x, the distance from the centre,
y, the superincumbent weight,
z, the density, here represented by y,

$$\mathrm{d}y = -my\mathrm{d}x; \ \frac{\mathrm{d}y}{y} = -m\mathrm{d}x; \ \mathrm{hl}y = m - mx.$$

u, the perpendicular to the direction,
$s = u^{\smile}$, the initial value of u,

$$u = \frac{s}{1 + pz} = \frac{s}{1 + py} = s - psy$$

$$\mathrm{d}u = -ps\mathrm{d}y$$

$$\mathrm{d}y = \frac{-\mathrm{d}u}{ps}$$

$$\mathrm{d}x = \frac{-\mathrm{d}y}{my}$$

$$v = \surd\,(x^2 - u^2)$$

$$\mathrm{d}r = \frac{\mathrm{d}u}{v}$$

$$\frac{\mathrm{d}u}{\mathrm{d}r} = v$$

$$\frac{\mathrm{d}x}{\mathrm{d}r} = \frac{\mathrm{d}u}{mpsy\mathrm{d}r} = \frac{v}{mpsy}$$

$$\frac{dy}{dr} = \frac{-v}{ps}$$

$$v^2 = x^2 - u^2 \text{ ; } vdv = xdx - udu$$

$$\frac{dv}{dr} = \frac{xdx}{vdr} - \frac{udu}{vdr} = \frac{x}{mpsy} - u \text{ ; or } = \frac{x}{mpsy} - s$$

$$d\,\frac{dv}{dr} = \frac{dx}{mpsy} - \frac{xdy}{mpsy^2} - du$$

$$\frac{ddv}{dr^2} = \frac{v}{m^2p^2s^2y^2} + \frac{xv}{mp^2s^2y^2} - v \text{ ; or } = \frac{v}{mp^2s^2y^2} \text{ only.}$$

$$d\,\frac{ddv}{dr^2} = \frac{dv}{mp^2s^2y^2} - \frac{2vdy}{mp^2s^2y^3}$$

$$\frac{d^3v}{dr^3} = \frac{1}{m^2p^3s^3y^3} - \frac{u}{mp^2s^2y^2} + \frac{2v^2}{mp^3s^3y^3}$$

$$d\,\frac{d^3v}{dr^3} = \frac{-3dy}{m^2p^3s^3y^4} + \frac{2udy}{mp^2s^2y^3} + \frac{4vdv}{mp^3s^3y^3} - \frac{6v^2dy}{mp^3s^3y^4}$$

$$\frac{d^4v}{dr^4} = \frac{3v}{m^2p^4s^4y^4} - \frac{2uv}{mp^3s^3y^3} + \frac{4v}{m^2p^4s^4y^4} - \frac{4uv}{mp^3s^3y^3}$$

$$+ \frac{6v^3}{mp^4s^4y^4} = \frac{7v}{m^2p^4s^4y^4} - \frac{6uv}{mp^3s^3y^3} + \frac{6v^3}{mp^4s^4y^4}$$

$$d\,\frac{d^4v}{dr^4} = \left(\frac{7}{mpsy} - 6u + \frac{18v^2}{psy}\right)\frac{dv}{mp^3s^3y^3} - \left(\frac{28v}{mpsy} - 18uv\right.$$

$$\left. + \frac{24v^3}{psy}\right)\frac{dy}{mp^3s^3y^4}$$

$$\frac{d^5v}{dr^5} = \left(\frac{7}{mps} - 6s + \frac{18v^2}{ps}\right) . \left(\frac{1}{m^2p^4s^4y^4} - \frac{s}{mp^3s^3y^3}\right)$$

$$+ \left(\frac{28}{mpsy} - 18s + \frac{24v^2}{psy}\right) \frac{v^3}{mp^4s^4y^4}.$$

We shall find no difficulty in substituting for the coefficients of these formulas the numerical values employed by Laplace, and quoted by Mr. Ivory, that is $p = .000293876$ and $\frac{1}{m} = .00125254$; whence $\frac{1}{p} = 3403$, $\frac{1}{mp} = 4.2624$, $\frac{1}{m^2p^2} = 18.168$, $\frac{1}{mp^2} = 14505$, $\frac{1}{m^2p^3} = 61826$, $\frac{1}{mp^3} = 49\,360\,700$, and $\frac{1}{m^2p^4} = 210\,395\,000$.

The formula $p\,s\,\Delta\,y = vr + \frac{dv}{dr} \cdot \frac{r^2}{2} + \frac{d^2v}{dr^2} \cdot \frac{r^3}{2.3} + \ldots$ will then become

$$
\begin{aligned}
.00029388\,\Delta\, y = {} & v\,\frac{r}{s} + (2.1312 - .5\,s^2)\,\frac{{}^{2}}{s^2} + 2417.5\,v\,\frac{r^3}{s^3} \\
& + (2576 - 606\ s^2 + 4\,113\,400\,v^2)\,\frac{r^4}{s^4} \\
& + (12\,273\,040 - 2\,468\,000\,s^2 + 10\,570\,000\,v^2)\,v\,\frac{r^5}{s^5} \\
& + ([2\,045\,500 - 411\,333\,s^2 + 5\,260\,000\,v^2] \,.\, (4.2624 - s^2) \\
& + (8\,182\,000 - 1\,234\,000\,s^2 + 7\,013\,200\,v^2)\ 3403\,v^2)\frac{r^6}{s^6} \\
& + \,.\,.\,.
\end{aligned}
$$

In order to compute the horizontal refraction in two portions from this formula, supposing its magnitude to be about .01, we may begin by taking half of this quantity, and make $r = .005$, and $r^2 = .000025$, s being $= 1$, and $v = 0$; we shall then have $p\,\Delta\, y = .00004578 + .000001231 + .000000082 + \ldots$

Now an important question is here to be considered; whether we are to content ourselves with simply adding together these terms as constituting the whole series, or whether it is justifiable to assume something more from analogy, or from probability, upon general principles, excluding all arbitrary conjectures depending on private reasons or imaginations.

If we admit the propriety of interpolating a table by the method of differences, which has seldom been called in question, it can hardly be denied that, with proper caution, the method of differences may very safely be applied to the continuation of a converging series. Supposing the progression to be very nearly geometrical, it cannot be questioned that there is a probability, approaching practically to a certainty, that we shall be much more correct if we suppose it to continue strictly as a geometrical progression, than if we break it off abruptly: and when the difference from a geometrical progression is more considerable, it seems at first sight natural to take the ratios or logarithms of the quotients, and to continue them by means of the differences. But where, as in the present instance, there are only three terms computed, and we have only one difference of these logarithms, the continual employment of this difference must be erroneous, because it will always ultimately lead us to a diverging series, affording the succeeding terms too large in proportion to the preceding. If, however, we compute one term only by this approximation, and suppose the remainder of the series to become geometrical, there will be a compensation of errors, which for ordinary purposes, will be amply sufficient, and which may often save us great labour in the computation of new terms.

Thus we have $\frac{45780}{1231} = 37.2$, and $\frac{1231}{82} = 15$; and 37.2 : 15 :: 15 : 6, so that we may safely assume 6 as the quotient of a geometrical progression nearly equivalent to the proposed series, its sum being equal to the last given term divided by $6 - 1$, or here to .000000016, making the whole .00004711, which must undeniably be true to the last place, as far as the convergency of the series is concerned. From this value of $p \Delta y$ we have $\Delta y = .16031$, $y = .83969$, $\text{hl}\, y = - .174842$, and $x = 1 - \frac{\text{hl}\, y}{m} = 1 + .174842 \times .00125254 = 1.0002190$; but since $u = 1 - \Delta u = 1 - p \Delta y, = .99995289$, we have, for the new value of s, $\frac{u}{x} = .9997339$, and $v = .02307$; and proceeding with these values to compute $p \Delta y$, on the supposition $\Delta r = .005$, we have the series $.00011535 + .00004079 + .00000697 + .00000260 + .00000071 + .00000028 + [.00000040] = .00016710$, instead of $.00029388 - .00004711 = .00024677$, consequently .005 is much too little, and we must multiply the terms by the powers of $\frac{.006}{.005}$ or $\frac{.007}{.005}$, and the sums will become .0002188 and .0002802 respectively, giving by interpolation .006456 for r; but it will be necessary to repeat the operation with $r = .0065$, which gives us .0002453, and requires the further correction of .000025 only, making the whole refraction .011525 or 39′ 37″, which can scarcely differ above a second or two from the truth, the terms actually computed showing that it *must* be *less* than 39′ 45″, and the remainder being capable of a very sufficient estimation. Laplace's result is 39′ 54″,6; so that there must probably be some numerical error in one of the computations; at any rate the difference does not arise from the want of convergence of the series.

It may therefore be left with confidence to the decision of every impartial mathematician, to an Olbers, a Bessel, or a Brinkley, whether or no the convergence of the series is not sufficiently exemplified, to prove its utility for any computations of this kind that may be required. And even if four subdivisions had been employed, or twice four, instead of two, the labour of computation would still have been trifling, in comparison with the complicated expedients that are required in other methods.

iv. *The Variation of the* TEMPERATURE *of the Atmosphere deduced from the mean* REFRACTION.

The best proof that the formula, published in the Nautical Almanac, is not deserving of the reproach of being merely *empirical,* will be found in the facility that it affords of deducing the actual density of the air, at a given height, from the table of astronomical refractions. For, by comparing the original series $p = v\frac{r}{s} + \left(\frac{\zeta}{mp} - s^2\right)\frac{r^2}{2s^3} + \left(\frac{\zeta' s}{mp} + \frac{\zeta v}{mp^2}\right)\frac{r^3}{6s^3} + \ldots,$ with the expression in the Nautical Almanac, $p = v\frac{r}{s} + (2.47 + .5\,v^2)\,\frac{r^2}{s^2} + 3600\,v\,\frac{r^3}{s^3} + \ldots,$ we obtain $\frac{\zeta}{2\,mp} = 2.97,$ $\frac{\zeta' s}{6\,mp} + \frac{\zeta v}{6\,mp^2} = 3600\,v,$ $\frac{\zeta'}{mp} = 2484\,\frac{v}{s}, = 1.2855,$ and $\zeta' = 537.6\,\frac{v}{s}$; and from these values we may immediately deduce the diminution of the temperature in ascending, and the rate at which that diminution varies at different heights, without any hypothesis whatever respecting the constitution of the atmosphere.

The number of feet, in which the temperature is depressed a degree of Fahrenheit, being called f, we have $\frac{1}{494\,f}$ for the variation of density in a foot dependent on this cause; but the variation of the pressure y for a foot is $\Delta y = \frac{m}{20\,900\,000} = \frac{1}{27300}$; and this, if the temperature were uniform, would be the value of Δz, the increment of the density, which is actually only $\frac{1}{27300} - \frac{1}{494f}$; hence $\frac{\Delta z}{\Delta y} = \frac{\mathrm{d}z}{\mathrm{d}y} = \frac{1}{\zeta} = 1 - \frac{27300}{494f}, \frac{27300}{494f} = 1 - \frac{1}{\zeta}, 494f = 27300 : \left(1 - \frac{1}{\zeta}\right), f = 55.26 : \left(\frac{\zeta - 1}{\zeta}\right) = 55.26\left(\frac{\zeta}{\zeta - 1}\right) = 248$, at the surface of the earth; and since $\zeta' = \frac{\mathrm{d}\zeta}{\mathrm{d}r}$ and $\frac{\mathrm{d}z}{\mathrm{d}r} = \frac{-v}{ps}$, we have $\frac{\mathrm{d}\zeta}{\mathrm{d}z} = \frac{-ps\zeta'}{v} = -537.6p$; whence, if we neglect the higher fluxions of ζ, we have for the point at which the density is reduced to one half, putting $\Delta z = -\frac{1}{2}$, $\Delta\zeta = 268.8\,p = .0759$; and ζ becomes 1.3614, giving $f = 208$ feet, for the elevation corresponding to a variation of temperature of 1° at the supposed altitude, which would probably be three or four miles above the surface. But, in order to compute the height from the density, or from the pressure, with greater convenience, it will be easiest to make the fluxion of y constant, so that we may obtain a formula for the height as depending upon the indication of the barometer. Thus, since $\mathrm{d}x = -\frac{\mathrm{d}y}{mz}, \frac{\mathrm{d}x}{\mathrm{d}y} = -\frac{1}{mz}$, $\mathrm{d}\frac{\mathrm{d}x}{\mathrm{d}y} = \frac{\mathrm{d}z}{mz^2} = \frac{\mathrm{d}y}{mz^2\zeta}$, and $\frac{\mathrm{d}^2x}{\mathrm{d}y^2} = \frac{1}{mz^2\zeta}$, whence $\mathrm{d}\frac{\mathrm{d}^2x}{\mathrm{d}y^2} = -\frac{2z\zeta\mathrm{d}z + z^2\mathrm{d}\zeta}{mz^4\zeta^2} = -\frac{2z\mathrm{d}y + z^2\zeta'\mathrm{d}r}{mz^4\zeta^2}$, and $\frac{\mathrm{d}^3x}{\mathrm{d}y^3} = \frac{-2}{m\zeta^2} - \frac{\zeta'}{m\zeta^2} \cdot \frac{\mathrm{d}r}{\mathrm{d}y} = \frac{p}{m\zeta^3} \cdot \frac{s}{v} \cdot \zeta' - \frac{2}{m\zeta^2}$: consequently $\Delta x = -\frac{\Delta y}{m} + \frac{\Delta y^2}{2m\zeta} + \left(\frac{p}{6m\zeta^3} \cdot \frac{s}{v} \cdot \zeta' - \frac{1}{3m\zeta^2}\right)\Delta y^3$, $x = 1$ being the radius of the earth; or, in feet, $\frac{\Delta x}{20\,900\,000} = -\frac{\Delta y}{766} + \frac{\Delta y^2}{1970} - \frac{\Delta y^3}{40371}$, and $\Delta x = -27300\,\Delta y + 10608\,\Delta y^2 - 518\,\Delta y^3$; or, if D be the descent of the barometer divided by the greater height, $H = 27300\,D + 10608\,D^2 + 518\,D^3$, since Δy is negative, and its square positive. Taking, for example, $D = \frac{1}{2}$, we have $H = 13650 + 2652 + 65 = 16367$ feet, or 3.1 miles, which appears to be not far from the truth: at Quito the barometer stands at 21.37, which gives a height of 8740 feet, instead of

9377; or if we augment the 8740 in the proportion of the expansion of air from 50° to 80°, it will become 9264; but it is well known that in computations of this kind it is necessary to introduce a variety of subsidiary corrections. The principal of these, however, is the reduction of the height of the barometer for temperature, and when this is applied, the result of the formula thus employed appears to be extremely accurate. Taking, for example, General Roy's observations on Moel Eilio, at the height of 2371 feet: the temperature, below, 68°, the corrected heights of the barometer 29.918 and 27.468, we have $D = .082$, and $H = 2306$, to which adding $\frac{18}{494}$, or 84, for the excess of the lower barometer above 50°, we have 2390, while General Roy's computations make the height 2393. In one of Mr. Greatorex's examples, the error of this method is 6 yards, while that of Dr. Maskelyne's is 12.

It may not be improper to observe that the correction of the refraction for temperature, as applied in the Nautical Almanac, may possibly be found to agree better with the mean temperatures of different climates than with the occasional variations at the same place, which may often be less regular in their causes and extent: and there is reason to think that, in some of these cases, the correction of $\frac{1}{400}$ for each degree of Fahrenheit, as employed by Bradley, is sufficient in the immediate neighbourhood of the horizon. Mr. Groombridge's observations, as published in the *Connaissance des Tems* for 1821, require the standard temperature of the table to be supposed 50°, when compared with the *exterior* thermometer, and not 48°, as the Greenwich observations seem to indicate. But it is at present impossible to expect any thing like perfect accuracy in a determination so liable to uncertainties of various kinds.

v. *Account of some Optical Inventions of* Professor AMICI. *From the Memoirs of the Italian Society*, Vol. XIX.

1. The first, in importance, of Mr. Amici's papers is an account of an iconantidiptic telescope. Jeaurat's invention of a telescope that should exhibit at once two images, one erect, the other inverted, coinciding in the axis only, was improved by

Kratzenstein and Euler: but it was demonstrated by Boscovich that " an iconantidiptic telescope with three achromatic object glasses produces a much less effect than a common telescope of half the length," so that the advantage gained by the double velocity, with which the two images approach each other, is nothing more than might be obtained from a common telescope with twice or more than twice the magnifying power, as it might easily be made. He also showed that the telescope must fail in its proposed object of superseding the necessity of micrometer wires, for that the images could never be made to meet in the axis without having the assistance of a wire to guide the direction of their motion.

Mr. Amici observes that the optical difficulty might be avoided, by employing a reflecting instead of a refracting telescope, and by combining the constructions of Gregory and Cassegrain in the same instrument; but that the weight of such a telescope would render it inconvenient for astronomical observations. Still, however, the wires would be required, and the advantage of viewing very faint stars, without the danger of extinguishing their light by the illumination, would be lost. He therefore proposes to obtain a double image by reflecting half the rays, so that it may be formed in a simply inverted and wholly reversed position; and that if the plane of reflection be situated, for example, in that of the meridian, the images of all the stars in the field would meet each other at the true moment of their transit, while, in the construction of Jeaurat, none of them could coincide unless they were precisely in the axis of the tube. For this purpose he places, "in the focus of the eyepiece next to the object glass, a small rectangular isosceles prism of glass, the plane opposite to the right angle passing through the axis of the telescope, and its edges being perpendicular to it, so as to intercept half of the pencils of rays belonging to each point of the object, and to form with them a reflected image" depending on the internal reflection of the base of the prism. The instrument in this form appears to be peculiarly adapted to the observation of transits, though it may possibly require some modification both in the angle of the prism, and in its place with regard to the focus of the telescope.

2. The second memoir appears to exhibit a less fortunate attempt to obtain novelty without essential improvement. It relates to " the construction of an achromatic telescope without lenses, and with a single refractive medium." " It has hitherto been believed," says the author, " by natural philosophers, that the dispersion of colours is constant for the same refracting medium, or that a given refraction, produced by the same substance, is accompanied by a given dispersion. But I have found that the dispersion produced by more than one refraction is not by any means constant, but varies according to the various inclinations of the incident ray." He finds, however, that this property is really deducible from the constant proportion of the sines, and observes in conclusion, that " although the theory of colours has been cultivated by so many distinguished mathematicians and opticians, from the days of Newton to the present time, the property here described not only remained unknown, but the thing was judged impossible, until I discovered its practicability by means of some experiments which I was making with another view. We may therefore consider this circumstance as a striking proof, among many others, that in the prosecution of physical science, experiment is very often, and perhaps most commonly, more successful than theory, with regard to the development of all the circumstances that accompany a given phenomenon."

Now it is well known, that Euler was aware of the difference of dispersion that might be obtained in this manner from the same refractive substance, and the author himself quotes the work of our countryman, Dr. Brewster, who has entered very fully into the investigation of the subject. " The celebrated Dr. Brewster, " he says, " in his excellent Treatise on New Instruments, informs us, page 400, that he has made several attempts to exclude colours by means of an object glass composed of two lenses of the same substance; but his experiments were not crowned with the desired success."

Mr. Amici, however, appears to have been considerably more successful in a practical point of view. He informs us that " ever since the year 1815, he has made telescopes of prisms of larger and smaller angles, which have fully answered his expectations. One of them, less than an inch in length, and half an inch in breadth, composed of little prisms of French glass, with angles of 45°, affords so much distinctness and precision in the outlines of the image, that it exceeds in its effect the most perfect achromatic opera glasses."

The construction of this extraordinary machine he has not more particularly described: he has, however, explained the general principle upon which it depends, and although it may be apprehended that it can never be applied to instruments of material importance, it really appears to have afforded him an elegant little plaything.

" Supposing, that through a prism, having its axis in a vertical direction, we look at a small square object, having one of its sides also vertical, it is obvious, that if we turn the edge of the prism so as to incline it towards the object, the image will become an oblong rectangle, instead of a square. If we then take a second prism of the same substance, and place it behind the former, with its axis horizontal, and turn it until it produces an equal deviation, the image will manifestly be prolonged in a vertical direction, and will again become a square, magnified, but still coloured. Now, since a coloured spectrum of a given extent may be produced in two different ways, that is, either by turning round its axis a prism with a small angle, in order to increase its refractive effect, or by making a prism of the same substance with a larger angle, it will be easy, without recurring to the first method, which would produce a distortion of the image, to determine the angle to be given to a third prism, in order that its least refraction may produce a spectrum of equal extent to that which is formed by the two combined prisms. If then we place this third prism behind the two former, in such a mamner that its refraction may be in the direction of the diagonal of the square, it will correct the dispersion of the colours, without distorting the object, which will of course still remain magnified; so that the system of these three prisms alone will constitute an achromatic telescope consisting of a single refractive substance only."

CORRECTION of Art. v. No. VI. P. 376, Line 8 from the bottom, *for* " and consequently to the magnetic axis of the earth," *read* " and consequently to that of the dipping needle."

ART. XV. *Miscellaneous Intelligence.*

1. MECHANICAL SCIENCE.

§ MECHANICS, THE ARTS, &c.

1. *Letter from* Messrs. Parkinson and Frodsham, *to the Editor of the Quarterly Journal of Science and the Arts, relating to the Chronometers employed in the late Arctic expedition.*

SIR,

We take the liberty to address ourselves to you in the hope that our communication will be considered to fall within the plan of your Journal, which expressly concerns itself in the interests of science and of the arts.

We are the makers of the chronometers which have received so favourable a report in the Official Account of the late Arctic Expedition; confident of our workmanship, we ventured them on so severe a trial at our own expense, for the purpose of advancing our reputation; it will appear but reasonable, therefore, that we should be solicitous to possess that which we have been at much pains to obtain, and on which the prosperity of our business depends.

Soon after the return of the Expedition, it came to our knowledge that a report was in circulation, and had even obtained credit with gentlemen distinguished in science, that we were the sellers only and not the makers of the chronometers which had borne our name, and that Mr. Molyneux, previously known as a workman employed in making chronometers, was the real maker; we lost no time in obtaining and making public Mr. Molyneux's statement that he had never even seen the chronometers in question, and at the same time we asserted our own right to be considered the makers.

We hoped that we had thus terminated all doubts especially as there has been no other claimant than ourselves; it was therefore with equal surprise and concern that we read a note in the review of Captain Parry's *Voyage* in the 49th Number of the *Quarterly Review*, expressing an opinion that neither Mr. Molyneux nor ourselves, but some third person, (whom however the reviewer neither names nor specifies), was the real maker of the chronometers, the merits of which he was pleased to notice.

We might reasonably have complained that an opinion, affecting so materially our reputation and trade, should have been admitted in so respectable a work without at least more consideration and inquiry than had obviously been bestowed; we however overlooked this, and took the more moderate course of appealing to the right feeling and justice of the Editor himself in a letter which we have subjoined, requesting only that we might be permitted to prove either to himself, or to the writer of the note, that the opinion expressed in it was wholly erroneous.

We have experienced the additional mortification of finding our appeal entirely disregarded; our letter, which we trust was in all respects proper and respectful, has not received the common courtesy of a reply, and a subsequent Number of the *Quarterly Review* has appeared without notice or acknowledgment of error.

Our motive, in desiring the publication of our letter, is to remove the doubts which the *Quarterly Review* has excited to our prejudice, and to shew that our not having done so long since, has been solely occasioned by our reasonable trust in the readiness of the Editor himself to assist, when it should be shewn him that he had inconsiderately given currency to an opinion, injurious to a tradesman's reputation, and altogether without foundation.

We remain, Sir, respectfully,
Your most obedient humble servants,
PARKINSON AND FRODSHAM.

Change Alley, 26th Oct., 1821.

(COPY.)

To the EDITOR of the *Quarterly Review*.

Change Alley, July 1821.

SIR,

We beg leave to address you in consequence of the following Note in page 205 of the last Number of the *Quarterly Review*. "There is a dispute as to the real maker of these valuable chronometers. Mr. Molyneux, who has been long distinguished for the excellence of his workmanship, having set up a claim which is denied by Parkinson and Frodsham. As far as we are able to judge from their contradictory statements, we should say the real operator was neither of them but some third person."

The chronometers in question, Nos. 228, 253, 254, and 259, of our name, were sent by ourselves at our own risk and hazard on the Northern Expedition; our object was to manifest their goodness by a public trial under such extreme circumstances; we were personally unknown to the gentlemen in whose hands we placed them, but, as selected by the government on such an occasion, we did not doubt their care and disposition to do justice to our chronometers: the report which has been published of their going during the voyage has, in every respect, fully equalled our expectations.

We have stated thus much to you, in the hope that we may interest you to inquire into the justice of a note which has gone forth to the prejudice of our reputation and business, through the widely-extended circulation of the *Quarterly Review;* we have enclosed you the papers on which the author of the note states that his opinion has been formed, that the real operator was neither Mr. Molyneux nor ourselves, but some third person. Mr. Molyneux's acknowledgment that he never saw the chronometers, is decisive as to his concern in them. If we knew what third person can have been supposed the maker, we would meet the supposition by obtaining a denial from the individual himself.

We are ready, Sir, to give you, or the author of the note, any reasonable proof that may be required, that (with the exception of the springs of Nos. 253 and 254, which were obtained from Mr. Hopkins, whose distinct branch of the trade it is to supply springs to chronometer makers, and for which springs he received 2*l.* 10*s.* each), the four chronometers were our own workmanship, made in our own house, under our own direction, by workmen whom we have instructed, and that they were all finally adjusted and corrected by ourselves. We have laboured unceasingly since our entrance into life to become good artists, and to approve ourselves such to the public. The adventure of our property in the late voyage is a proof of our confidence in the excellence of our chronometers, and of our anxiety to establish our reputation. We cannot, therefore, but feel deeply when we see the credit which we have laboured to deserve, ascribed to some unknown person; and when we find the encouragement to our business, which we were receiving from the public, withheld in consequence of the promulgation of an opinion which has certainly no foundation in fact.

We hope that you will not refuse to satisfy yourself of the truth of this statement.

We are, Sir,
Your obedient servants,
PARKINSON AND FRODSHAM.

2. *Steam Engines of England.*—M. Dupin, whilst speaking of the immense mechanical force set in action by the steam engines of England, gives the following illustration of its amount. The great pyramid of Egypt required for its erection the labour of above 100,000 men for twenty years: but if it were required again to raise the stones from the quarries and place them at their present height, the action of the steam engines of England which are managed at most by 36,000 men, would be sufficient to produce the effect in eighteen hours. And M. Dupin says, that if it were required to know how long a time they would take to cut the stones and move them from the quarries to the pyramid, a very few days would be found sufficient.

The calculation of M. Dupin is as follows: the volume of the great pyramid is 4,000,000 cubic metres, its weight is about 10,400,000 tons, or 10,400,000,000 kilogrammes. The centre of gravity of the pyramid is elevated 49 metres from the base and taking 11 metres as the mean depth of the quarries, the total height of elevation is 60 metres, which multiplied by 10,400,000 tons gives 624,000,000 tons raised 1 metre. Then the total of the steam engines in England represents a power of 320,000 horses. These engines, moved for 24 hours, would raise 862,800,000 tons 1 metre high, and consequently 647,100,000 tons in 18 hours, which surpasses the produce of labour spent in raising the materials of the great pyramid.

3. *Prize Question.*—When the nozzle of a blowing machine is placed at a certain distance from that of the tuyère, a stronger current of air is obtained than when both are placed together, as is frequently done. This effect is produced by various causes dependant on the elastic nature of the fluid in motion, and of the surrounding atmosphere. The Society of Sciences and Arts at Metz have founded the following prize question on this experiment: "What are the changes necessary to be made in the tuyère of blowing machines, to introduce, in the most advantageous manner, the good effect indicated above, or any other improvement for the rapid transmission of air to greater or smaller distances." The prize is 300 francs, and is to be adjudged in April, 1822.

4. *Prize Question.*—The Royal Academy of Sciences of Toulouse has proposed as the subject of prize essays, "A physico-mathematical theory of drawing and forcing-pumps, stating the ratio between the moving power and the quantity of water elevated; attention being given to all the obstacles which the force has to overcome." Among these obstacles are enumerated, the weight and inertia of the column of water, its friction against the tubes, its contraction at the apertures of the valves, the weight and friction of the pistons, the weight of the valves, the inequality between the upper and lower surface at the moment the pressure opens them, &c." The papers are to be written in French or Latin, and sent in before May, 1823. The prize is a gold medal of 500 francs value.

5. *Lithography.*—An experiment has lately been made to take off impressions from plants by lithographic printing. A specimen of *Sibthorpia Europæa*, which was gathered several years ago in Cornwall, was covered with lithographic ink and impressed on a stone, from which stone several impressions were afterwards taken. The experiment was not so successful as was wished, but still promised to be beneficial in leading to the means of multiplying copies of the impressions of plants,

much more accurate, in some respects, than a drawing can be expected to be.—*Phil. Mag.*, Sept., 235.

6. *Preservation of Milk.*—The following method is recommended for the preservation of milk, either at sea or in warm climates. Provide pint or quart bottles, which must be perfectly clean, sweet, and dry; draw the milk from the cow into the bottles, and as they are filled, immediately cork them well up, and fasten the corks with packthread or wire. Then spread a little straw on the bottom of a boiler, on which place the bottles, with straw between them, until the boiler contains a sufficient quantity. Fill it up with cold water; heat the water, and as soon as it begins to boil, draw the fire, and let the whole cool gradually. When quite cold, take out the bottles, and pack them with straw or saw-dust in hampers, and stow them in the coolest part of the ship, or in a cool place. Some years since, there was a Swedish or Danish vessel at Liverpool, having milk on board preserved in this manner; it had been carried twice to the West Indies and back to Denmark, and had been above eighteen months in the bottles; nevertheless, it was as sweet as when first milked from the cow.—*New Monthly Mag.*, 316.

7. *Preservation of Cauliflowers.*—These vegetables have been preserved two or three months by digging a trench under a wall, eighteen inches wide and deep, laying in the cauliflowers with the stems inclined upwards, and covering the whole in with earth, heaping up the surface in an inclined form, so that the rain should run off.

8. *Use of Larch Bark in Tanning.*—Mr. E. Smith, from repeated trials made by himself and friends, strongly recommends the use of larch bark in tanning, not only for light calf, deer, or sheep's skins, but for stout hides; and states, that sole-leather tanned with it, and worn against other leather tanned with vallonia, resisted the wear better, and did not imbibe so much water. He then asks whether there is any further occasion for the importation of Dutch or German bark.

9. *Atkin's Rock.*—The dangerous ledge of Atkin's Rock has been marked and observed very accurately by Captain Cork, of the Barnet, from Demerara to Liverpool. Its position has not been determined exactly, but the Captain announces its situation to be precisely 50° 5′ latitude, and 12° west longitude from Greenwich.

10. *Preservation of Fresco Painting.*—A new process for removing frescoes from one wall to another without in-

jury to the painting, has been devised by Signor Steffano Barezzi, of Milan. The picture is covered with a prepared canvass to which it adheres, and is thus detached from the wall. The canvass is afterwards applied to another wall, to which the painting again attaches itself without the least trait being destroyed. The practicability of this method has been successfully proved, and the inventor is now employed in transferring a large fresco from the church Della Pace, at Rome. Great expectations are entertained that he will be able thus to rescue from destruction the celebrated Cena of Leonardo da Vinci.—*Magazine of Fine Arts*, 474.*

11. *Green Paint.*—Gas tar, mixed with yellow ochre, makes an excellent green paint, very useful for preserving coarse wood work or other articles.

II. CHEMICAL SCIENCE.

§ CHEMISTRY.

1. *Curious effect of Sea Water upon Cast Iron.*—In a recent visit to Plymouth, Mr. Hatchett obtained from Mr. Whidbey, a portion of a cast-iron gun, which had long been immersed in sea water: it was incrusted to the depth of an inch with a substance having all the exterior characters of impure plumbago; easily sectile, greasy to the touch, and leaving a black streak upon paper. This substance, digested in water, afforded a small quantity of muriate of iron, but was not otherwise affected. Digested in muriatic acid, a considerable portion was dissolved without any effervescence; and the solution had the properties of pure muriate of iron, with a trace of manganese. The insoluble portion, when collected upon a filter, washed, and dried, was a shining black powder, very soft and unctuous to the touch, and apparently pure plumbago. At Dr. Wollaston's suggestion, I examined it for manganese: I deflagrated a portion of it with chlorate of potassa, dissolved the residue in muriatic acid, and threw down oxide of iron by pure ammonia; the neutral solution was filtered, and evaporated to dryness; the dry salt was entirely dissipated by heat, and gave no trace of manganese. The relative proportions of the component parts of this substance were,

Oxide of iron - -	81
Plumbago - - -	16
	97

Anchors, and other articles of wrought iron, when similarly exposed are only superficially oxidized, and exhibit no other peculiar appearance. There can, therefore, be little doubt that

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* Is not the "Last Supper" of L. da Vinci already destroyed? Ed.

the rapid decay and change of cast iron is partly to be attributed to a galvanic action, the plumbaginous crust in contact of the cast metal producing an electro-motive combination, aided by and promoting the decomposition of the sea water, and of its saline contents. Considering the resemblance of this crust to that described by Mr. Daniell, as produced by the action of acids upon cast iron, (see Vol. II., p. 233, of this Journal), I expected to detect a portion of silica in it, but was disappointed.

2. *Solubility of Magnesia, and its Carbonate in Water.*—Dr. Fyfe has lately experimented on the solution of magnesia and its carbonate in water; and not only finds that these substances are soluble, but also that, as with lime, they are more soluble in cold than in hot water. The magnesia was prepared by precipitation, washing, and ignition; water was then digested on it, and afterwards the magnesia, dissolved in the water, separated by carbonate of ammonia and phosphate of soda. In this way it was ascertained, that 1 part of magnesia required 5142 parts of water, at a temperature of 60°, to dissolve it; and at a temperature of 212°, 36000 parts of water. The effect of temperature is so great in altering the solubility of magnesia in water, that if a cold saturated solution be heated in a narrow-necked matrass, it becomes turbid on rising to 212°.

On repeating the experiments with carbonate of magnesia, it was found that 1 part required 2493 parts of water, at the temperature of 60°, for its solution, and 9000 parts, at the boiling point.—*Edinburgh Journal,* vol. v., p. 305.

3. *On the Solution of Carbonate of Lime, by* Mr. Dalton.—Mr. Dalton, in a paper containing remarks on the analysis of spring and mineral waters, has stated some interesting circumstances respecting the alkaline properties of solution of carbonate of lime. It is stated, " that all spring water containing carbonate or super-carbonate of lime, is essentially limy or alkaline, by the colour tests. And this alkalinity is not destroyed till some more powerful acid, such as the sulphuric or muriatic, is added, sufficient to saturate the whole of the lime. Indeed, these acids may be considered as sufficient for tests of the quantity of lime in such waters; and nothing more is required than to mark the quantity of acid necessary to neutralize the lime. It does not signify whether the water is boiled or unboiled, nor whether it contains sulphate of lime along with the carbonate; it is still limy in proportion to the quantity of carbonate of lime it contains. Agreeably to this idea, too, I find that the metallic oxides, as those of iron or copper, are thrown down by common spring water, just the same as by free lime. Notwithstanding, this carbonate of lime, in solution in water, contains twice the acid that chalk or limestone does. I fully expected the super-

carbonate of lime in solution to be acid; but it is strongly alkaline, and scarcely any quantity of carbonic acid water put to it, will overcome this alkalinity. Pure carbonic acid water is, however, acid to the tests. I could not be convinced of the remarkable fact stated in this paragraph, till I actually formed super-carbonate of lime, by super-saturating lime water in the usual way, till the liquid from being milky became clear. It still continued limy, and was even doubtfully so when two or three times the quantity of acid was added. It should seem, then, to be as impossible to obtain a neutral carbonate of lime, as it is to obtain a neutral carbonate of ammonia, in the sense here attached to the word neutral."—*Memoirs of the Manchester Society*.

4. *On Sulphureous Mineral Waters, and the Nitrogen contained in them.*—The existence of nitrogen in mineral waters has frequently been ascertained, and its quantity stated; but no very precise ideas have hitherto been published, on the cause of its existence in the waters, and the attempt of M. Anglada is, perhaps, the first made to shew its source, and the situations in which it may be expected. Having observed from the experience of others and himself, that nitrogen occurred in waters containing sulphuretted hydrogen, he was induced to search whether it was not constantly present in such waters, and finding that to be the case, reasoned and experimented upon its production. In many sulphurous springs, the sources of which were easily arrived at, it was readily observed, that nitrogen either rose from the sides of the spring, or could be obtained without difficulty from the water; but in others, which were confined by pipes or conduits, it was necessary to open up the works, and get to the source. In all cases, however, sulphurous waters were found to contain nitrogen; and it was remarked, that though the water contained abundance of sulphuretted hydrogen, the gas contained none.

M. Anglada expected by heat to obtain sulphuretted hydrogen and carbonic acid gas from sulphureous water; but in no case obtained any thing except pure nitrogen gas. Guided by this result, he concluded that, in all the sulphureous waters that had thus yielded him nitrogen, that gas came from the air which the waters had taken up in their subterraneous course, and from which the oxygen had been separated by the sulphur, &c. In order, as it were, to prove this opinion, a portion of a sulphureous water was treated with acetate of lead, to separate all the sulphuretted hydrogen from it, and then boiled: it gave out more gas than before, and the gas was a mixture of oxygen and hydrogen.

The air thus furnished to waters, is given to them, M. Anglada thinks, in the bowels of the earth, by currents of air of which we know little or nothing.

In consequence of this action of the air on waters of this kind, it happens sometimes that a water decidedly sulphureous at its source, ceases to be so at a little distance from it; dependent on the strength of the water, and the means it has of getting air. In those cases, the previous sulphureous state of the water may be deduced from the disengagement of pure nitrogen, or containing very little oxygen, and from certain glairy appearances which are exhibited by those waters.

M. Anglada also remarks, that in many waters containing carbonic acid, abundance of nitrogen is found, and he suggests, that probably the air may act in them on some carbonaceous matters, giving rise to carbonic acid and nitrogen at the same time. It is also remarked in the conclusions attached to this paper, and which being given above, need not be repeated, that the change takes place at all temperatures, and that if nitrogen is not found in every sulphurous water, it is in all those containing a hydro-sulphuretted alkali.—*Ann. de Chim.*, vol. xviii. p. 113.

5. *On Meteoric Iron, and the Masses supposed to be such.*—In the year 1810, whilst examining a specimen of graphite from Saxony, Mr. J. F. John discovered nickel in it, and afterwards ascertained the presence of cobalt in a mass of iron, similar to that of Pallas, but found in an old collection. These circumstances led him to consider that the opinion generally entertained of some characters of meteoric iron, or meteorolites, were incorrect. It has been assumed that any stones or pieces of iron, supposed to be meteoric, and containing nickel, to the exclusion of all other metals, were certainly of the origin supposed. The uncertainty of this test, and the remembrance that M. Laugier had discovered chromium in some aërolites, and also that the occurrence of this metal had been proposed as a test of the meteoric origin of stones, &c., induced M. John again to analyze portions of the large masses of iron considered as meteoric, and also portions of iron taken by the magnet from meteorolites. The following are the results of these analyses:

	Iron of Pallas.	Iron of Ellbogen in Bohemia.	Iron of Humboldt.
Iron	90	87.5	91.5
Nickel	7.5	8.75	6.5
Cobalt	2.5	1.85	2
Chromium	trace	and loss 1.9	trace.
Manganese	0		0
	100	100	100

The mass of Aix-la-Chapelle contains iron, a little arsenic, traces of charcoal and of cobalt, and perhaps of sulphur. The mass taken from beneath the pavement in 1762 is evidently the produce of art. Polyxene, or the black grains found in a particular sort of platina, are simply metallic iron, without any mixture of other metals. The iron from Portuguese America, composed of crystalline grains, and which is very rare, contains a little copper and cobalt, and perhaps a trace of chromium and manganese. This latter iron was obtained from M. Krusenstern, and M. John found a similar kind in the mineralogical collection of Zurich, which, according to the table, had been brought by M. Peterson from Kamtschatka.

Having pulverized some meteoric stones, and obtained the iron from them by means of a magnet, M. John analyzed it for the purpose of comparing it with the analyses already mentioned. The results were as follows:

	Iron from the Aërolite of Chatonnay.	Of l'Aigle.	Of Sienne.
Iron	92.72	92.72	92.72
Nickel	5.5	5.5	5.1
Sulphur	1	Quantities too small to be weighed.	
Cobalt	0.78		
Chrome	trace		

The conclusions drawn from these experiments are, 1. That the iron of meteoric stones and of the large ductile masses mentioned, contain the same substances; namely, iron, nickel, cobalt, chrome, and perhaps also manganese, as in that from Ellbogen. 2d. Iron of meteoric stones contains less nickel than the large masses of soft iron. 3. Iron of meteoric stones evidently contains sulphur, but probably not in combination with the whole of it, but forming magnetic pyrites disseminated through it. The large masses found in the earth contain no sulphur.—*Ann. de Chim.* xviii. p. 198.

6. *Tests for Arsenic.*—Dr. Porter, of the University of South Carolina, in observing on the tests for the detection of arsenic, remarks, that an appearance, similar to Scheele's green, is produced by carbonate of potash, added to a solution of copper containing coffee, but without arsenic, more striking than if a weak solution of arsenic be used. He also states that, in the production of Scheele's green by arsenic, sulphate of copper and carbonate of potash, chromate of potash might be substituted for the arsenic; and that the precipitate produced could not be distinguished by the eye from Scheele's green. Also that Mr. Hume's test of the nitrate of silver (as modified in its application by Dr. Marcet,) gave, with chromate of potash, a yellow precipitate, which, when placed side by side with one pro-

duced by arsenic, could not be distinguished by colour or appearance from it.—Silliman's *Jour.* iii. p. 355.

7. *On the detection of Arsenic in Ores of Antimony.*—The following is the process of M. Serulas, by which he detects arsenic in antimony. The antimony, or its oxide, is mixed with cream of tartar, and heated in a close crucible: in this way an alloy of the antimony with potassium is formed. The alloy is then placed under a bell-glass filled with water, and a large quantity of hydrogen is soon liberated. This, when burned in a narrow glass jar, deposits a brown pellicle on its sides, if the antimony contains arsenic, but no pellicle is produced if arsenic be not present. Very small quantities may be detected in this way.

8. Berzelius *on the Oxide of Platinum and Gold.*—After remarking on the numbers given for the oxides of platinum by Mr. Cooper and Dr. Thomson, M. Berzelius proceeds to describe the process he now adopts to obtain these numbers accurately. The muriate of platina and potassa being a neutral compound, a portion of it was well washed and dried; it was then anhydrous, and by heat gave off chlorine, without any trace of water; so that it may be concluded to be a double chloride of platinum and potassium. ×2.251 grains were slowly heated in a current of dry hydrogen as long as muriatic acid was formed. It lost .65 of a grain in weight, and the residue washed left .898 of platinum undissolved. It was therefore composed of

Chlorine	.650
Platina	.898
Chloride of potassa	.703

In the tables of M. Berzelius the numbers are such that, if calculated, it will be found that the platinum contains twice as much chlorine in the above compound as the potassium. An atom of muriate of potassa is 1865.13, and 2 atoms of muriate of platina are 4201.6, so that the compound atom would weigh 6066.19. This would give in 2.251 grains of the substance .657 chlorine, .898 platina, and .696 muriate of potassa, which is nearly accordant with the experiment above.

The same results were obtained with the muriate of soda and platinum. This salt contains 1 atom of muriate of soda, 2 atoms muriate of platinum, and 12 atoms of water.

M. Berzelius still retains his opinion of the number for gold, or the weight of its atom (2486.) He adds an experiment in which he reduced it by phosphorous, but says it is not so precise as the reduction by mercury, consequently it adds nothing in proof of the correctness of the numbers given. M. Pelletier, it will be remembered, gives the number of gold as 2993.

M. Berzelius describes two muriates of gold. The super-muriate crystallizes readily in small needles, of a pale-yellow colour; it is less soluble in water than the neutral muriate, and its solution is of a gold-yellow colour. The neutral muriate is obtained by exposing the super-muriate to a low heat. It loses first muriatic acid, and then a mixture of it with chlorine, so that it begins to decompose even before all the free acid is driven off. To obtain it perfectly neutral, it must be prepared by adding hot water to the proto-muriate of gold, which is then reduced to gold and the neutral muriate. Its solution is of an intense ruby colour; it yields a reddish-brown saline mass, which liquifies in the air. The addition of muriatic acid changes the colour to yellow, and per-muriate crystallizes, if the solution is concentrated.—*Ann. de Chem.* xviii. p. 146.

9. *On rendering Tissues incombustible.*—M. Gay-Lussac mentions, as the result of his experiments on the best means of rendering cloth incombustible, that solutions of muriate, sulphate, phosphate, and borate of ammonia, with borax, and some mixtures of these salts, were found the most effectual; in fact, according to his theory on the subject, those substances which at the temperature of burning best shielded the fibre from the air, and supplied most incombustible gas or vapour to the flame, proved to be most efficacious.

10. *Heat produced by Chlorine.*—Among the obvious qualities of chlorine, Dr. Silliman mentions the heat it excites on the hand when plunged into it. The effect is equal to a temperature of 90° or 100°.

11. *Explosion of Chlorine and Hydrogen.*—Dr. Silliman cautions chemists against making mixtures of chlorine and hydrogen in large quantities without due precaution. He relates some instances in which two or three quarts of the gases had been mixed, where the explosions produced on exposure to sunlight were instantaneous and very powerful, and one also in which the explosion took place by exposure of the gases to a very diffuse and dim light.

12. *Oxalic Acid.*—M. Berzelius decomposed the oxalic acid by first forming an oxalate of lime of determinate composition, as to the water it contained, and then digested it with muriate of gold. The gold was reduced, and carbonic acid formed. 4 grains of oxalate, containing 3.25 of the anhydrous compound, reduced 3.352 grains of metallic gold, from which the composition of oxalic acid is deduced as 2 atoms of carbon and three atoms of oxygen, no hydrogen being present. This result was confirmed by the action of the quadroxalate of potassa on solution of gold; 2 grains of it precipitate 2.05 of the metal.

13. *Alum in decomposed Mica Slate.*—Among the sources of alum there is one which we have never seen mentioned by any author. It is from the decomposition (not of clay-slate, but) of mica-slate. We have been frequently presented with specimens of alum found in decomposed mica slates, as from the towns of Preston, Waterbury, and Huntington in Connecticut, and indeed we have seen but few instances of American alum derived from any other source. It is, as we are informed, so abundant in some places, that the people use it in dyeing without resorting to any other supply. We are not aware that this source of alum has been observed in other countries.—*American Journal,* iii., p. 240.

14. *American Hydraulic Lime.*—The following is an analysis made of an excellent hydraulic lime used in the construction of the great canal in the state of New York:

Carbonic acid . .	35.05
Lime	25.
Silex	15.05
Alumine . . .	16.05
Water	5.03
Oxide of iron . .	2.02
	98.2

It will not slake, but is pulverized, and then mixed with half its quantity (probably bulk) of sand.

§ II. ELECTRICITY, MAGNETISM, &c.

1. *Effects of Lightning.*—A house at Geneva, which had plates of tinned iron disposed in various ways about the roof and other parts, was struck by lightning on the 3d of July. The lightning produced various effects here and there, but among them none was more remarkable than that produced on one of the plates of tin that had been placed on the roof against the chimney. This plate is pierced with two circular holes, about an inch in diameter each, and four inches apart. Each hole is strongly burred, but the remarkable circumstance is, that the burs of the two holes are in opposite directions, which, according to M. Pictet, either indicates that the electric fluid has passed through the plate forming one hole, has moved five inches along it, and then gone through again; or that there were two currents of fluid which moved simultaneously in opposite directions, at five inches distance from each other.

2. *Production of Magnetism.*—M. Poenitz of Dresden has lately experimented on the production of magnetism by hammering, friction, &c., and has come to the conclusion, per-

haps before drawn, that it is not produced in the iron, but given to it by the external magnetism of the earth ; all that the motion given to the iron in the various mechanical processes used, does, is to render it more susceptible of receiving magnetism. One of his modes of giving this motion to the particles, is to fix one end of a rod of iron or steel steadily, placing the rod in any position required, as either perpendicular or parallel to the dip, &c., and then to make it vibrate by drawing the free end from the axis of the rod, and suddenly letting it loose. If in a favourable position, the iron soon acquires magnetism.

3. *Effect of Iron on the rate of Chronometers.*—Mr. Barlow, of Woolwich, has lately made experiments on the effect of iron, free from any permanent magnetism, in altering the rates of chronometers placed in its vicinity. Mr. Fisher had remarked the difference of rate occurring in his chronometer when on board and on shore, though the vessel was frozen in, and therefore could produce no variation by its motion. He therefore attributed it to the magnetic action of the iron in the vessel, a conclusion that has been confirmed by Mr. Barlow's experiments.

In these experiments, various chronometers were placed in different positions near a mass of unmagnetic iron, and an alteration in the rates of them observed. This alteration varied with their respective situations to the iron, but was always uniform in the same position. The effects amounted sometimes to 5″ per day, and were at last traced to magnetism in the balance and spring of the chronometer.

The plan suggested by Mr. Barlow, of estimating the effects of a ship's metal on the compass needle, namely, by placing a plate of iron in such a position, and so near to the needle, as to be equal in effect to the rest of the metal in the ship, is well known, and the return of his Majesty's ship Leven, from a voyage of sixteen months, affords proof, by the experiments that have been made on board, of its value. The same plan is now proposed by him for ascertaining the ship-rates of chronometers before they go on board, by simply taking their rates on a certain situation, and at a proper distance, from such a plate of iron.

4. *Electro-Magnetic Rotation.*—An ingenious little instrument has been invented by M. Ampere, in illustration of the rotatory motion of the wire round the pole of a magnet. Its advantage consists in comprising the voltaic combination used in itself. Suppose a cylinder of copper, about two inches in diameter and the same in height, and within it a smaller cylinder about half an inch in diameter ; these are fastened together by a bottom, having a hole in its centre the size of the smaller cylinder, so

that the two cylinders form, as it were, a circular cell for acid: a piece of metal is fastened from side to side, like a bridge, across the top of the smaller cylinder, and from the middle of it rises a piece of wire, supporting at its top a small metal cup, containing mercury. A short cylinder of zinc is then procured, of a size that will permit it to go freely into the copper cell before described: a wire, in the form like the letter U inverted (∩), is soldered to it at opposite sides, and in the bend of this wire a metallic point is fixed, which, when placed in the little cup of mercury before described, suspends the zinc cylinder freely in the copper cell; then weak acid being put into the cell, the zinc and copper form a voltaic combination, and the two sides of the ∩ wire are both in the same state, so that the pole of a small magnet placed in the cylinder, that is left open in the axis of the apparatus, makes the wire, and the zinc cylinder with it, revolve. If the apparatus be 9 or 10 inches in diameter, it is stated that there is a tendency to rotation by the action of the terrestrial magnetism alone.

5. *Note on New Electro-Magnetical Motions, by* M. Faraday,—At page 96 of this volume, I mentioned the expectation I entertained of making a wire through which a current of voltaic electricity was passing, obey the magnetic poles of the earth in the way it does the poles of a bar magnet. In the latter case it rotates, in the former I expected it would vary in weight; but the attempts I then made, to prove the existence of this action, failed. Since then I have been more successful, and the object of the present note is so far to complete that paper, as to shew in what manner the rotative force of the wire round the terrestrial magnetic pole, is exerted, and what the effects produced by it, are.

Considering the magnetic pole as a mere centre of action, the existence and position of which may be determined by well-known means, it was shewn by many experiments, in the paper, page 74, that the electro-magnetic wire would rotate round the pole, without any reference to the position of the axis joining it with the opposite pole in the same bar; for sometimes the axis was horizontal, at other times vertical, whilst the rotation continued the same. It was also shewn that the wire, when influenced by the pole, moved laterally, its parts describing circles in planes perpendicular nearly to the wire itself. Hence the wire, when strait and confined to one point above, described a cone in its revolution, but when bent into a crank, it described a cylinder; and the effect was evidently in all cases for each point of the wire to describe a circle round the pole, in a plane perpendicular to the current of electricity through the wire. In dispensing with the magnet, used to give these motions, and operating with the terrestrial magnetic pole, it was easy, by applying the information gained above, to deduce beforehand the direction the motions would probably take: for, assuming that the dipping-needle, if it does not point to the pole of the earth, points at least in the direction in which that pole is active, it is evident that a straight electro-magnetic wire, affected by the terrestrial as by an artificial pole, would move laterally at right angles to the needle; that is to say, it would endeavour to describe a cylinder round the pole, the radius of which may be represented by the line of the needle prolonged to the pole itself. As these cylinders, or circles, would be of immense magnitude, it was evident that only a very minute portion of them could be brought within the reach of experiment; still, however, that portion would be sufficient to indicate their existence, inasmuch as the motions taking place in the part under consideration, must be of the same kind, and in the same direction, as in every other part.

Reasoning thus, I presumed that an electro-magnetic wire should move laterally, or in a line perpendicular to the current of electricity passing through it, in a plane perpendicular to the dipping-needle; and the dip being here 72° 30′, that plane would form an angle with the horizon of 17° 30′, measured on the magnetic meridian. This is not so far removed from the horizontal plane, but that I expected to get motions in the latter, and succeeded in the following manner:—A piece of copper wire, about .045 of an inch thick, and fourteen inches long, had an inch at each extremity bent at right angles, in the same direction, and the ends amalgamated; the wire was then suspended horizontally, by a long silk thread, from the ceiling. A basin of clean pure mercury was placed under each extremity of the wire, and raised till the ends just dipped into the metal. The mercury in both basins was covered by a stratum of diluted pure nitric acid, which dissolving any film, allowed free motion. Then connecting the mercury in one basin with one pole of Hare's Calorimotor, the instrument mentioned page 74, the moment the other pole was connected with the other basin, the suspended wire moved laterally across the basins till it touched the sides: on breaking the connexion, the wire resumed its first position; on restoring it, the motion was again produced. On changing the position of the wire, the effect still took place; and the direction of the motion was always the same relative to the wire, or rather to the current passing through it, being at right angles to it. Thus when the wire was E and W, the E end to the zinc, the W end to the copper plate, the motion was towards the north; when the connexions were reversed, the motion was towards the south. When the wire hung N and S, the N end to the zinc plate, the S end to the copper plate, the motion was towards the W; when the connexions were reversed, towards the E; and the intermediate positions had their motions in intermediate directions.

The tendency, therefore, of the wire to revolve in a circle round the pole of the earth, is evident, and the direction of the motion is precisely the same as that pointed out in the former experiments. The experiment also points out the power which causes Ampere's curve to traverse, and the way in which that power is exerted. The well-known experiment, made by M. Ampere, proves, that a wire ring, made to conduct a current of electricity, if it be allowed to turn on a vertical axis, moves into a plane east and west of the magnetic meridian; if on an E and W horizontal axis, it moves into a plane perpendicular to the dipping-needle. Now if the curve be considered as a polygon of an infinite number of sides, and each of these sides be compared in succession to the straight wire just described, it will be seen that the motions given to them by the terrestrial pole, or poles, are such as would necessarily bring the polygon they form into a plane perpendicular to the dipping-needle; so that the traversing of the ring may be reduced to the simple rotation of the wire round a pole. It is true the whole magnetism of the earth is concerned in producing the effect, and not merely that portion which I have, for the moment, supposed to respect the north pole of the earth as its centre of action; but the effect is the same, and produced in the same manner; and the introduction of the influence of the southern hemisphere, only renders the result analogous to the experiment at page 82, where two poles are concerned, instead of that at page 77, &c., where one pole only is active.

Besides the above proof of rotation round the terrestrial pole, I have made an experiment still more striking. As in the experiment of rotation round the pole of a magnet, the pole is perpendicular to but a small portion of the wire, and more or less oblique to the rest, I considered it probable, that a wire, very delicately hung, and connected, might be made to rotate round the dip of the needle by the earth's magnetism alone; the upper part being restrained to a point in the line of the dip, the lower being made to move in a circle surrounding it. This result was obtained in the following manner: a piece of copper-wire, about 0.018 of an inch in diameter, and six inches long, was well amalgamated all over, and hung by a loop to another piece of the same wire, as described at page 285, so as to allow very free motion, and its lower end was thrust through a small piece of cork, to make it buoyant on mercury; the upper piece was connected with a thick wire, that went away to one pole of the voltaic apparatus; a glass basin, ten inches in diameter, was filled with pure clear mercury, and a little dilute acid put on its surface as before; the thick wire was then hung over the centre of the glass basin, and depressed so low that the thin moveable wire having its lower end resting on the surface of the mercury, made an angle of about 40° with the horizon. Immediately the circuit through the mercury was completed, this wire began to move and rotate, and continued to describe a cone whilst the connexions were preserved, which, though its axis was perpendicular, evidently, from the varying rapidity of its motion, regarded a line parallel to the dipping needle as that in which the power acted that formed it. The direction of the motion was, as expected, the same as that given by the pole of a magnet pointing to the south. If the centre from which the wire hung was elevated until the inclination of the wire was equal to that of the dip, no motion took place when the wire was parallel to the dip; if the wire was not so much inclined as the dip, the motion in one part of the circle capable of being described by the lower end was reversed; results that necessarily follow from the relation of the dip and the moving wire, and which may easily be extended.

I have described the effects above as produced by the north pole of the earth, assuming that pole as a centre of action, acting in a line represented by the dip of the needle. This has been done that the phenomena might more readily be compared with those produced by the pole of a magnet. M. Biot has shewn by calculation that the magnetic poles of the earth may be considered as two points in the magnetic axis very near to each other in the centre of the globe. M. Ampere has in his theory advanced the opinion that the magnetism of the earth is caused by electric currents moving round its axis parallel to the equator. Of the consonance existing among the calculation, the theory and the facts, some idea may perhaps be gained from what was said, page 86, on the rotation of a pole through and round a wire ring. The different sides of the plane which pass through the ring, there described, and which may represent the equator in M. Ampere's theory, accord perfectly with the hemispheres of the globe; and the relative position of the supposed points of attraction and repulsion, coincide with those assigned by M. Biot for the poles of the earth itself. Whatever, however, may be the state and arrangement of terrestrial magnetism, the experiments I have described bear me out, I think, in presuming, that in every part of the terrestrial globe an electro-magnetic wire, if left to the free action of terrestrial magnetism, will move in a plane (for so the small part we can experiment on may be considered), perpendicular to the dip of the needle, and in a direction perpendicular to the current of electricity passing through it.

Reverting now to the expectation I entertained of altering the apparent weight of a wire, it was founded on the idea that the wire, moving towards the north round the pole, must rise, and moving towards the south must descend; inasmuch as a plane perpendicular to the dipping needle, ascends and descends in these directions. In order to ascertain the existence of this effect, I bent a wire twice at right angles, as in the first experiment described in this note, and fastened on to each extremity a short piece of thin copper wire amalgamated, and made the connexion into the basins of mercury by these thin wires. The wire was then suspended, not as before, from the ceiling, but from a small and delicate lever, which would indicate any apparent alteration in the weight of the wire; the connexions were then made with the voltaic instrument, but I was surprised to find that the wire seemed to become lighter in both directions, though not so much when its motion was towards the south as towards the north. On farther trial it was found to ascend on the contacts being made, whatever its position to the magnetic meridian, and I soon ascertained that it did not depend on the earth's magnetism, nor on any local magnetic action of the conductors, or surrounding bodies, on the wire.

After some examination I discovered the cause of this unexpected phenomenon. An amalgamated piece of the thin copper-wire was dipped into clean mercury, having a stratum of water or dilute acid over it; this, however, was not necessary, but it preserved the mercury clean and the wire cool. In this position the cohesive attraction of the mercury raised a little elevation of the metal round the wire of a certain magnitude, which tended to depress the wire by adding to its weight. When the mercury and the wire were connected with the poles of the voltaic apparatus, this elevation visibly diminished in magnitude by an apparent alteration in the cohesive attraction of the mercury, and a part of the force which before tended to depress the wire was thus removed. This alteration took place equally, whatever the direction in which the current was passing through the wire and the mercury, and the effect ceased the moment the connexions were broken.

Thus the cause which made the wire ascend in the former case was evident, and by knowing it, it was easy to construct an apparatus in which the ascent should be very considerable. A piece of copper bell-wire, about two inches long, had portions of the amalgamated fine copper-wire soldered on to its ends, and those bent downwards till parallel to each other. This was then hung by a silk thread from the lever, and the fine wire ends dipped into two cups of clean mercury. When the communications were completed from the voltaic instrument through these two cups, the wires would rise nearly an inch out of the mercury, and descend again on breaking the communition.

Thus it appears that, when a fine amalgamated copper-wire dips into mercury, and a current of voltaic electricity passes through the combination, a peculiar effect is produced at the place where the wire first touches the mercury, equivalent to a diminution of the cohesive attraction of the mercury. The effect rapidly diminished by increasing the size of the wire, and 20 pair of plates of Dr. Wollaston's construction, and four inches square, would not produce it with the fine wire: on the contrary, two large plates are sufficient. Dr. Hare's calorimotor was the instrument used, and the charge was so weak that it would barely warm two inches of any sized wire. Whether the effect is an actual diminution of the attraction of the particles of the mercury, or depends on some other cause, remains as yet to be determined. But in any case its influence is so powerful, that it must always be estimated in experiments made to determine the force and direction of an electro-magnetic wire, acted on by a magnetic pole, if the direction is otherwise than horizontal, and if they are observed in the way described in this note. Thus, at the magnetic equator, for instance, where the apparent alteration of weight in an electro-magnetic wire may be expected to be greatest, the diminution of weight in its attempt to ascend would be increased by this effect, and the apparently increased gravity produced by its attempt to descend would be diminished, or perhaps entirely counteracted.

I have received an account by letter from Paris, of an ingenious apparatus (see page 415,) contrived by M. Ampere, to illustrate the rotatory motions described in my former paper. M. Ampere states that, if made of sufficient size, it will rotate by the magnetic action of the earth, and it is evident that that will be the case in latitudes at some distance from the equator, if the rotatory wires, namely, these by which the ring of zinc is suspended, are in such a position as to form an angle with a vertical line, larger than that formed by the direction of the dip.

It is to be remarked, that the motions mentioned in this note were produced by a single pair of plates, and therefore, as well as those described in the paper, page 74, are the reverse of what would be produced by two or more pair of plates. It should be remembered also, that the north pole of the earth is opposite in its powers to what I have called the north poles of needles or magnets, and similar to their south poles.

I may be allowed, in conclusion, to express a hope that the law I have ventured to announce, respecting the directions of the rotatory motions of an electro-magnetic wire, influenced by terrestrial magnetism, will be put to the test in different latitudes; or, what is nearly the same thing, that the law laid down by M. Ampere, as regulating the position taken by his curve, namely, that it moves into a plane perpendicular to the dipping-needle, will be experimentally ascertained by all those having the opportunity.

III. NATURAL HISTORY.

§ I. MINERALOGY, METEOROLOGY, &c.

1. *Oxalate of Iron found as a Mineral.*—This curious substance was found by M. Breithaupt, in friable lignite, and at first supposed to be eisen resin, or mellate of iron; but M. M. de Rivero, on analyzing it, found it to be a compound of oxalic acid and protoxide of iron. It was found in small flat crystalline pieces of a yellow colour, and similar in appearance to the protoxalate of the laboratory. It was scratched by the nail. Its specific gravity was 1.3; and when rubbed, it became negatively electrical. All its properties agreed with those of oxalate of iron, and it was distinguished from mellate of iron in the circumstance that when decomposed by ammonia, the ammoniacal solution did not precipitate alum, an effect which does take place with the mellate. When decomposed by ammonia, protoxide of iron and oxalate of ammonia were obtained, and the elements appeared to be combined in the proportion of

portion of	53.86	protoxide of iron
	46.14	oxalic acid.

The name proposed for this mineral is Humboldtine.—*Ann. de Chim.*, xviii., p. 207.

2. *Native Copper of Lake Superior.*—Silliman's *Journal of Science*, Vol. III., contains an account, by Mr. Schoolcraft, of the native copper mines on the southern shore of Lake Superior. The first appearances of the copper are found on the head of the portage across Keweena-point, 270 miles beyond the Soult de St. Mara, and continues from thence to the river Ontonagon. The copper is metallic, and occurs in the pebbles disseminated in particles from the size of a grain of sand to lumps of two pounds in weight. A strip of alluvial sand extends from the lake up the river two or three leagues, to which succeed high broken hills of a sterile aspect, among which are the copper mines, at a distance of 32 miles from the lake. One very large mass of native copper reposes on the west bank of the river, at the foot of a bank of alluvion, lying amidst blocks of granite, hornblende, and other bodies peculiar to the soil of that place. The copper is pure and malleable, very bright, and lies in connection with serpentine rock, the face of which it nearly overlays, and is also disseminated through the substance of the stone. The rock is of an irregular form, about 3ft. 8in. long, and 3ft. 4in. wide; it may contain altogether about 11 cubical feet. The weight of metallic copper in the rock is estimated at 2200lb., but the quantity has evidently been much diminished since its first discovery.

Mr. Schoolcraft adds, that the serpentine rock is not in situ, nor is it so found in any part of the region he visited. He thinks that the source of this copper, and that found in the region of the Ontonagon, is the Porcupine Mountains. These are situated 30 miles westward of the river, and are supposed to be the seat of extinguished volcanoes, that have thrown forth these masses of native copper. This opinion is supported by the fact of the mountains being composed (as far as is observed) of granite, probably associated with other primary rocks, and among them serpentine; that the red sandstone-rock at their base is highly inclined towards the mountains, so as to be almost vertical, being apparently thrown up by the granite; and also by the height (1800 feet above the lake) of the peaks, their conical and rugged forms, and other appearances of volcanic mountains.

Mr. Schoolcraft then states the occurrence of native copper in islands on the other lakes and localities, but forming part of the same region. He concludes by stating it as his opinion, that though the alluvial soil along the banks of the Ontonagon up to its source, and in the contiguous region, contains very frequent, and some most extraordinary imbedded masses of copper, yet there is no body of it sufficiently extensive to become the object of mining operations; but he thinks that a mineralogical survey of the rock formation skirting the Ontonogon, would lead to the discovery of very valuable mines of the sulphuret and carbonate of copper, the working of which would be rendered still more advantageous by the occasional occurrence of masses of native copper.—*American Journal*, iii. 201.

3. *Mont Brasier.*—There is a mountain called Mont Brasier, situated between Senes and Larógne, in the Alps, which has been reported from time to time to emit noises and flame. This report has been verified by M. Dubois-Aymé, who, when in the neighbourhood in 1818, examined the mountain, collected evidence of the flames having been seen, and himself heard detonation in the mountain. On examining the mountain, he found that in the strata of limestone that formed its sides were beds of pyritous chalk, marly schists, radiated sulphuret of iron, bituminous substances, &c., and he concludes that the flames and noises are produced by the accidental firing of hydrogen liberated by the action of water on these substances. It is to be remarked that the detonations are most frequent when the wind is in one direction, an effect probably due to the mixture that then takes place of air with the hydrogen within the crevices of the mountain.—*Ann. de Chim.*, xviii., p. 158.

4. Scipione Mazella's *Account of the Rise of Monte Nuovo, in the Year* 1538.

[To the EDITOR of the *Quarterly Journal of Science.*]

DEAR SIR—I send you an account of the Monte Nuovo, extracted from a Description of the Environs of Naples, by Scipione Mazella, printed there in the year 1594, fifty-six years after the formation of the mountain: as the Latin description is quoted from an earlier work, it is not improbable that the author may have been a contemporary, if not an eye-witness of the remarkable event he commemorates.

Believe me very sincerely yours,
ASHHURST MAJENDIE.

" Opposite to the Monte Barbaro is a mountain about three miles in circumference, and nearly of the same height as the Monte Barbaro: it extends on the south towards the sea, on the north to the Lake Avernus, on the west nearly to the Sudatorio, and on the east joins the base of the Monte Barbaro: it is called by the country-people Monte Nuovo, because it was formed in the space of a day and a night. In the year 1538, on the 29th of September, several earthquakes having been felt throughout the whole district of Puzzuolo on the preceding days, the earth opened near Tripergola with a terrible sound like thunder, so that it was expected that the whole country would have been destroyed: the sky was then serene, and from the aperture burst forth flames of fire, bearing with them cinders and red hot stones, with dense smoke: these stones were carried up into the air with such force, that it was a wonderful and terrible thing to contemplate. Afterwards the wind rose with great fury, and the cinders were dispersed on all sides, and driven even into Africa. So vast a quantity of stones and cinders were thrown round the chasm, as to form the mountain now called the Monte Nuovo. In consequence of this chasm, and the formation of the mountain, the castle of Tripergola, with a great part of the lakes Avernus and Lucrine, and all those antient and noble buildings which were on their banks, and most of the baths were overwhelmed. The celebrated philosopher Simone Portio of Naples wrote a learned treatise on the burning of Tripergola, and relative to our subject states as follows: " Puteolorum regio fuit biennio fere magnis terremotibus agitata, ut nulla in ea superesset domu integra, quæ proximam minabatur ruinam. At die vigessimo septimo et vigessimo octavo Septemb. anno 1538 perpetuis diebus, et noctibus terra est commota; mare passibus fere 200 recessit quo loco Accolæ ingentem piscium multitudinem cæperunt et aquæ dulces erant. Die vero 29 magnus terræ tractus, qui inter radices montis Gauri (quem Barbarum appellant), et mare juxta Avernum jacet, sese erigere videbatur, et montis subito nascentes imitari figuram. Eo ipso die horâ noctis 2 hic terræ cumulus aperto veluti ore, magno cum fremitu miros evomuit ignes, pumices, lapides, cineresque fœdi tam magnam copiam, ut quæ adhuc extabant Puteolorum ædificia operuerit, herbas omnes texeret, arbores fregeret, pendentemque vindemiam ad sextum usque lapidem in cineres verterit, aves, et nonnullas quadrupedes bestias interemerit. Fugientibus per tenebras Puteolanis cum natis, et uxoribus magno ejulatu Neapolem sese recipientibus, cinis propè voragineus erat siccus, longè vero lutesus, et humidus cecedit. Sed quod omnem superat admirationem, mons circum eam voraginem (nunc dicitur mons novus) ex pumicibus, et cinere plusquam 1000 passuum altitudine una nocte congestus aspicitur, in quo multa in erant spiramenta, e quibus nunc duo supersunt, alterum juxta Avernum, alterum in medio montis ; Averni magna pars operta cinere. Baluea ita tot seculis celebrata, quæque tot ægris salutem præstabant cinere sepulta jacent, durat hoc incendium usq ; ad hanc diem cum aliqua tamen intercapedine."

5. *Fall of an Aërolite at Juvinas.*—A large aërolite fell on the 15th June last at Juvinas, a village in the arrondissement of l'Argentière, department de l'Ardèche, respecting which some very accurate details have been preserved. It fell about four o'clock P.M., the sky being clear, and the sun shining bright; a continued rolling noise was heard for above three minutes, during which time four distinct detonations took place. The noise was heard at Tarascon, at Nismes, and still farther off. A brilliant fire was seen in the air by persons, at Nismes, St. Thome, (a league to the west of Viviers), and Aps, a league farther off; all agree in saying it resembled a fire, burning like a star, and descending slowly in the N.W., and which, on disappearing, left a train of smoke. At first strange reports were circulated about the fire and the noise, but after eight or nine days, two countrymen of Juvinas, a village 20,600 toises to the N.W. of Viviers, described, that whilst working on the land they heard a dreadful noise, and saw an enormous mass of fire fall about fifty steps off from them into the ground ; tearing up the surface, and making much smoke. Being frightened they ran away, and at first durst not mention the circumstance ; but after a time, other persons becoming acquainted with the fact, search was made in the ground where the fire descended ; and, at the depth of five feet a large stone was discovered, weighing 220lb., or 91 kilogrammes. The countrymen, now relieved from their fright, thought from its weight that it contained gold, and could not be dissuaded from breaking it up. Large portions of it were, however, preserved. Some of the fragments are with Dr. Embri of Aubenas, others with M. Allijas, watch-maker

of the same place, and some which had been seen by M. Flaugergues, by whom the account is drawn up, belonged to Dr. Mausin. The stone appeared to be composed of two substances. The one in largest proportion is grey, and mixed with small black-shining grains. The surface is covered by a thin stratum, resembling a black and polished glare, not unlike that of the potteries. It appears to be formed of the black particle before spoken of, that have been fused at the surface, and has been very fluid, for the smallest inequalities are cased by it without being filled up. The stone is moderately hard, is scratched by steel instruments, does not strike fire with steel, and is not acted on by nitric acid.

M. Flaugergues agrees with those theorists who would consider this stone as having formed a very small comet or planetary body which has been met in its course by the earth.—*Journ. de Phys.* xcii. p. 463.

In a further account of this aërolite given by M. L. A. D. Firman, it is stated that another meteoric stone a kilogramme in weight, was found a little distance off, and several small ones at Mayras, near to Juvinas. M. de Malbos, who happened to be at Barias when the stone fell, was looking towards the place where it first appeared at the time. He saw a globe of fire descend perpendicularly from the heavens. He shewed it to his workmen, and counting his pulse estimated the time between its appearance and the explosion that followed, at five seconds. He observed also the obscure vapoury trace left by the meteorolite in the air. It was not continued to the earth but ceased to be emitted before the stone reached the ground, and remained seven or eight minutes undissipated.—*Journ. de Phys.* xciii, p. 71.

6. *Earthquake in Cunnemara.*—Upwards of one hundred acres of the lands of Letterbrocken, part of the property of the Provost of Trinity College in Joyce County, and consisting of prime pasture and mountain, on which a number of tenants resided, commenced moving and carrying with it huge rocks, immense masses of earth, the entire crop of wheat, oats, potatoes, &c., precipitated the whole into the sea and disappeared. Previous to its movement, a great noise was heard for some time, and the ground was convulsed. It is supposed that the previous drought which had occurred, prepared the way for this phenomenon. Two days after, a large tract of land thickly inhabited, the property of R. Martin, Esq. M. P., in the same neighbourhood, was visited with a like phenomenon, but even of a more destructive nature; the loss of the sufferers not being confined to their land and crops, but their entire stock and property being swallowed up by the earthquake. These occurrences are mentioned in the *Gents. Mag.* for November, from the *Tuam Gazette*, and their date given as ten days previous.

7. *Earthquake at Celebes.*—The Batavian Journals of April give an account of an earthquake very destructive in its effects which took place on the 29th of December 1820, on the south coast of Celebes. It did immense damage, particularly at Boelækomba where the sea rose several times a prodigious height, and then falling again with great rapidity, alternately deluged and left the shores, destroying all the plantations from Bontain to Boelækomba. Many hundred persons lost their lives. The forts of Boelækomba and Bontain were much damaged.

On the 4th of January this year, another shock of an earthquake occurred in the same neighbourhood.

8. *Mud Volcanoes in Bogs.*—A small tract of bog, called Forest Bog, about one mile and a half from Mount-mellick, situate about sixteen miles in a north-east direction from the Bog of Kilmaleady, has been strangely agitated for some days. It boils upwards, rising to a considerable height, the matter thrown up falling again into the basin from whence it issued. It has not overflown; but the people of the neighbourhood are in dread of some catastrophe occurring. The phenomena resemble those of the mud volcanoes of America, and are probably occasioned by internal fermentation.

9. *Scottish Pearl.*—A very fine pearl was found not long since in the river Tay, and is supposed to be one of the finest ever found in Scotland. It is hardly to be surpassed either for size or beauty by any oriental pearl. It is in the possession of Mr. C. Murray, Jeweller, in Perth.

10. *Rein Deer.*—Mr. Bullock has succeeded in bringing specimens of the Rein Deer to this country, and hopes are entertained that they may lead to the colonization of our mountain-forests by this animal. While on a tour in Norway he procured a herd of twenty, which were destroyed by eating a poisonous plant which grew on a small island on which they were kept. He then bought a second herd of twelve, and succeeded in bringing them alive and well into the Thames. Here however, in consequence of the custom-house officer not feeling authorized to allow the deer to be landed, eight died on board the vessel before permission could be obtained from the authorities in London. The remnant saved consists of a male and female, a fawn (since dead), and a male which has been cut. The latter is about ten hands high and proportionably stout. The others are a hand or two lower. Their fur is very thick and fine, and delicately warm and soft. Their horns branch beautifully and are covered with a short fur. The antlers of the largest animal are three feet in length. Their hoofs are very broad and flexible between the divisions, enabling them to clamber up pre-

cipices and hang on rocks inaccessible to other animals. They are very swift. They seem reconciled to hay as food, and like brandy which is administered to them as a medicine.

With the deer Mr. Bullock has brought a native Laplander, his wife and child. These beings are about four feet eight inches in height; the man being of the common size, the woman rather tall. The child is about five years old.—*New Mon. Mag.* p. 506.

11. *First appearance of the Boa Constrictor in the Island of St. Vincent.*—A most singular circumstance occurred last week in the Charaib country, when some negroes, who were working near Sandy Bay, discovered an immense serpent, hitherto wholly unknown in any of these islands, and which was shot through the head by one of the party. It is supposed to be a species of *Boa* so common on the neighbouring continent, but in what way it reached the shores of St. Vincent is quite unknown. Its entire length was between fourteen and fifteen feet, the circumference of the body between three and four feet. When first seen it was lying in a coil, but raised itself on being roused.—*Royal Gazette and Bahama Advertiser*, August 1821.

§ II. MEDICINE.

1. *Salivation and Ulceration of the Gums produced by Hydrocyanic or Prussic Acid.*—Dr. Macleod, in his experience with the prussic acid used as a medicine, has had occasion to notice three instances in which the administration of it produced ulceration of the gums with salivation. In the first instance, the effect was slight; in the second, it ceased with the discontinuance of the medicine, and returned when that was re-administered; in the third, the ulceration was very severe, extensive, and difficult to heal, and the soreness of the mouth exceedingly distressing; the medicine was discontinued, but on being repeated about three months afterwards, the effects were reproduced.

2. *Preservative against Scarlet Fever.*—It is announced in the *Journal de Medicina Pratique* of Berlin, that the Belladonna is a preservative against this fever. The fact was first discovered at Leipsig, but it has lately been confirmed by several experiments.

3. *On Irritation of the Spinal Nerves.*—

[To the Editor of the *Quarterly Journal of Science.*]

SIR—I take the liberty to submit to your notice a pathological fact, which has not, to the best of my knowledge, been generally remarked, and attention to which, as far as my own experience goes, promises some diminution of those difficulties with which the healing art has to contend. Most medical practitioners who have attended to the subject of spinal disease, must have observed that its symptoms frequently resemble various and dissimilar maladies, and that commonly the function of every organ is impaired whose nerves originate near the seat of the disorder. The occurrence of pain in *distant parts* forcibly attracted my observation, and induced frequent examinations of the spinal column; and after some years' attention, I consider myself enabled to state, that in a great number of diseases, morbid symptoms may be discovered about the origins of the nerves which proceed to the affected parts, or of those spinal branches which unite with them, and that if the spine be examined, more or less pain will commonly be felt by the patient on the application of pressure about or between those vertebræ from which such nerves emerge. If disease is confined to one side of the body, or one arm or one leg, this tenderness will be felt on the *same side* of the spine *only*; but if central parts, or both sides of the body, or both arms or legs, are diseased, tenderness will be felt on *both sides* of the spine. This symptom has been found to attend various other affections. This spinal affection may perhaps be considered as the *consequence* of diseases, but of its existence at their commencement any person may satisfy himself; and this circumstance, combined with the success which has attended the employment of topical applications to the tender parts about the vertebræ, appear to indicate that the *cause may* exist there. Prejudice sometimes operates against the idea of connexions so remote; but in many instances patients are surprised at the discovery of tenderness in a part, of whose implication in disease they had not the least suspicion.

The opinion entertained by some of our Continental neighbours, of the importance of the spinal brain in disease, is well known. That many of our maladies are the sympathetic consequences of the operation of a distant cause, and that diseases apparently the most dissimilar may have one common origin, have been the doctrines of some of our own most eminent pathologists; and though the injuries inflicted on our frames by mechanical and chemical agencies usually manifest their effects at the part where the cause has acted, we may be *too* much disposed to generalize from these premises, and to conclude that the *cause* of pain, inflammation, and the other phenomena of disease, also exist *at the part* where the symptoms are perceived. The records, however, of disease abound with cases, which, combined with daily observation, and the discoveries of morbid anatomy, tend to shake our confidence in this very natural and general inference, and to indicate that the cause can, and frequently does, exist very remotely from its phenomena, and that vascular fulness about the origins of nerves can produce the most formidable symptoms in the parts to which they are distributed. Such instances I need not enumerate, but may be permitted to remark that they appear to corroborate the opinion, that all the degrees of morbid sensation, from the slightest tingling and itching to the severest pain, may be produced by one common cause, obstructed nervous function, of which the ultimate effects are spasm, convulsion, and paralysis.

I am, Sir,
Your very obedient servant,
Malmesbury, Dec. 10, 1821. RICHARD P. PLAYER.

4. *Medical Prize Question.*—The Academical Society of the Lower Loire has proposed a prize, consisting of a gold medal value 300 francs, for the best answer to questions respecting the yellow fever. It is required to trace its origin, to specify its causes and nature ; to describe the state of the atmosphere and local circumstances where it prevails; to notify its identity or otherwise with similar fevers in Europe, *&c.*; to distinguish whether it be complicated with any other malady. There is also a second subject relating to the means for preventing its spreading, the proper modes of quarantine, *&c.* The memoirs to be sent, post free, to the Secretary of the Society before the 1st of May, 1822. Each to bear a motto with a repetition in a sealed paper, containing, as usual, the author's name and address.

5. *Medical Prize Question.*—The Royal Society of Medicine at Marseilles have proposed the following questions : 1. To determine the structure and functions of the spinal marrow. 2. To describe the nature, causes, symptoms, and treatment of the diseases by which the spinal marrow is affected. It is desired that clinical observations and pathological anatomy should be made the principal objects of the memoires. They may be written in Latin or French. The extent of time allowed is till July, 1822, and the prize a gold medal.

IV. GENERAL LITERATURE.

1. *Cabinet of Ancient Glass.*—A cabinet has been opened in Naples in the Studii Palace for the antique glasses found in Pompeii and Herculaneum. The collection contains a great variety of forms and colours, and proves that the ancients made use of glass as the moderns do, both in decorating their rooms, and in instruments of chemistry. The cabinet also contains a number of cinenary urns, for the most part enclosed in vases of lead.

2. *Lithography.*—A work is now in the course of publication at Stutgard, consisting of lithographic engravings by Strisner, from pictures of the early German masters, in the collection of Sulpice, Boisserée, and other amateurs. The whole when completed will contain 144 subjects; these are published in quarterly numbers in imperial folio, with descriptive, letter-press, written by Boisserée. The ability of the engraver which has been displayed in another lithographic work, that of the Munich Gallery, warrants the expectation of such a series of plates as will enable those hitherto unacquainted with the peculiar merits of the early painters of this school, fully to appreciate their excellence.

3. *Public Education.*—A plan has been lately suggested, and will be acted upon at Edinburgh, for instructing by lectures and demonstrations, the operative mechanics of that city, in the principles of those branches of science which are useful in the various trades that are carried on there. Lectures on practical mechanics and practical chemistry will be delivered twice a-week during the winter. A library has been formed, and the institution is conducted by a committee of fourteen, having a clerk and librarian. It is much to be desired that this plan were carried into execution in all the great towns of the United Kingdom, where it is possible: for efficacious as private interest has been found to be in improving the arts, yet there is no doubt that they must advance with the knowledge of the principles on which they are founded.

4. *Statistics of Spain.*—The Spanish monarchy is composed of Spain and her colonies. The kingdom of Spain contains 10,372,000 inhabitants spread over a surface of 24,661 square leagues. The colonies on a surface of 669,094 square leagues reckon 17,700,000 inhabitants distributed as follows;

	Square Leagues.	Inhabitants.
New Spain	118,477	7,550,000
Guatimala	43,089	1,200,000
Cuba and Florida . .	115,039	592,000
Puerto Rico & St. Domingo	2,805	493,000
New Grenada . . .	80,433	1,600,000
Caraccas	64,561	900,000
Peru	60,172	1,500,000
Chili	92,000	900,000
La Plata	144,955	1,100,000
The Philippines . . .	13,888	1,740,000
The Mariannes . . .	1,425	80,000

The revenues of Spain in 1817 amounted to 620,000,000 reals, (about 6,000,000*l.*)

In the kingdom of Denmark there were born in 1820, 32,376 children, among whom were 3,089 natural children. The number of deaths was 23,532. At Copenhagen the births were 2,225, including 852 natural children; the deaths 1,162.

5. *Census in America.*

	1800	1810	1820
Connecticut	251,002	261,942	273,248
New York	586,050	939,049	1,379,989
New Jersey	211,149	245,562	277,575
Pennsylvania	602,363	810,091	1,046,844
Delaware .	64,270	72,674	72,749
Maryland .	340,704	380,346	407,300

6. *Observatory at Abo in Finland.*—The emperor Alexander has erected a magnificent observatory at Abo, in Finland, the direction of which has been given to the celebrated astronomer Balbeck.

7. *Rewards for Discovery in the North.*—In the new Longitude Act, which is the 58th of Geo. III. amended, it is assumed that no ship has gone beyond 81° of N. lat., and 113° of W. long. within the arctic circle. The rewards proposed in it are,

£5,000 to any subject of Great Britain, who shall reach the longitude of 130°, from Greenwich, within the arctic circle.

£10,000, besides the above, for the North-West passage into the Pacific.

£1,000 for reaching 83° of North latitude; and a similar sum for 85°, 87°, and 89°, respectively.

8. *Convent of St. Bernard.*—A subscription has been entered into at Geneva, to be applied to the purpose of repairing and warming the convent on the Grand St. Bernard. It appears, that its inhabitants, who are known over the whole of Europe, as having saved many a wanderer from destruction, amidst the snows and precipices of their inclement mountain, suffer severely in their health, from the damp and cold which prevail in their crazy mansion; they have, therefore, the strongest possible claims upon the charitable traveller; and we trust that the British will not be backward in lending their aid
alleviation of their sufferings. The Helvetic Soci
tributed the sum of 600 francs towards effectin
objects.

Subscriptions are received by Messrs. Decandoll
tini, bankers, at Geneva.

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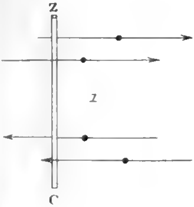

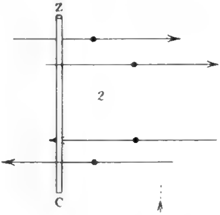

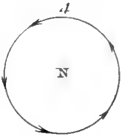

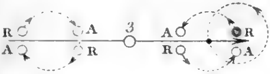

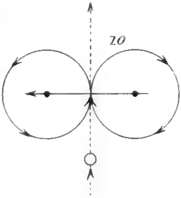

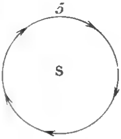

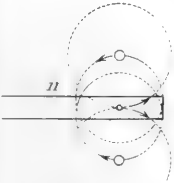

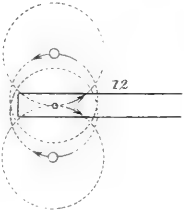

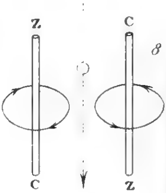

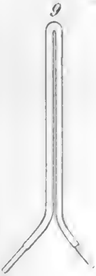

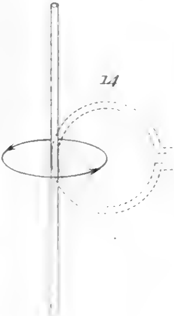

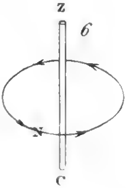

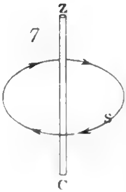

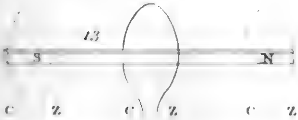

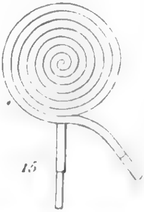

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Fig. 3.

Ancient Sphynx discovered at Colchester

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Fig. 4.

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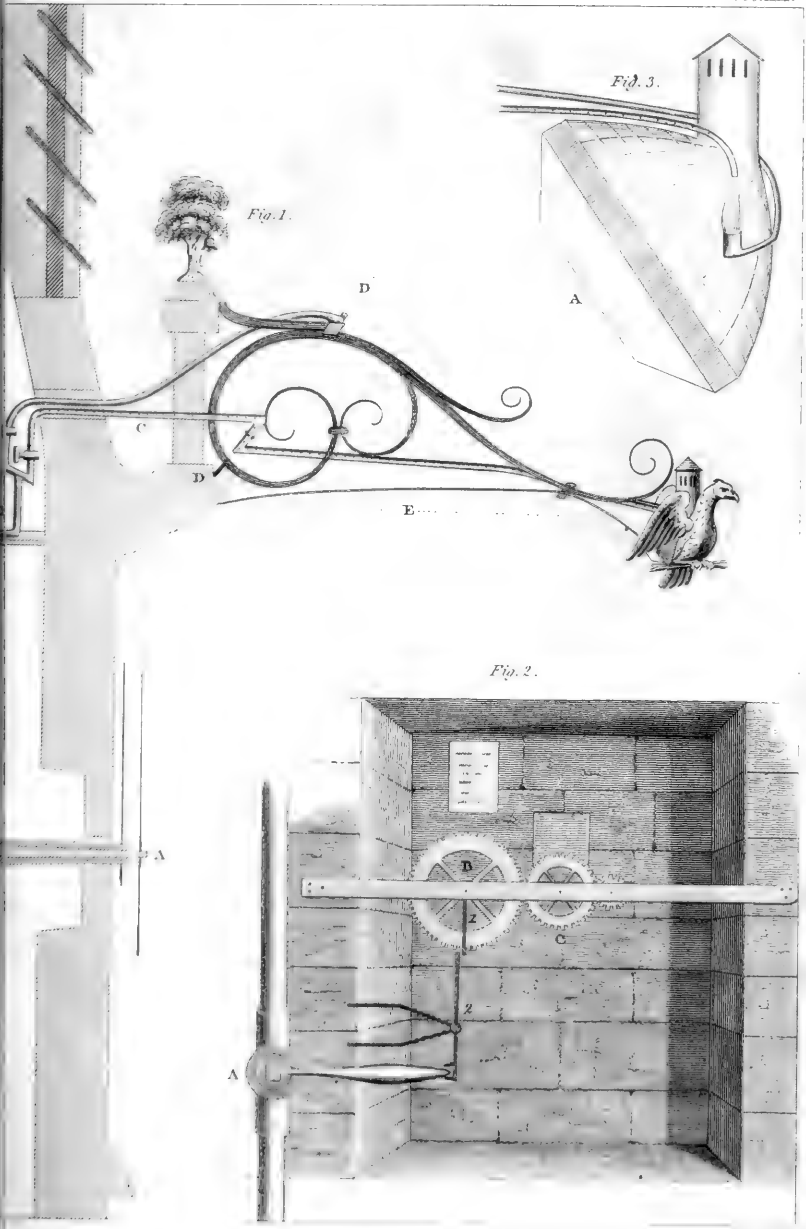

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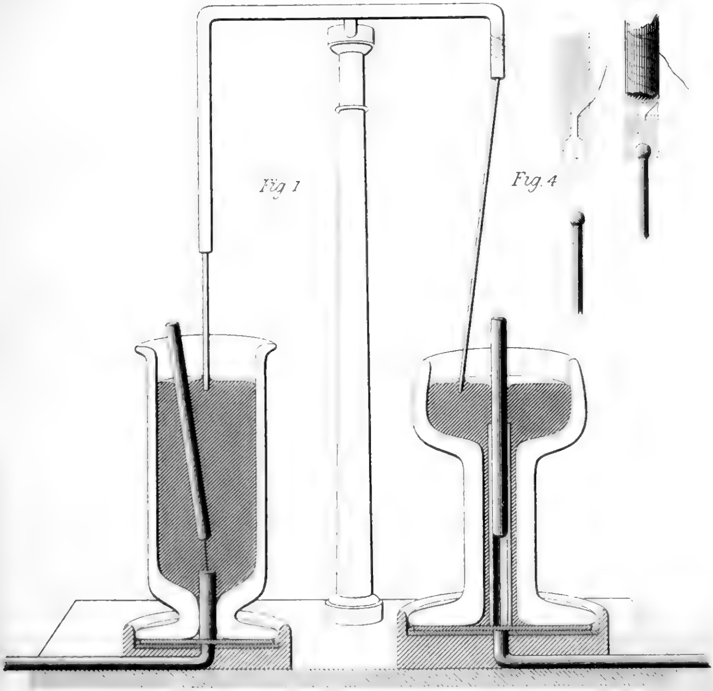
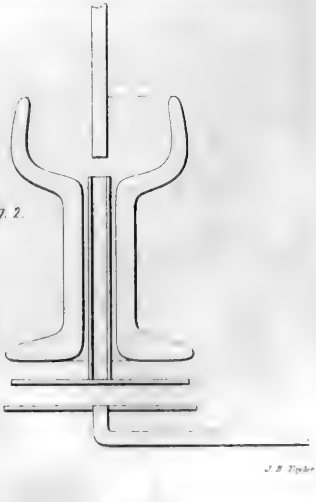


Fig. 3.

Fig 1

Fig. 4

Fig 5



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